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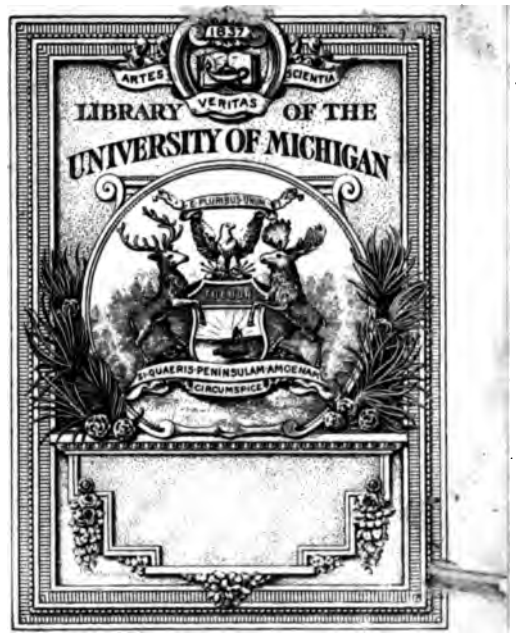
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CHARLESTON

MEDICAL JOURNAL

AND

REVIEW.

EDITOR AND PUBLISHER.

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Homo naturæ minister et interpres, tantum facit et intelligit, quantum de naturæ ordine, re vel mente, observaverit; nec amplius scit aut potest.—Bacon.

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GENERAL ALPHABETICAL INDEX

TO THE

FOURTEENTH VOLUME

OF THE

Charleston Medical Journal and Review.

For original Communications, Reviews and Book Notices, look for the names of the Authors of Articles and Works, as well as for the titles of the subjects.

For Selected, Translated and Editorial Articles, look for the titles of the subjects only.

CHARLESTON

MEDICAL JOURNAL AND REVIEW.

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ART. I.—*Endemic and Indigenous Diseases.* By SAMUEL HENRY DICKSON, M.D., Prof. of Practice of Medicine in the Jefferson College, Philadelphia.

Endemicity, a word first used, I believe, by Boudin, in his condite work on Medical Geography and Statistics, is very convenient for the precise expression of a quality apt to be too loosely spoken of by writers. We shall often find the two ideas confounded, of a *local existence*, for an indefinite time, continuous and uninterrupted, and of a *local production*, a truly *indigenous* origin. In Dunglison's great Dictionary, we have suggested, in the definition of the word "indigenous," the exact purport of the word Endemic—the opposite of Exotic. Diseases may be pointed out, perennial in their presence in certain localities, and impossible of extirpation, which were not originally produced there, are therefore not indigenous, and therefore not strictly endemic. Such, for example, are some of the zymotic affections which make their constant appearance on the bills of mortality of large cities—small-pox, scarlatina, pertussis; let us add syphilis and psora to the repulsive list. Others we know belong strictly to the situations where they occur; but we will not be at any difficulty in finding a class of which their local attachment or generation is well known, concerning which, however, we doubt as to the true source. Thus, Plica Polonica is affirmed by many to depend upon the habits of the people subject to it, and not the contingencies of place. The learned author above quoted, gives ague as the striking and apposite illus-

tration of Endemicity; and in truth, all that we clearly know of it, is its generation in, and adhesion to, certain localities. Pellagra is met with in Lombardy, and in the Landes of Southern France; strangers are not attacked with it, nor does it seem to diffuse itself beyond the poorer classes. It is, in its own country, ascribed by most to diet; by some, to general modes of living; by a few, to a single article of food; and by some scientific and curious inquirers, to a special vitiation of that single article, maize.

Thus we see, that ague and pellagra, when closely considered, are not to be included in the same category, or definition of Endemicity. A stranger—all strangers are liable in certain places to ague. They are liable under all varieties of circumstance, condition, habits, diet, &c. So it seems to be at Aleppo with the bouton d'Aleppe, to which we might add many others spoken of by authors, such as the Tigretier of Abyssinia, the Astaragaza of Ethiopia. Some act slowly, others rapidly. Goitre and Cretinism may be mentioned here as being no less obscure than malarial ague, and connected strongly, but indistinctly, with mere place—Bouton d'Aleppe—universal among natives, but not exclusive; Plica Polonica, exclusive, but not universal; and thus also, of Pellagra, which, so far as appears, attacks natives alone, but not all the natives.

The great characteristic in the natural history of diseases, however, which renders them interesting to the general Pathologist, and to the Therapeutist every where, is their varying capacity of dispersion—diffusion—extensive distribution. Some Endemics are circumscribed within narrow limits; some are more or less capable of being distributed; some seem capable of wide diffusion. Every weekly bill of mortality, wherever published, contains a list of deaths from exotic, or truly foreign maladies. The Botanist and Zoologist meet every day with plants and animals, which do not belong to the positions which they now occupy. Lifting my eyes from the page on which I write, I see before me ten varieties of trees, in full foliage, of which only two are indigenous, or of American origin. The fig, the lagurstrimia, the oleander, the varnish tree, the pomegranate, the melia azedarach, are

all foreign, most of them, like small pox and cholera, of Eastern origin. Our domestic animals are all imported—the horse, cow, ass, sheep, goat and dog. Every disease must be, or must have been, indigenous somewhere—in that sense, endemic then; if capable of migration, it may extend far away from that central location, and, if the conditions have changed which produced it, we may not find it there now. Even ague seems to have ceased in certain localities, and sometimes appears where unknown previously, and unexpected, owing to the generation or concurrence of the essential conditions. Men take with them animals and vegetables in their migrations; parasites of both kinds follow them against their will. The Norway rat invades all lands. The dandelion, and other noxious foreign weeds, spread themselves abroad unaccountably, and cannot be extirpated. Here, in America, the native man is driven away, or disappears and dies out, and many exotic races have taken his place. Our fields are spread wide with foreign wheat, rye, oats and rice—our orchards planted with fruits of distant origin—the apple, peach, pear, and apricot.

What native diseases have we? what endemics, in the strict and precise sense? We brought with us small pox, and the other exanthemata; we can date the first occurrence of typhus and typhoid: the natives knew nothing of cholera, asphyxia or yellow fever.

Of Endemicity, in the proper meaning, we have no examples but in the class of malarial affections, catalogue them as we may. But we may truly say of any disease, that it is naturalized—when, like our foreign human races, like trees and fruits, and seeds, and weeds, and domestic and parasitic animals, it shows such a congeniality with place, soil, climate, or whatever constitutes the essential characteristics of locality, that it takes firm root, becomes a denizen of the spot—does not die out—refuses to be driven away or exterminated. The favoring contingencies which endow disease with this unhappy tenacity, must vary greatly. Sometimes they may be obvious—as where we reason of the influence of density of population, want of cleanliness, of proper and sufficient food, of ventilation, in keeping up typhus and

psora, and syphilis, in the poor of our large cities. Sometimes they are undetected, as when we reason of the presence of typhoid in some of our beautiful Piedmontese situations always subject to it—and of yellow fever in so many of our American towns and cities, where it is either perennial or of regularly annual reproduction, as in Havana and Vera Cruz.

We suppose that we know, historically, when the first yellow fever appeared in Charleston. It is clearly not endemic in the same precise sense in which intermittent and remittent fevers are endemic. It was always treated of by our earlier historians and medical writers—the distinguished Chalmers included—as an exotic, as of foreign origin and introduction. But it may have become here, as we believe it is in many other American localities, naturalized—a denizen of fixed root, ineradicable, and ready to germinate under apposite conditions.

This is a most important question. I, who unequivocally maintain its foreign origin, cannot help feeling a melancholy doubt whether we shall ever be able to exterminate it. I know that scarlatina is of foreign origin; I am hopeless of its extermination. So of small pox, and measles, and hooping cough, &c.; also, less obviously, of cholera asiatica.

I confess myself occasionally surprised to see so much anxiety shown to establish the indigenous character of this terrible pestilence, which does such fatal injury by its occasional spread among us. I am still more surprised at the inference so promptly drawn by many who believe it thus indigenous, of the inutility of any attempts to interfere with its farther and perpetually renewed introduction from its more certain or endemic seats. Let us consider this matter. Here, we have occasional interruptions of its presence or prevalence. We may pass a year, two, three, or four years, without the occurrence of a single case. I suppose that the most ardent controversialist will not deny that the longest of such intervals known, or recorded, have been those in which we have had least intercourse with other places subject to its sway. I believe, though it may not yet be undeniably or certainly proved, that it has never shown itself here, or in our immediate neighborhood, unless when the coincidence

occurred of the presence of some vessel within our harbor, or the existence of some cases, or (as Drake tells us in the Mississippi Valley,) of some fomites from its known sources. Hence arises, in my mind, a hope—not a very sanguine one, it is true—that being on the extreme limit of the yellow fever zone, we may, by avoiding or preventing the re-introduction of it through its various modes of communication by infected atmosphere in the hold of a ship, or entangled in fomites, succeed in getting rid of it altogether and permanently.

I do not intend to enter upon, or discuss the question of its contagiousness in the familiar sense attached to this word. The poison which gives rise to it, is contained, we all know, in the atmosphere; on board a ship it exists often with such virulence, that all are attacked who venture to remain on board even a short time, not having gained immunity by acclimation, or previous seasoning. Nor does any one doubt that the immediate vicinity of such a ship is dangerous in greater or less degree. And farther, as atmospheric air, with all its intermixtures and qualities, and properties, must be entangled in certain porous articles, it is reasonable, on all fair and theoretic grounds, to believe that many such articles may become fomites, and convey the disease, or the cause of the disease; for we do not, and cannot, know how much may be contained of the poison in any given bulk of air, or how much air must be inhaled or applied to the surface to be efficient.

But to return to the general topic of our brief essay. Endemics present each its individual character, so widely separating them in history that they will scarcely bear classification. Diseases that are truly indigenous in any locality, affect natives of that locality chiefly; are, as it were, of elective affinity. Nay, this liability seems sometimes to attach to natives exclusively. Such is the general rule, but there would seem to be at least one remarkable exception. 1. Goitre and Cretinism, Pellagra and Plica Polonica, are endemics which, so far as I am aware, affect only those who are born and continue to reside in the places where they originate. 2. The Bouton d' Aleppe—a cutaneous inflammation peculiar to the region indicated by its name, is universal

in its attack on the natives—none of whom say our authorities, escape. But it will occasionally attack visitors also and travellers, whence we infer that it does not require so long time to fix itself upon the constitution as the former.

3. Malarial endemics fasten specially upon natives, but attack strangers readily. This is true of all the forms of disease ascribed to Malaria. 4. Yellow fever so commonly spoken of, controversially, as endemic and indigenous, is an exception to these general rules above alluded to. Nay—the term exception is too weak to express the value of the fact, if indeed it be properly applicable here. It is far more correct to say that Yellow Fever is in direct contrast to the endemics mentioned, as indeed to all others of which I can find any description. It is so specially apt to attack new comers everywhere, that it is in not a few places called "the stranger's fever." In some localities natives are said to be absolutely exempt; in others, as in Charleston, only comparatively so. The exemption is specially circumscribed—not seeming to be a diffused result of climatic or other general agencies. Thus Semmes says, that the people of the country immediately surrounding Vera Cruz, are more liable to it than strangers from a distance. Our fellow citizens of the parishes surrounding the city we know enjoy not even a comparative exemption. It would appear to be somewhat modified too by contingencies, as thus. The immunity of natives as far as I can learn is complete, wherever Yellow Fever prevails uninterruptedly throughout the whole year—that is unchecked by any winter. Where there is a perfect interval, the disease being unknown in the winter and spring, there is not so complete an exemption of natives. Where, as in Charleston, an interval of a year or more is occasionally enjoyed, natives become subject in greater or less degree — though strangers are notoriously more susceptible of attack. When the disease invades at long intervals, as at Philadelphia, Gibraltar, Cadiz, natives seem to be just as liable as strangers or visitors—at least I am informed of no marked difference.

These diversities in the conduct and history of endemics should lead us to careful and minute inquiry. They proba-

bly depend upon differences in the nature and mode of their causation. But the question of cause is always difficult and obscure, and it may be safely asserted that we do not clearly know the origin, the mode of generation, the causative agency productive of any endemic whatever. Read the discussions and observe how illogically and unreasonably the most irrelevant suggestions are pertinaciously urged upon us; how constantly the non-causa pro-causa is argued and vehemently maintained. It has been rendered highly probable that some of this class of maladies depend upon parasitic life—vegetable and animal. Some may be connected with the peculiar character of the soil, of the waters drank, of the food and the modes of preparing it. But it is hardly too much to affirm that our best knowledge of these matters is, after all, merely conjectural, upon grounds, too, of very equivocal value and relevancy.

Assuming that the facts are as stated, can any one tell us why Trismus is more frequent upon certain islands—the Shetland Isles, those of the Caribbean sea, and our own Long Island, than on any continent? Why Elephantiasis should be met with oftener in Barbadoes than in Ceylon? Hydrocele more frequent in Ceylon than in Barbadoes? Why the Tara should be confined to Siberia, and Beriberi to the East Indies? Why the plague should arise in Egypt and not in Cuba; and the yellow fever in Havana and not in Cairo.

We are more familiar with the Endemic presence of malarial fevers than any other; and we have speculated most abundantly upon their source. We are satisfied that we know the reason why typhus prevails in Dublin and St. Giles and on board an emigrant vessel or a slave ship. Perhaps it is not quite so clear why its congener typhoid or enteric fever attacks some of our most beautiful country villages, and salubrious summer resorts. Every young possessor of a diploma can tell you when he meets with cholera, from what foul drain or noisome sewer its generating effluvium has escaped in the neighborhood. How definitely are we instructed along our Southern coast in the doctrine of causation of the hæmagastric pestilence which casts its dark shadow over

our hopes and prospects, and desolates our streets and houses! Nowhere is the non-causa pro-causa so arrogantly and polemically thrust upon us as in this difficult and interesting investigation. In one locality the dreaded malady was dug up with newly turned earth, where gas-pipes were laid or drains cut. In another, it was the product of a foul fermenting cess-pool; in another, of a slaughter-pen. In the dense lanes of a crowded city it arose from the contingencies to which typhus is ascribed generally; and in the scattered and rural residences of a sandy sea-shore village, from those which give out malaria or produce intermittents, none being then or there present.

To pronounce any malady of specific character to be indigenous, when it only occasionally presents itself in the locality indicated, might be said at the very outset to be illogical. Its relation is not absolutely to place, if it be not always found in that place. To what other contingency then, is it absolutely related? The inquiry has not yet been satisfactorily answered—the causa sine qua non, is yet a matter in dispute. Not heat, nor filth vegetable nor animal, nor moisture, nor dryness; for these have all varied separately and in combination, without affecting the results. It has been seen where the surface was unbroken—absent when it was widely turned up. It has failed to show itself for consecutive years of great variety of condition. It has shown itself in consecutive years of equally marked variety. Last year, cold and dry as was the summer, it assailed a pleasant and neat village in the vicinity of Charleston, nor did the city altogether escape. This season, while hot and dry, it invaded the city first, and has spared its neighbors. They are turning up no soil, laying no gas-pipes, digging no drains. The streets are extensively paved, and the police are not accused of neglecting them or failing to keep them clean. Yet there, we have the same intruder which forty years ago was ascribed to unpaved streets, undrained surfaces, and neglected scavenger's observances. It is plain that we have failed in this direction to indicate the cause—or by efforts suggested by these ancient views to weaken its force or arrest its effects. What may be true elsewhere I know not. Of that locality however,

I think, that we may affirm that on no single occasion within the period indicated has yellow fever existed unprecedented by two contingencies. 1. The previous existence of the pestilence in some city or cities with which it was in commercial intercourse; and 2. The presence of some vessel from that or those places in its port.

I said that I was oppressed by a painful doubt whether my native city, is not "within the yellow fever zone," in a certain sense; that is, whether its climate is not so congenial with the detested cause of that terrible plague as to afford it a fostering nidus, and to preserve it unextinguished from year to year. The winter of Philadelphia is certainly fatal to it. Yellow fever cannot lie over from year to year in that locality. Nor can it in Norfolk, or even in Wilmington. It seems to me altogether futile to contend for its spontaneous production as an endemic in either of those localities, as indigenous or not clearly exotic. But advancing Southward the question assumes a serious aspect. If in Havana and Vera Cruz it be perennial—waking into annual vigour like flower and fruit—where shall we draw the line? The orange and lime live doubtfully on the coast of South-Carolina; why not the Yellow fever germ—or if I must not be allowed the phrase—the poison producing yellow fever, why not also persistent there? I hope it is not. The chief ground of that hope is the uniform influence of all long continued interruptions of commerce upon this point. Our history shows that both our wars and our embargo and non-intercourse act kept us clear of our dreaded guest. And since my attention has been drawn to the subject, I am not aware that any single case has occurred in Charleston when there has not been an infected vessel, either at the time, or recently, previously in its harbor. I am not at all embarrassed with the difficulties of transmission, or the impossibility of tracing the imported cause from point to point, or the rapidity of its efficient diffusion when imported. The ingenuity of the smuggler, the eccentric movements of the wild reveller, the daring of the reluctant prisoner, afford me analogies enough of ready explanation. Add to these the observed rapidity of spread of the most palpable contagions,

and the total impossibility of limiting or tracing their modes of communication, and there is nothing to embarrass the formation of an opinion as to the necessary connection between the ships lying at quarantine in the harbor, and the cases occurring in and all over the city.

No satisfactory solution of this problem will be arrived at until we institute for a few successive years an absolute and entire interruption of intercourse during the summer months with the ever-infected places where the hæmagastric pestilence, though not indigenous, nor originally a native, has yet become a denizen, ineradicable, and always active and self-dispensing. We have here, it seems, the materials always ready for explosion; let us keep out the spark, whether it be brought in the form of a vegetable sporule, an animal ovum, an organic or inorganic substance, a living parasite of either kingdom, a merely poisonous bulk of infected atmosphere, or an atom of contagion. What matters this dispute about words! Let us examine if the approach, the importation of some materies morbi, palpable or impalpable, is an essential point; if the coincidence is uniform; if the visit or presence of a *vessel* from an infected place be necessary.

Next, if having kept away this mode of infection, we can trace to *persons* or *fomites* the introduction of the disease,—this also seems not unfairly deduced from known facts—and lastly, if we can ascertain that around the body of the sick there is diffused an efficient poison capable of generating the disease in a healthy body. We shall then have taken it out of the category of endemics, as having no exclusive relation to place any more than small pox or measles, which are now spoken of no longer as indigenous anywhere, or endemic in or of any special locality.

ART. II.—*Exopthalmia as a Symptom of Disease.* By C. E. FLEMING, M.D., Columbia, S. C.

My attention having been lately called to cases of Exopthalmia occurring in anæmic patients, and having performed several experiments with a view of ascertaining the cause of this symptom, it has seemed to me that the subject deserves more attention than it has generally received from the profession.

Exopthalmia as its name implies (ex out and opthalmos the eye) signifies an abnormal protrusion of the eye-ball. The affection has also been termed Proptosis. French authors make four varieties in accordance with the exciting cause. Thus we have,

1. Exopthalmia as caused by wounds.
2. Exopthalmia as caused by new formations, as tumor, &c., within the orbit.
3. Exopthalmia as caused by relaxation of the muscles of the eye.
4. Exopthalmia as due to Anæmia.

It is not my intention in this paper to discuss the subject of Exopthalmia under these several heads. My purpose is merely to speak of a morbid condition of the system, in the latter stage of which Exopthalmia occurs. Six cases which have been brought under my own immediate notice, presented quite uniformly the following phenomena. The disease commenced with anæmia, which more or less rapidly attained a marked degree ; after a time an affection of the heart made its appearance, commencing as functional disturbance, but finally resulting in organic change, especially in dilatation. Consecutive to the heart symptoms, the thyroid gland became involved, being as a general rule considerably, though sometimes only slightly, enlarged. Lastly, the eyes began to protrude, and well marked Exopthalmia was ultimately developed.

In every one of the cases to which I refer, the patient had suffered at a previous period from rheumatism, in some of its forms.

It is my purpose to comment briefly on the several circumstances above mentioned in the order of their occurrence.

THE ANÆMIA when fully formed, is generally quite characteristic; the patient is pale, nervous, and excitable. The actions of the heart are increased some forty or fifty beats per minute. The carotid arteries throb violently. I have seen their pulsations distinctly at a distance of some fifteen feet, and during any temporary agitation the frequency of the heart and the violence of the arterial pulsations are yet further augmented. The respiratory functions also, are more or less disordered, dyspnœa being frequently present, especially when the patient assumes the recumbent posture. There is headache, humming in the ears, dizziness when in the upright position, hysteria and other evidence of nervous disorder.

In females the uterine functions are generally more or less disturbed; and uterine disorder has hence been regarded as the *cause* of the affection by certain writers. I am disposed however, to regard these gentlemen as in error; for, as a general rule, the uterine disturbance is not *amenorrhœa*, which indeed would produce anœmia, but *dysmenorrhœa* which is more frequently a *result* of anæmia. The assertion moreover, at once falls to the ground, when it is found that males are quite as frequently affected with the disease which is the subject of this paper, as females. The anæmia established, continues during the appearance of the other symptoms, and terminates either in recovery or in death. In the few post-mortem examinations of fatal cases which have been recorded by authors, little has been found to throw light upon the disease. The most marked phenomena observed were distension of the veins, especially of the jugulars, with serous effusions, and pathological conditions of the heart and thyroid gland which will be fully described by and by.

Anæmia is undoubtedly the "fons et origo" of the various phenomena yet to be described, and the question therefore at once arises, what is the cause of the anæmia? This question is involved in great obscurity. Undoubtedly the anæmia is not the whole disease, for while the various phenomena I have to mention, and especially the exopthalmia are

comparatively rare, anæmia is frequent. The fact already mentioned that in all the cases which have come under my own personal observation *rheumatism* had existed prior to the occurrence of the anæmia, seems to me to be one of considerable importance in this connection. It is well known that anæmia is a frequent result of rheumatic affections. In females also menstrual derangement (already mentioned as having been regarded by some as the cause of the disease under discussion,) occurs among the rheumatic sequelæ. I am therefore inclined to the opinion that rheumatism is at the root of the condition of system described; an opinion derived at once from the connection between rheumatism and the most prominent symptoms of the disease, and from the history of the cases to which I have alluded. Nevertheless, though strongly inclined to this *opinion*, the limited number of cases upon which it is based do not permit me to state it as a fact.

THE BELLOWS SOUND OF THE HEART invariably makes its appearance during the progress of the anæmia. At first the heart is only functionally diseased, but organic changes undoubtedly occur secondarily, in many cases. Percussion rarely shows that its bulk is increased. This enlargement is due to dilatation (as is shown by the sharp sound readily observed on auscultation,) and not to hypertrophy. It is only another result of that loss of tone, and general relaxation which prevails throughout the system in consequence of the impoverished state of the blood. The dilatation, the watery state of the blood, and the nervous disturbance, sufficiently account for the bellows sound.

Next to be considered is the ENLARGEMENT OF THE THYROID GLAND. The degree of this enlargement is very variable. Sometimes it is so great that dyspnœa is produced by pressure on the trachea; sometimes it is scarcely perceptible. The external surface of the tumor thus formed is smooth and regular, and some of the enlarged branches of the thyroid artery can be felt pulsating near the surface.

The exact pathological condition of the gland under these circumstances has not been thoroughly investigated. Dr. Banks, in a report of a case examined by himself, says, "a

section of different parts of the thyroid gland disclosed the existence of numerous cysts containing a yellow fluid like honey. The contents of some of the cysts were dark colored and resembled coagulated blood." But Dr. Banks does not inform us of the chemical composition of this fluid; probably an analysis was not made.

It is however generally believed that the enlargement of the gland is a true hypertrophy. A hypertrophy due on the one hand to an increase in the *size* of the *gland vesicles* of the thyroid which in some cases even, as in that reported by Dr. Banks, may attain cyst like dimensions, on the other hand due to an increase in the *number* of the gland vesicles.

The phenomena of the disease at once reveal that there is a close connection between the enlargement of the thyroid and the cardiac affection. This is especially shown by the fact that its size is much increased when the heart is excited to increased action, and again diminishes with the diminished action of the heart. The exact nature of this connection is not known. Other glands are quite as closely, or more closely connected with the heart, both as to vessels and nerves, yet are not similarly affected. But parts which are distant and have no direct connection, often sympathise strongly with each other, and it sometimes happens that the organ secondarily involved becomes most prominently diseased. A relation also exists between the thyroid enlargement and anæmia. This is at once shown not only by the co-existence of the two conditions in the disease we describe, but by the fact that where *goitre* prevails endemically, the patients generally present an anæmic appearance. The relation thus shown to exist suggests a rational mode of accounting for the enlargement. It may readily be supposed that the peculiar change in the constituents of the blood, determines a deviation from the normal nutrition of the gland, which is so modified that the gland vesicles increase both in size and number. Nor are we without parallel cases. In the anæmia of intermittent fever, the spleen, another of the ductless glands, is as constantly enlarged as is the thyroid in the cases under consideration. Not only is the nutrition of the gland modified by the modified *state* of the blood, but the *supply* is affect-

ed *quantitatively.* And herein lies the source of yet further modifications.

Dr. Marshall Hall, in his work on convulsive and paroxysmal diseases says that, " in convulsive and paroxysmal diseases of females there is a tendency to spasm of the muscles of the neck, and the seizures are the direct result of the impeded return of blood, from the deep seated veins being compressed by the irregular muscular action." Now the female subjects of the disease we are studying are generally hysterical, and there may perhaps exist the same impediment to the return of blood from the head to a less amount, but more continuously. But be this as it may, the fact of *great venous* congestion of the jugular veins and their branches is indisputable, and has been established by autopsies, as has already been shown. The glands are therefore constantly in a state of hyperæmia; and hyperæmia is well known to be a frequent cause of hypertrophy.

In addition to the modifying influence exerted upon the *nutrition* of the gland by an increased supply of an abnormal circulating fluid, the same causes exercise also a modifying effect upon its *secretion.* Any reasoning upon the exact nature of this derangement will, of course, partake of a speculative character, while we are so completely ignorant of the normal character of the secretion of this organ. But if, as we may well suppose, the thyroid exercises an office similar to that of the spleen, which Grey has shown in an elaborate dissertation, to modify the blood both as to quantity and quality, it will, at once, be understood that the thyroid does secrete, and that it is only in the mode in which it escapes when fully formed that its secretion differs in its modus operandi from that of the glands with ducts. For while in the ordinary glands the secretions are conveyed away by one or more ducts to the point for which they are destined, here as in other ductless glands, the products pass by imbibition into the blood-vessels, and are thus carried into the general circulation.

That the silent process here indicated is deranged in the disease in question, there can be no doubt. Two elements exist in every gland, the *vascular* and the *secreting*, and where

the first is disordered, disorder of the second invariably follows in its train. We have already shown the vascular disorder of the thyroid; the disordered secretion therefore may be inferred as a matter of course. The exact nature of this disordered secretion and the relation between it and the enlargement of the gland are purely hypothetical.

Still later, in the order of sequence, than the enlargement of the thyroid gland EXOPTHALMIA makes its appearance. The protrusion of the eyes varies in extent in different individuals, being in some very marked, while in others it is scarcely perceptible. But this diversity exists in health as well as in disease. A distinguished author says, "In the performance of every function and in the condition of every organ, there is considerable diversity within the limits of health, and a state of things which, if continued, would constitute disease, may be so fugitive as not to merit the name; so that both in point of degree and duration, it is impossible to say of any particular variation from the ordinary condition of the system, whether it is healthy or morbid." The condition of the eye, we know, is different in different individuals. We see many persons with the eyes set back in the orbits, constituting what we vulgarly term "sunken eye;" the eye and its appendages may be perfectly healthy, but the muscles are shorter than usual. And in "pop-eye," the reverse of the latter, the muscles are longer than usual. The immediate cause of the protrusion in this disease, like that of the enlarged thyroid gland still remains involved in obscurity. As many theories have been proposed as there have been writers upon the subject. Some have affirmed that the eye itself enlarges; by others this has been disputed. The opinion of the latter class I am disposed to adopt. Those who entertain the opinion that the eyeball does enlarge, ascribe the enlargement to a want of tonicity of the fibrous coat, especially of the sclerotic. Without doubt this supposed enlargement exists only in the imagination of imperfect observers. I would rather attribute the protrusion to the muscles which hold the eye in position. These, from the impoverished state of the blood, lose that tonicity which characterizes them in their healthy state, and become weak and

unable to perform the office assigned them by nature; for it should be borne in mind, that the muscles of the eye, and especially the recti, do not by their simultaneous contraction diminish the transverse and lengthen the antero-posterior diameter of the eye, as has been asserted by some writers; but that on the contrary, when they contract together, they draw the globe back upon the cushion of fat in the posterior part of the orbit, and thus in fact *diminish* the antero-posterior diameter. An abnormal relaxation or lengthening of these muscles therefore will be understood to be capable of producing exopthalmia. In illustration of the latter assertion, I have two cases in my mind at this moment; one a lady, whom I have repeatedly seen, had the internal rectus divided for strabismus; the eye then turned outwards by the action of the external rectus, and was protruded quite perceptibly. The other case was that of a young man, who first had the internal rectus divided for strabismus. The eye also turned outwards, and was slightly protruded. To overcome this deformity he had the external rectus also divided when a very considerable protrusion of the eye occurred. Now if a division and consequent loss of power of *one* or *two* of these muscles permits protrusion, how much more probability is there that the eyes will fall forward, when *all* the muscles have lost tone and vigor, and become relaxed, flaccid and thus elongated. During July, 1857, I made several experiments, with a view of illustrating this subject, which though not new, having been previously performed by others, are so conclusive that I give a brief summary of them in this place.

Experiment 1. The conjunctiva of the eye of a dog was incised, the external rectus hooked up and divided as in the usual operation for strabismus. A similar operation was performed upon the internal rectus of the other eye. The eye, in the first instance, turned inwards; in the second, outwards with a scarcely perceptible protuberance, and they remained in that condition.

Experiments 3 and 4, were repetitions of the first, with like results.

Experiments 5. On still another animal, all the muscles of

one eye, except the external rectus were divided, when the eye almost immediately shot forwards to a considerable degree, being also rolled outwards by the undivided muscle.

In opposition to the doctrine indicated by these cases and experiments, there appears in the Medico Chirurgical Review, for March, 1858, an article on strabismus, by Dr. Hewson, of Philadelphia, in which a theory is presented that seems to me quite untenable, and yet is supported by plausible arguments and seemingly corroborative facts. He asserts that the recti muscles have no influence in drawing back the eyeball, and maintaining it in its proper position, but that this power is vested solely and exclusively in the eyelids, or rather in the orbicularis muscle. Now if this is not the office of the recti muscles, what *is* their office? To turn the eye in different directions in the exercise of vision? Perhaps so. But if one single and individual muscle acting alone has that power which is requisite to turn the eye to a considerable extent in any one direction, is there not the very strongest possible grounds for believing that when they all act together, they hold the eye back in the socket. I cannot believe that the recti muscles act merely in rotating the eye; and that it is the office of the orbicularis muscle to hold the eye in situ. *If the lids are held off, the upper one drawn upward, and the lower downward, we see no protrusion.* If a division of the fascia of the eye causes protrusion when *one* of the recti is divided, why is it that there is so much more protrusion when *two* are severed, since but very little more, if any, of the fascia is divided? It depends, I think, more upon the fact that the support of the eye is destroyed in the division of the muscles and not in division of the fascia.

The sinking of the lachrymal caruncle may be explained, without resorting to the supposition that the ocular sheath is injured. If, on the division of the muscles, the eye protrudes, as the ball projects, the caruncle will *appear* to have receded, though it is really in the same spot, but its relation to the globe of the eye has been changed, which is due to a change in the situation of the globe.

The cases and experiments above detailed come properly

under the head of traumatic exopthalmia, but they serve to aid in establishing the point that atony or loss of power of the muscles of the eye is capable of causing a protrusion of the globe.

It will be conceded, generally, I think, that the eyes *will* protrude when this condition of the muscles exists. But does it exist, and is it the cause of protrusion in the cases of exopthalmia under consideration. We unhesitatingly answer both questions in the affirmative. An argument of the following nature might be urged against this theory; that if the protrusion were due to atony or weakness of these muscles, then why does not the eye always return, when strength and tone have been restored to them? A refutation of this argument can, we think, be easily given. The tissues round about the eye have become adapted to the abnornal position of this organ, and though tone may be restored to the muscles, the enlargement of the gland somewhat reduced, and the blood enriched, the eye cannot return. And besides the muscles may have suffered organic change, being lengthened to such an extent that the return and maintenance of the eye in position, may be impossible. But the fact that strength and tone have been restored to them does not necessarily imply that they must retract and shorten, for do we not see similar permanently elongated muscles in old dislocations performing their duties perfectly? The muscles of the eye are as much subject to this condition as any others. In the commencement of this affection, as the eye is protruded, the space left empty by the eye going forward, is filled up by enlarged vessels, increase of adipose tissue, &c., &c. These though not in themselves sufficient to cause the protrusion, may still exist and prove an obstacle to the return of the eye. Such being the condition of the parts, it would require a considerable degree of retractive force to draw the eye back again to its normal position, more than its muscles possess in their most healthy and vigorous state. Sometimes also effusion has occurred into the space left by the protrusion of the eyes; in some few cases where the eye returns, this effusion may have been thin and watery, and have been absorbed. In others, the fluid portion may have been absorbed, leaving

a more or less consistent residue, which might prevent the return of the displaced organ. Much of the difference of opinion which exists as to the return of the eye may depend on the time this state has existed. And here let me remark that the occasional return of the eye, by no means invalidates what I have previously stated in regard to the cause of the eye not returning. For those cases where return takes place, are of very recent date, and have had time neither for the tissues to adapt themselves to the eye, nor for the muscles to undergo organic change. But to return, though they may in some degree contribute to it, I cannot think these deposits and the distension of the vessels can in themselves *cause* a protrusion of the eyes, for the situation of these organs in their long cavity is such that this pressure from behind could not exist without lateral pressure also, and lateral pressure would act upon the optic nerve, and impair vision. For the optic nerve, like all other nerves of special sense, never gives pain when irritated, but presents only a functional disturbance; thus an irritation of the auditory nerve causes a peculiar buzzing noise without pain; and irritation of the optic nerve by pressure or otherwise, produces always visual derangement. But visual derangement, as is well known, does not exist in the affection under consideration. The optic nerve, it is asserted, can be stretched forward without injury to sight; but it can not, with the same impunity, endure lateral pressure. Why this is so, we are unable to say. May we not then fairly conclude that these deposits are not the causes of the protrusion? An argument of the following nature has been urged by another class of writers, who consider distension of the vessels as the cause of the protrusion. They say the eyes in some patients are easily replaced by gentle pressure, for a while, from in front; this pressure disgorges the vessels in the posterior part of the orbit, and thus removes the impediment to its return. But the fact that the eye may sometimes return, has been before explained by our own theory; that if the muscles are relaxed, without any consistent deposit behind, the eye may, by slight pressure backwards, be returned to its normal position; and what can these authors say of those cases where the eye cannot be

made to return? If engorgement of the vessels were the sole cause, and that could be removed in one case by pressure from in front, what reason can they assign that the eye refuses to return in others? Besides would we not have some evidence of congestion in the vessels of the conjunctiva and sclerotica, if this were the cause? But no such condition has ever been found to have existed. Moreover, if distension of the vessels existed to any extent, the eyelids would be puffed out, as the congestion would be between the cellular sheath of the eye and orbit.

In his mention of this affection (which by the way does not occupy much space,) Walton denies that protrusion can be caused by loss of tone of the muscles of the eye, as there exists no such loss of tone in the other muscles of the body; and he also says the freedom of motion which the eye enjoys opposes the theory. We think, Mr. Walton, as high authority as he is acknowledged to be, is like all other men, liable to fall into error, and to be sometimes mistaken. And we have an instance of it in this opinion of his. As regards his first remark, I have only to say there are many things in Pathology, strange and inexplicable. Why these particular muscles should so lose their strength and tone in preference to all the others, we are no better able to explain than we are to give the reason why the testicles sometimes become swollen in parotitis; than why the mammæ become enlarged in pregnancy; and many other similar facts. But because we do not see the *cause* of these things, we are surely not to deny their existence. And as regards his second remark, viz.: the movements of the eye, I will merely say that I contend that the muscles of the eye are weakened and have lost their tone, not that they are paralyzed. For though *paralysis* might impair their mobility, *atony* would not. Again Walton remarks that in the most debilitating diseases with perfect muscular prostration, the eyeballs do not protrude. But is it not a fact now well known in medicine, that besides predisposing causes, the variety of constitutions of different individuals so influence disease, that few, very few are affected in exactly the same manner, even by the same disease, and under apparently similar circumstances. Walton as-

cribes the protrusion to effusion into the ocular sheath ; but Mackenzie has disproved his assertion, for says he, " the freedom of motion which the eye enjoys would preclude the possibility of its being the case, besides no reduction has followed an exploratory puncture." Mackenzie, though he has proved the theory of Walton to be false, has himself fallen into error, and adopted a theory in all respects equally as objectionable as that of Walton. He attributes the protrusion to a varicosed state of the opthalmic veins ; a theory which I have already sufficiently refuted.

We have thus detailed the principal symptoms of the morbid condition under consideration, in the order in which they most frequently occur. Cases indeed have been recorded, in which all the symptoms appear to have occurred simultaneously, but these must be regarded as exceptions to the general rule. In the great majority of cases, the sequence is as has been given. In those cases which have been reported as terminating in recovery, the enlargement of the thyroid gland frequently remains to a greater or less extent, and as has already been indicated the exopthalmia also is frequently persistent after the producing causes have been removed.

With regard to the TREATMENT of this disease, it will at once be understood that remedies must be chiefly addressed to the *anæmic condition* of the system. The indication is evidently for tonics, and for chalybeates to improve the character of the blood, and the strength of the system. Reliance may be placed on iron in all its forms, but more especially on the pill of the carbonate of iron. It would be useless to enumerate the list of tonics from which benefit may be derived in this disease. Which ever is selected, however, must be given regularly and continuously, if any good is to be obtained. The hygienic measures proper for anæmia, must also be resorted to ; exercise carefully adapted to the strength of the patient should be taken in the open air. The diet should be *good* and *nutritious*, and fresh beef is especially useful. If the cardiac affection has appeared, it also becomes a subject for treatment. Digitalis may be given to diminish the force and frequency of the heart's action. A belladonna

plaster over the region of the heart, has been recommended for the same purpose, and has been employed with seeming advantage. In mild cases it may be used alone, and in the more severe it may be employed, alternating with the digitalis, or in conjunction with it.

Complications arising during the progress of the disease, are to be treated upon general principles. Rheumatic symptoms, which are frequently present in all classes of patients, and menstrual irregularities, which are seldom absent in the case of females, are to be treated by those remedies which are directed for each respectively in the books on practice of medicine.

No application is necessary to the eyes or to the enlarged gland, though to satisfy the whims of the patients, some of whom think it absolutely necessary that they should use something, we may employ some simple collyrium, or merely cold water application to the eyes. Tincture of iodine may be painted with a camel's hair pencil, over the surface of the enlarged gland. But as yet we have discovered no specific for either condition.

ART. III.—*Extracts from* M. CLERC'S *Lectures on Venereal Diseases.* By our Paris Correspondent, J. K. KANE, M.D., of Philadelphia. No. 2.

In my last lecture, gentlemen, I stated my opinion that each of the two varieties of chancre transmits itself as a distinct species, and I gave you some of the facts which have led me to this belief. With regard to the infectant chancre, M. le docteur Bassereau, has come to the same conclusion; but not having traced the simple chancre to its source, he has been unable to reconcile the absence of consecutive constitutional symptoms with a venereal origin. He therefore concludes that the non-infectant chancre is an entirely distinct disease, resembling the infectant chancre only in being contagious. He thinks that the simple chancre is identical with

the contagious ulcer of the genital organs described by Celsus and Galen, while the indurated variety is, like syphilis itself, of modern origin. I differ from him. I think that there can be no doubt of the fact that the simple chancre is syphilitic, and that it is the result of the inoculation of the virus of an infectant chancre on a subject who has, or who has had constitutional syphilis. In other words, that it is the analogue of the varioloid and false vaccine. My observations with regard to its transmission have been as full and as satisfactory as those with regard to that of the infectant chancre. In every case in which I have been able to compare the infected with the infecting individual, I have found it transmitting itself as simple chancre.

Perhaps it would not be amiss for me to give you briefly a few of my observations of cases of non-infectant chancre.

1st. One morning in 1845, a friend of mine, himself an old interne, being present at the visit of one of the wards at St. Lazare, thought he recognized in an ulceration of the groin, succeeding a suppurating bubo, some characters not belonging to chancre, and to prove the strength of his convictions on the subject, inoculated himself between the first and second metacarpals of the left hand, with pus taken from the sore. This inoculation with the pus of a simple chancre, for such the ulcer was, give rise to a simple chancre on the part of our hardy compeer.

2nd. In the course of the year 1852, a medical student came to consult me at the dispensary, for a preputial chancre of eight days duration. This young man, who had had no previous syphilitic affection, informed me that the chancre had been communicated to him by a woman in the Latin Quarter, whom he himself had treated the year before for constitutional syphilis. The chancre of this woman could not, for reasons which I will explain to you another time, be other than simple. I therefore told my patient that his chancre was simple and non-infectant, and would continue to be so to the end; and advised him to treat it merely as a local sore. He did so, his chancre did not indurate, and up to the time when I lost sight of him—about two years afterwards—

not the slightest symptoms of constitutional syphilis had appeared.

3rd. In February, 1854, a young girl presented herself at the clinic, having a non-indured chancre of the fourchette, which had given rise to a phlegmonous bubo. This chancre had been communicated by a soldier who was himself affected with several non-indurated chancres.

4th. In the beginning of July, 1854, a young man of twenty-three years of age, a hair-dresser by profession, had on the mucous membrane of his penis six simple non-indurated chancres. During the course of his treatment, which was purely local, a phlegmonous bubo developed itself in the right groin and suppurated. No symptom of constitutional infection has ever manifested itself in this patient. Having learnt from him that the woman from whom he had contracted his chancres was at the Lourcine, M. Vivien, one of the externes of that hospital, who was following my clinic at the time, examined her and found several non-indurated chancres.

5th. A short time ago a journeyman printer, aged twenty-one years, presented himself at the dispensary, to be treated for a number of non-indurated chancres of the prepuce, and also for plaques muqueuses of the fauces and anus, consecutive to a solitary infectant chancre contracted six months before, of which the cicatrix was still apparent. While under treatment for his simple chancres, this patient had sexual connection with a young girl, to whom he communicated his chancres. She in turn cohabited with a friend of my patient. This last partaker of her too generously granted favors shared the common lot, and was brought by his friend to the clinic with seven simple chancres of the prepuce and gland. This individual, who was also a journeyman printer, and a very intelligent fellow, hearing me speak, in connection with his ulcerations, of the transmission of chancres according to their variety, informed me that netwithstanding his state of health, he had had connection with a young girl, and that the results of this imprudence had necessitated her entrance into the Lourcine. I announced to the class, that in all probability we should find on this woman, chancres of the same variety as those on the patient before us. M. Cullerier, into whose

service she had gone, was kind enough to examine her in our presence, and discovered five simple chancres forming a cross at the entrance of the vagina.

The simple chancre then transmits itself as simple chancre. It propagates itself from one patient to another, preserving its peculiar characteristics, of which the most important is, that it always rests a purely local affection, never giving rise to constitutional accidents.

It does not enter into the plan of this part of our course to deduce the consequences as regards diagnosis, prognosis, and treatment of the existence of these two varieties of primitive syphilitic ulcer, but I cannot help alluding to the importance of great caution in making inoculations of chancrous virus from one patient to another, lest we should convey the pus of an indurated chancre to a patient having a simple one, and so give him a constitutional syphilis which he would otherwise have escaped.

The existence of two varieties of syphilitic chancre and their transmission as pathological species being established, it remains for me to demonstrate my third proposition, viz.: That the existence of the simple chancre is due to a re-inoculation by the syphilitic virus of subjects who have or have had constitutional syphilis; in the same manner as varioloid results from the repeated infection of a subject who has already had the small-pox.

For the development of this proposition it will be necessary for us to consider briefly some facts in relation to other virulent diseases, particularly small-pox and vaccine.

Happily for the human race, an individual who has once been smitten by a contagious disease, enjoys as a general rule, an immunity from a second infection of the same disorder. It is rarely that we see small-pox or vaccine, measles or scarlatina, twice on the same patient. So likewise constitutional syphilis does not repeat itself on the individual. Unfortunately this immunity of patients who have once been affected by small-pox, vaccine or constitutional syphilis, from future infection, is not complete, since their system may still be the subject of a modified morbid action as the result of after inoculation. The old small-pox patient may still have

varioloid on being inoculated with variolous virus; and nothing is more common than the false vaccine pustule as the result of vaccination. Varioloid shows itself not only in those subjects who, having been vaccinated, or having already had the small-pox, are again exposed to infection, but also in individuals whose systems are refractory to small-pox and the vaccine virus, and who submit themselves to inoculation with variolous pus.

It is important to note here, that such inoculations of variolous pus on refractory or previously affected systems, do not always produce varioloid; very often they are absolutely negative. The varioloid constitutes a true pathological species. Its march, duration and termination are entirely its own, and it is equally capable of being transmitted by inoculation to subjects who have had the small-pox, and those who have not had it. In 1831, Doctor Guillou of St. Pol de Leon, published a memoir* in which he stated that he had inoculated the varioloid on to more than six hundred infants and adults, who had never been vaccinated, nor had the small-pox, and that he produced varioloid in every case. So large a number of inoculations as this, establishes incontestably that the varioloid is contagious, and that it transmits itself in its own species. It must be admitted that other experimentors† say that they have produced small-pox by inoculating unpro tected persons with varioloid, and that these inoculations, made under the same conditions as those of Dr. Guillou, tend to establish that the varioloid, inoculated on a person who has never been vaccinated or had the small-pox, may sometimes give rise to small-pox.

The numerous re-vaccinations which have been made during the last few years, have shown that among the subjects submitted to this repetition of vaccinal inoculation, there are some who reproduce the true vaccine just as though they were vaccinated for the first time, others on whom the virus appears to have absolutely no effect, and others again who de-

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* Transactions medicales, &c. Cahiers de Dec. 1830, and Feb. 1831.

† Lafont, Gouzi, Trousseau, &c.

velop a special pustule, which is not the true vaccine, but a sort of hybrid, and has been called the false vaccine or vaccinoid. *"A degenerate product of the true, the false vaccine," says M. Bousquet, "has neither the same march, form, nor duration. * * * * "Whereas the good vaccine barely shows itself at the end of the third day; the bad much more precocious, is observable from the first, or at most the second day of its insertion."

† The absence of a period of incubation, has been given also by M. Gaultier de Claubray, as one of the constant characters of the false vaccine. ‡Husson, gave moreover the absence of induration at the base of the pustule as a characteristic sign of the false vaccine. "One cannot" says he, in his remarkable article on vaccine in the Dictionnaire des Sciences Medicales, "one cannot call this button (that of the false vaccine) a tumor, for it wants the circumscribed induration that makes the base of the vaccine tumor."

Remark here, that the absence of the period of incubation, and the indurated base, are qualities which belong to the simple chancre, and which serve to distinguish it from the infectant chancre, which on the contrary incubates and indurates like the true vaccine.

When inoculated on a non-vaccinated subject the false vaccine seems like the varioloid, sometimes to transmit itself with all its characters, as a pathological species; at others to produce the true vaccine. "Do not imagine however," says M. Bousquet in the work just cited, "that the false vaccine necessarily produces the true," &c., &c.

Admitting then that the vaccine and variola can modify the virus peculiar to them so as to produce two new affections; the analogies which exist between these diseases and syphilis, naturally lead us to ask whether subjects who have been once affected by that disease, and who expose them-

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* Bousquet Nouveau Traite de la vaccine et des éruptions, varioleuses P. 180, 1848.

† Gaultier de Claubray, de l'altération du virus vaccin et de l'opportunite des révaccinations, 1838.

‡ Dictionnaire des Sciences Med. Tome LVI., p. 419.

selves a second time to the contagion of chancrous virus, may not create, so to speak, a hybrid analogous to the false vaccine. My observations have led me to believe that this is really the case, and I shall endeavor to demonstrate that the non-infectant chancre is the precise analogue of the varioloid and false vaccine.

Individuals who have had constitutional syphilis, can still contract chancres, and it is not rare to see examples of this. Now it has been observed that such chancres are always simple, non-indurated and non-infectant, obeying the law as it has been called of *unicity* of *infection.* M. Ricord, says truly, that we do not see an indurated chancre twice on the same individual. So the patient who has once had general syphilis, can have for the future only simple chancres, though he be exposed to the contagion of both varieties of primitive syphilitic ulcer. This can only be explained in two ways; either the patient loses altogether the power of being affected by exposure to the infectant chancre, or else his constitution having been modified by the previous syphilis, transforms the indurated chancre into a simple one.

If this latter be the case, a patient having constitutional syphilis, and exposing himself to the contagion of an indurated chancre, may be expected, if it take effect on him at all, to reproduce a simple chancre. I have never had an opportunity of observing a patient who having the syphilitic taint, has had connection with a woman having an indurated chancre, but it is not rare to see patients, who having an infectant chancre, inoculate themselves from it. Such patients are evidently in the same conditions as though they were inoculated with the pus from a chancre belonging to another individual. Now in these accidental inoculations, the result is always a simple non-indurated chancre. I will cite you an instance of this.

1st. In the month of June, 1854, a medical student brought a patient to the dispensary, who had an indurated chancre of the scrotum, which was followed shortly afterwards by a very confluent papulous eruption. During the existence of this chancre, the proper precautions for isolating the sore from the surrounding parts, not having been taken, the skin

of the penis was inoculated, on the portion which corresponded to the chancre on the scrotum. The result was an ulceration as large as a dime, the cicatrisation of which was not obtained until after three weeks of treatment; but neither at any portion of its career nor after it was healed, did it present the pathognomonic induration of infectant chancre, which was so well marked in the chancre of the scrotum. It is then extremely probable that the organism of this patient, modified by the infectant chancre of the scrotum, produced in this accidental inoculation of the penis, a simple chancre, just as the modified organism of vaccinated or variolated persons produces varioloid, when we submit it anew to the influence of variolous virus. I once inoculated two patients affected with tertiary syphilis (a tuberculous eruption and exostosis in each case,) with pus of the indurated chancre taken from two different patients. I cannot say whether it was owing to the existence of an analogous condition to that which produces the negative results in cases of re-vaccination, or because the indurated chancres from which I took the pus had lost the power to inoculate, but it certainly took no effect in either case.

ART. IV.—*A Lecture on Suspended Fœtal Animation; one of a course delivered at the University Medical College, New-York City.* By T. GAILLARD THOMAS, M.D., Lecturer on Obstetrics.

Gentlemen—I need not dilate upon the importance of your thoroughly understanding those pathological conditions which may cause the death of the new born child, at the very moment of its birth; when its parent first thrills with joy that "a man is born unto the world," and the friends who surround her begin to rejoice in the termination of her travail and the advent of a new object of love and attention.

A few minutes hesitation or a slight error on the part of the attendant is now worth a life-time to the being whose

lease of life has just commenced, and who must enter instantly upon a new function, that of respiration, to maintain it.

Although the tenacity of the fœtus for life is remarkable, and although, as has been proved by Milne, Edwards and Le Gallois, the neonatus will survive without air much longer than its parent would, we may say that if within two or three minutes respiration is not at least attempted, the chances for life are slight, and each succeeding minute makes them slighter. In this short space of time then we are called upon to determine what is the cause of the absence of the respiratory effort, and intelligently to endeavor to remove or modify the condition which has produced it.

My conviction is that this subject has been examined into, in those works generally used by the American student as text books, less carefully than any other of equal importance in the whole range of obstetrics, and that the rules given for the guidance of the accoucheur are in direct ratio unsatisfactory, uncertain and inefficient. It is for this reason that I will consume the entire hour allotted to me this morning in its consideration.

CAUSES OF SUSPENSION OF FŒTAL ANIMATION.

Suspension of fœtal animation you will always find due to one of these causes:

Asphyxia,
Syncope,
Cerebral Congestion,
Or Apoplexy.

And with a little caution, judgment and experience, you will generally be able to diagnosticate one from the other, and apply to each its appropriate remedy.

ASPHYXIA.

The term Asphyxia, is used to designate that condition which results from the cessation of the contact of air containing a sufficient amount of free oxygen with the blood. That fluid rapidly becoming charged with carbonic acid, refuses to pass from the lungs, the heart is left without material upon which to contract, the brain is thus not supplied, and a vari-

ety of symptoms indicative of the state called Asphyxia soon show themselves.

Causes.—In the adult, Asphyxia is due to one of two causes, the absence of air as by strangling, or the admission to the lungs of air, which contains an insufficient amount of free oxygen as in breathing air containing carbonic acid, gas, &c. In the fœtus the same causes for it exist, though in it, breathing like a fish and not like a mammal, they are not at first quite so evident. Place a fish in a vase of clear water and how does it breathe? The oxygen contained in the water passing by, the movement of the ventral fins over the gills is divided from the blood which flows within their thin-walled blood-vessels, only by the coats of those vessels through which an interchange of gases readily occurs. In like manner the fœtal tufts, washed by the oxygenated blood of the mother, which is divided from the blood within them only by their thin walls, admit of an interchange which gives oxygen to the fœtal blood, and carbonic acid to that of the mother.

If the gills of the fish be bathed in water no longer containing oxygen but carbonic acid, the animal dies as rapidly almost as if it had been entirely bereft of air; and so the fœtus becomes asphyxiated either from a disordered condition of the blood, which washes over its placental tufts, or from the entire or partial exclusion of oxygenated fluid from contact with its blood.

We may accordingly arrange the causes of fœtal asphyxia thus:

1st. Compression of the cord.

2nd. Compression of the placenta.

3rd. Hæmorrhage.

4th. Separation of the placenta.

5th. Maternal toxæmia.

1st. cause.—The first of these causes, compression of the cord may occur from its prolapsing or falling down alongside of the child's head, or in a lingering labor from its being pressed for too long a time against the child's body. This, of course, presents the flow of blood to and from the placenta, which is synonymous with the lungs, the blood soon becomes charged with carbonic acid, and death ensues.

2d. Cause. The second cause is perhaps even more frequent than this, although it has received much less attention. After a labor has lasted many hours in the second stage, or after the administration of ergot at any period of the stage, the uterus not only exerts its contractile powers by alternate and intermitting contractions, but closing firmly, as it were spasmodically, upon the child, it enters into a constant and unintermitting contraction, the power of which they can testify to, whose arms have been nearly paralysed in the performance of the operation of version. Now this firm and unnatural contraction of the organ presses the spongy mass, the placenta, firmly against the child's body, closes its pouches, in which the mother's blood flows over the fœtal tufts and obliterates the uterine sinuses through which that blood passes to the placenta. It is in fact only an exaggeration of that state which we produce by rupturing the bag of waters to check hæmorrhage in the first stage of labor, and exactly the same principle is developed as we employ when we bind a billiard ball firmly in the palm of the hand, to check hæmorrhage from the palmar arch.

3rd Cause. The placenta may not however be simply compressed; it may be partially detached, the blood which flows into its sinuses readily escapes without oxygenating the fœtal blood, and the child dies without itself having lost a drop. This constitutes the third cause, hæmorrhage.

4th Cause. The fourth cause is only one step further than this; the entire placenta is detached before the birth of the child, being cast off by the violence of the uterine contractions.

The 2nd and 4th of these causes are those which produce the fatal results so often obtaining in breech presentations, where the passage of the head is not much retarded, and yet the death of the fœtus occurs. Here the empty uterus contracting, either crumples and compresses the placenta, or entirely casts it off before the head is delivered. In a natural labor the placenta is probably at times thus detached before the birth of the child, as we sometimes see it expelled before delivery in placenta prævia; as a general rule, however, it

does not take place before the head has been delivered, and the child's body is sweeping over the perineum, and thus the detachment proves of no serious consequence since respiration may now be effected by the lungs.

5th Cause. If, as already stated, the water in which the fish swims, contains carbonic acid instead of oxygen, the blood in the vessels of the gills will not be aërated, and in like manner if the mother's blood contains any poison which prevents oxygenation, a like result is produced.

This cause of death is not as common as others which have been and will be mentioned; it exists however, under these circumstances. Sometimes the placenta during the latter months of pregnancy undergoes a fatty degeneration, which injures its powers as a decarbonizing agent, and renders the child, which depends upon it for its respiration, so prone to asphyxia, that the expulsive efforts of the uterus, interfering with its (the placenta's) already disturbed circulation is sufficient to turn the balance against it.

Another way in which asphyxia is thus produced, is by the disordered placental circulation occurring with puerperal convulsions or other spasmodic seizures. During the convulsion the mother's blood is not purified, because respiration is very imperfectly performed, her carbonised blood going to the placenta is not allowed to circulate freely through it on account of its compression, and the partial closure of its sinuses from the efforts of the abdominal muscles, and thus a double reason exists for fœtal asphyxia.

But again, puerperal convulsions own as a very frequent (may we not almost say constant?) cause, uræmia from functional or organic disease of the kidneys, and the poisons, urea and carbonate of ammonia, existing in the carbonized and badly circulating blood, still farther aid in preventing oxygenation, and producing a poisoned condition of the fluids of the fœtus. From this triple danger we may almost say that it is exceptional for the child to escape, and hence the fearful mortality accompanying that accident to labor.

Symptoms.—It is of course important to be able at once to recognize Asphyxia from Syncope, or any of the other condi-

tions which produce the death of the fœtus as the treatment adapted to it will essentially differ from that which is applicable to them.

The facies of the asphyxiated child is eloquent in telling of the lesions ; the face is dusky, the lips purple and pouting, the eyes glassy, and the limbs unyielding and not flaccid, as they are when syncope has been the cause of trouble. The heart may be felt beating feebly or not at all, or its strokes may be intermittent, while there is not the slightest effort at respiration.

Prognosis.—The prognosis under these circumstances will depend very much upon the condition of the heart ; if it is beating ever so feebly there is hope, and we should under no circumstances give up trying to resuscitate the child as long as it continues to do so. Dr. Blundell relates a case in which these efforts were continued for one hour and five minutes before obvious signs of life appeared, and yet the child recovered, and numerous cases are on record in which children have been resuscitated after being for one hour asphyxiated. Should the heart have ceased to beat however, there is little if any hope, although even here we should make an effort under the supposition that it may be beating so slightly as to have escaped our notice.

Treatment.—In pointing out the treatment of fœtal asphyxia, I shall be very brief, and avoiding studiously the literature of the subject, give you concisely what I conceive to be the best resources of our art at the present day.

When the child is born and you perceive that it does not respire, pass the finger, guarded by a piece of soft linen, into the fauces, and wipe out all the mucus which adheres around the rima glottidis ; then slap it sharply upon the buttocks and chest, blow suddenly and violently into its face and mouth, and dash cold water upon it with the hand. Should it still show no effort at respiration, tie and cut the cord, and carrying the child to an open window allow a draft of cold air to blow in upon it, for it has been fully proved by the experiments of Marshall, Hall, Brown—Sequard and others, that

the young of warm blooded animals will die from asphyxia much sooner in a warm than a cold temperature.

As you will have no time for reflection when you hold in your hands a dying child, whose only chance for life rests with you, let me ask you particular attention now to this question: "What change do you desire to accomplish in the condition of your little patient, to place it under the best circumstances for recovery?" In replying to it let us retrace our steps a little.

You will remember that the cause of the cessation of the heart's action is due to the fact that the blood going to the lungs is not depurated, and consequently absolutely refuses to pass onwards through the capillaries, and return to the left side of the heart where it is needed. The pulmonic capillaries then are turgid and distended, with dark carbonized blood which will remain there, in spite of all you can do until it is oxygenated. Now if you stimulate the heart's action, what do you accomplish? Nothing I conceive except the greater congestion of the already overloaded lung, the greater distension of the already over-distended blood vessels, and consequent increase of danger to the life which you are trying to preserve. Suppose now that you put the child, as so often advised, in a warm bath; you by it increase the force of the flagging circulation, send more blood to the lungs, cause greater congestion there, and accomplish the very thing which you are most anxious to avoid. This is not theory but fact, supported by well conducted experimentation. Dr. Haighton, says Blundell, found that immersing a kitten in warm water would cause its death much sooner than in cold; and Sir Anthony Carlisle proved that a hedge-hog would survive immersion in cold water (38°F.) thirty minutes or more, while in warm water (98°F.) it will die in eight minutes. The animal then drowns much faster in warm than in cold water, and why? Undoubtedly because the circulation being more active in the former, congestion of the lungs is more rapid and complete, hence an earlier result in death.

Avoid the warm bath then, and at once disgorge the distended capillaries of the lungs by decarbonizing the blood

which they contain, and this can only be accomplished by artificial respiration.

A few years ago the poverty of our means of producing artificial respiration was indicated by their multiplicity; now our wealth of resource is shown by the fact that there is but one method by which it is effected. Had Marshall Hall written not another line he would have deserved and obtained immortality from those alone which described "Postural Respiration," and forever placed mankind under a heavy debt of gratitude to him.

To explain his method to you briefly, I will repeat before you on the body of this recently dead fœtus, one of his experiments.

Experiment.—I have closed, as you observe, with adhesive plaster the mouth and one nostril, so as to render them impervious to air, and in the other nostril have inserted a caoutchouc tube, having attached to the end a bent glass tube containing water. I now turn the body (which is lying on the abdomen) on the side, and you see bubbles of air rush through the tube and enter the lungs; and as I turn it again on the abdomen, other bubbles rush back again, showing an escape of air from the lungs. This child's body then (which was delivered about three hours ago,) is now breathing as if it were still animate, and by a process very similar to that by which it would then do so. Upon the facts elucidated by this simple experiment, hangs one of the most beautiful discoveries of which modern medicine, rich as it is in evidences of progress can boast, and here is the philosophy of it all. When the body is placed upon the abdomen, and slight pressure is made over the spine, the air in the lungs is expelled, and when it is turned upon the side, the anterior part of the thorax falls forward by its own weight, the ribs separate and the air rushes through the glottis, opened by the falling forward of the tongue, and the lungs are filled.

But let us return to our baby which we have placed by an open window, and upon which we have been operating by Hall's "ready method of respiration."

If it breathes at all we may aid the circulation and increase

the animal heat; for these purposes rub the limbs upwards with the hands, (or rather let an assistant do this while we go on respiring,) and now as the stagnation of blood in the lungs is disappearing, place the child in a warm bath and upon removing it wrap it in a warm flannel. Should it not recover by these means, we have at least done all that science at the present day can do for its benefit, and avoid the painful consciousness, which reflection on the subject must surely bring us, that we may have been by our misguided efforts instrumental in the evil result which we so much deplore.

SYNCOPE.

Obstetrical writers generally mention Syncope as a cause of suspended fœtal animation, but very little is said of it, and by some it is entirely passed over. My belief is that syncope is by no means a rare cause of death to the fœtus and for this part of our subject I ask of you an attentive hearing.

Causes. The cause of Syncope in the adult is any thing which stops the heart's action suddenly, and consequently leaves the brain unsupplied with blood; the same is true of that accident occurring in the fœtus, and the individual causes may be thus arranged.

1st. Compression of the brain by overlapping of the sutures.

2d. Loss of blood by rupture of the cord.

3d. Loss of blood by pressure on the cord.

1*st*. *Cause.* I am doubtful whether the effects of pressure on the brain can properly be classed under syncope, but they resemble it more closely than they do any of the other lesions, and I will so treat of them. The sutures which exist in the skull of the fœtus, are wisely provided for the purpose of allowing it to change its shape during labor, in accordance with the requirements of the pelvis. By their means the bones overlap to such an extent as to compress the brain so much as, according to the belief of Dr. Hamilton, of Edinburgh, and Ramsbotham, of London, to cause a

lethargic and insensible condition of the fœtus during labor; hence its stillness during that process. This pressure, if very violent and long continued, will completely empty the cerebral vessels, and temporarily impair the supply of blood to the brain, and when birth takes place, it may require so long a time for a proper balance of the circulation to be established, that death may occur before it is effected. Again, pressure may prove fatal in this way; the medulla oblongata upon which the respiratory action of the lungs depends, may become so enfeebled that it refuses to send proper influences to those organs. Now it is a well ascertained fact that the action of the heart is subservient, through the agency of the sympathetic system of nerves, to that of the lungs, as has been demonstrated by the experiments of Le Gallois and Sir Benjamin Brodie, who, cutting off the heads of rabbits and ligating the bloodvessels caused the heart's action to continue for hours, by alternately inflating and compressing the lungs. Where the medulla is thus crippled then, death may occur before it has rallied sufficiently to do its duty.

2nd Cause. As Anæmia produces syncope in the adult, so will it likewise do in the child. Thus if any rupture of the vessels of the cord should occur, syncope would evidently be the result of any considerable loss of blood. This, however, is very rare, and since in separations of the placenta from the uterus, it is now well established that the whole flow is from the mother, we would be inclined to believe that we might safely regard it as an axiom that fœtal anæmia, from sudden removal of a portion of its vital fluid as by no means of common occurrence. Observation and reflection have led me to a very different conclusion, one which, as far as my knowledge goes, is not mentioned by the more modern obstetrical text-books, but allusions to which may in found in the works of older observers, as Fretéau, Desormeax, Velpeau, &c. I offer it to you now as a suggestion by which to direct your attention to the subject at the bed-side, where I think you will find abundant evidence of what I now tell you.

3rd Cause. The last and most frequent cause of syncope in the

neonatus, is anæmia occurring immediately from pressure on the cord.

In studying up the subject of asphyxia in your works on obstetrics, you will be struck by an apparent contradiction in their descriptions, which it will be difficult to reconcile. Thus Cazeaux, after describing the dusky appearance of the surface of the asphyxiated fœtus, says: "Nothing is more common than to see the fœtus come forth without any abnormal coloration of the skin, and even with a very remarkable paleness, and flaccidity of the limbs, although the apparent cause of death has been compression of the cord." You will find too that the very same condition described by some as asphyxia, is called by others syncope, and by others still anæmia. Thus Velpeau, in speaking of asphyxia, says: "It is this state which has been known under the name of Asphyxia of the neonatus, which Gardien prefers to call Syncope, and which others have described under the name of Anæmia." Now the pale face of the syncopic infant does not certainly resemble so closely the congested and dusky one of asphyxia that they could be readily mistaken, or described synonymously.

The explanation of the seeming paradox lies, I opine, in the anatomical structure of the cord. Let us examine it. The funis umbilicalis consists of three vessels, two arteries which carry blood from the fœtus to the placenta, and one vein which carries it from the placenta to the child. If this canal is fully compressed, all communication between the child and placenta is cut off, and asphyxia results; but if the pressure upon it is only partial in its effects, the vein will yield to it, and the flow through it to the child will be checked, while the arteries being difficult of compression, will still continue transmitting blood from the child to the placenta, which is an entire loss since it cannot return to the fœtal circulation.

This difference in the compressibility of arteries and veins exists all over the body, why should we deny it an existence here?

If you compress equally an artery and vein lying side by

side in the same tissues, you will stay the current through the vein, while that through the artery will still continue, and soon you will find that the distal side of your compressing power will be gorged with blood, which cannot return to the heart. In the same way the placenta and placental end of the cord are gorged with blood which cannot return to the child's body. But if any one wishes to convince himself of this fact by experiment, let him as soon as a child is born, lightly compress the cord with his fingers, and he will soon satisfy himself that blood is at every stroke of the heart sent from the child, which cannot return to it so long as the pressure is continued.

When the cord has prolapsed into the vagina, we can readily understand how immediate and complete obliteration of its calibre can occur, but when it has not done so, I believe that almost every case of asphyxia caused by its complete compression is preceded by partial obliteration and consequent anæmia. This may explain those cases of asphyxia in which none of the marks of that condition are to be found, but in which those of anæmia are so markedly present as to induce some to describe all the cases under that name, as Velpeau states.

Unfortunately it is not always easy to diagnosticate this condition from asphyxia, for a child asphyxiated for a long time, loses many of its characteristic marks, as the purple and dusky face, &c. Still where the state of anæmia is well marked, we will generally be able to recognize it and direct treatment accordingly.

Symptoms. The symptoms by which syncope will be recognized in the young child, will be paleness, (extreme if anæmia has given rise to it,) flaccidity of the limbs, and a sunken cadaverous look of the face.

Prognosis. When a child is still-born from syncope, the prognosis will depend entirely on the severity of the attack, exactly as it would do in an adult, but in syncope depending upon anæmia, the prognosis is particularly grave. The type of this class is the pale, flaccid, and wilted child (generally called asphyxiated,) which we may rouse up by remedial

means until it breathes and cries, and then relapses and soon dies.

Treatment. I know of no practice in a teacher of medicine which proves more pernicious and more perplexing to the student than that of "splitting hairs," in describing the diagnosis and treatment of diseased conditions. It is my wish to avoid this charge here, and I will do so by endeavoring not to surfeit your memories by a number of useless rules which will not apply at the bed-side. Do not understand me as advising you to occupy the precious minutes which follow the birth of a still-born child, in trying to determine whether or not the dangerous state before you is the result of asphyxia or syncope, but in either case adopt this as a rule: *the first thing needful is respiration; establish it artificially if you cannot make it occur naturally.* The treatment of the two conditions is in fact exactly the same with this additional precept in a child whose pale, bloodless and wilted look leads you to believe in the existence of anæmia; do not cut the cord, but while an assistant causes it to respire, take the funis in your fingers and press all the blood which it contains slowly and steadily into the child's body. In other words, "strip the cord," towards the child so as to empty its vessels and cause their contents to flow into the system of your anæmic patient.

If the views which I have here offered you are correct, this treatment will certainly be appropriate, while if they be incorrect no harm will accrue from it (I assert from repeated trials,) unless the child be suffering from cerebral congestion or apoplexy, and this would scarcely be confused with the state of which we have been speaking.

The funis is often very long, and almost always long enough to contain a sufficient amount of blood to destroy the life of a newly born child, should its capacity be removed from its circulation. By the method I have recommended, you give all this to the child, and in many cases you will see excellent results from it. I remember a case to which I was called by one of my students, in which the following clear and curious corroboration of this view was observed. The child which

appeared to me to be the type of a syncopic and anæmic infant, was with difficulty made to cry very fleebly, and by artificial respiration to breathe at long intervals. As soon as a little blood was forced into its body from the cord which I held in my hands, it would show much more obvious signs of restoration, and as soon as pressure on the cord was removed it would relapse. This I repeated several times to my own satisfaction, and to that of the gentleman who was with me.

It is needless to remind you of the care and gentleness with which this transfusion into the child must be performed, for you will at once perceive that rashness and haste might rupture some of the vessels of the fœtal portal system.

Lastly, I would draw your attention to the fact that you do not restore blood to the circulation of the child for the purpose only of arousing it from syncope, but to overcome the state of anæmia into whioh it has fallen ; for if the latter condition continued, removal of the former would accomplish nothing.

CEREBRAL CONGESTION.

The third cause of death to the neonatus is cerebral congestion, an accident which has been unfortunately confused with apoplexy, and described under that name.

Again disclaiming any intention of multiplying your troubles by multiplying divisions, I would advise you to fix clearly in your minds the fact that in the fœtus, cerebral congestion and apoplexy are in their pathology no less unlike than the same conditions are in the adult, and we have no more justification in confounding them. The differential diagnosis between these two lesions is, I believe, often impossible in the young child ; for the effusion generally taking place from the vessels on the surface of the brain, and according to Cruveilhier most commonly from those on the surface of the cerebellum, paralysis does not occur, nor are the other symptoms of apoplexy in the adult generally noticed. Of what use then you may ask, is it to describe them as different conditions ? I answer, because in many cases we may make a

correct diagnosis, because the prognosis of the case will rest upon it; and because here as elsewhere, if we ever wish to improve in the treatment of diseases, we must certainly first begin by understanding well their true pathological characters, and dividing them accordingly.

Causes. The causes of congestion of the brain in the fœtus are these:

1st. Face presentation.

2d. Constriction of the cervical vessels by the cord.

3d. Retarded delivery of the shoulders.

1st Cause. Decidedly the most common cause of death in face presentations is the bent and obstructed state of the vessels of the neck, which in the veins prevents the blood, which still goes to the brain through the less compressible arteries, from again returning to the heart. Let any one who doubts this press his head violently backwards until the occiput touches the spine, and retain it in this position for a few minutes, and he will soon be convinced by the symptoms which will show themselves that active congestion of the brain is occurring. The blackened, bloated and blood-shot face of a child coming by this presentation, will also fully assure you of the correctness of this view, and should you be so unfortunate as to lose the child, its peculiarly convincing appearance will never let you doubt it more. Alive or dead, the face-delivered child looks much more like a sot just delivered from a watch-house, than an innocent delivered from its mother's womb. This arises in fact from the congestion of the vessels above the point of obstruction, and in part from the compression of the face in the vagina, and the formation upon it of the caput succedaneum.

2d Cause. The cord is sometimes wound two or three times around the neck of the child so as to constrict its vessels and congest the brain and parts above the ligature. Such an accident might likewise produce asphyxia, but my belief being that it more commonly proves deleterious by acting on the cerebral circulation, I have preferred mentioning it here. It probably causes death however by both means at once, exactly in the same way as we unfortunately often see it oc-

cur in the barbarous and cruel mode of capital punishment resorted to in this country and in England.* Here again the veins are compressed and their flow checked, while the more vigorous current through the arteries still goes on until, with the coincidence of apnœa, death is produced.

3d Cause. When the head has been delivered and the shoulders are stopped in their passage, the child may breathe and cry for from ten to twenty minutes without danger. But sometimes the chest is so much compressed by the parts within the pelvis, that respiration cannot go on, and it may die under these circumstances either from asphyxia (the nearly empty uterus crumpling or detaching the placenta,) or from cerebral congestion, the soft parts of the mother's passage closing the canals of the veins.

Prognosis. The prognosis in congestion of the brain is favorable whenever the heart is felt beating.

Symptoms. The symptoms of cerebral congestion leave it by no means difficult of diagnosis. They are a blackened and congested face, puffed and violet lips, protruding eyes and a generally swollen appearance of the head.

Treatment. The treatment of this condition is embodied in these three principles: 1st. Let blood flow freely from the severed cord to the amount of one ounce or more as the case may require. 2d. Establish artificial respiration; and 3d, avoid the warm bath.

APOPLEXY.

We now come to the last cause of fœtal death—apoplexy. The delicate blood vessels of the brain distended by the fluid which, forced into them by the systolic action of the heart, cannot return on account of obstruction, now yield, and effusion takes place into some part of the organ, the membranes, ventricles or substance. Any of those causes

* Dr. Reid in his Physiological Anatom. and Patholog. Researches, and others who disbelieve the possibility of increase of the fluids within the cranium, deny the existence of cerebral congestion as a result of hanging.

then which produce congestion, may, by being continued or exaggerated, cause apoplexy. In addition to these must be mentioned injuries done to the head by violent uterine efforts, forcing it against some prominent point in a deformed pelvis, and injuries from the forceps or vectis. Any thing in fact which would cause fracture in the fœtal skull would likewise be sufficient to cause apoplexy, though of course they do not depend upon each other, and one may occur without reference to the other.

Prognosis. The prognosis of apoplexy is very grave. The child cannot be resuscitated or is resuscitated, and remains stupid and listless for some hours or days, and then dies. At other times convulsions supervene and death soon follows.

Symptoms. We may suspect the existence of apoplexy when those symptoms just mentioned under the head of prognosis, are suspended to those which follow mere congestion of the brain, and we make our prognosis accordingly.

Sometimes, however, hemiplegia is present, and the signs of apoplexy are pretty well established. A case of complete hemiplegia caused by injuries done to the skull by the forceps, was mentioned to me as occurring in his practice by Dr. James O'Rourke, of this city. The child remained paralyzed for many months and ultimately recovered.

It may be well for me to state here that the facial paralysis observed at times in children delivered by the forceps, is due not to a lesion occurring in the brain, but to compression of the portio dura of the seventh pair of nerves as it emerges from the skull through the stylo mastoid foramen.

To give you upon closing this lecture, a resume of the treatment I have advised, I would say, 1st. Let the lesion be what it may, establish respiration. 2d. Should signs of cerebral congestion or apoplexy exist in addition, let blood from the cord; and 3d. Should the appearance of your patient lead you to believe it anæmic, "strip the cord" towards its body before cutting it.

ART. V.—*On Prophylaxis in Cholera Infantum.* By ROBERT S. BAILEY, M.D., of Charleston, S. C.

It is not my intention to describe the nature, ætiology or pathology of this disease, this having been already done by numerous distinguished medical writers, but chiefly to notice that particular period which elapses between early infancy and the completion of the first dentition; (more especially if that process takes place at the commencement of the warm seasons,) and the proper course to be pursued for the nourishment of the child, believing that some erroneous views prevail both with the profession and the laity on this subject, which I will endeavor to illustrate by the following cases:

Case 1st. January, 1857 a lovely and interesting child, (a girl,) almost three months old, had enjoyed excellent health, till her mother had an abscess of the breast, which interrupted its nursing. I strongly advised her to attempt nursing the child with one breast, and assist, by administering *cow's milk and water in proper proportions—her health however, begun to be affected—she became feverish, and debilitated, and eventually had to wean the child. The cow's milk however, was continued, and a wet nurse also procured; but the child preferred the former to the latter, and entirely weaned herself from the nurse; this mode of nourishment agreed. I recommended the parents as soon as she had a tooth to gradually accustom the child to animal food before the warm season commenced, and not long afterwards, I had the satisfaction of observing her partake of the leg of a chicken, apparently with a keen appetite. It was sometime before I saw the child again; it appeared healthy, but had lost its plumpness. I was then informed that they were advised not

* For a process by which cow's milk can be made to resemble human milk, by diminishing the casein and increasing the quantity of butter, see an interesting paper on "Natural and Artificial Lactation," by Henry Cummings, M. D., in the American Journal of the Medical Sciences, for July, 1858.

to use animal food in any shape, as it might be the cause of the child's death. A farinaceous diet was therefore persisted in, and I did not again interfere in the case. About the latter end of March, the child had a slight cough, and croupiness, and was supposed to be taking the hooping cough. An emulsion was prescribed, and she shortly recovered. As the warm season approached, she had diarrhœa, which soon became worse. I was requested to see her on the 10th of August; I found her reduced to a skeleton, in much pain, and suffering from tenesmus; her evacuations scanty and green, and tinged with blood. Beef tea was given, which she appeared to relish; also a mixture of rhubarb and magnesia. She lingered till the 14th, when she expired.

Case 2d. A child about four months old, (also a girl,) and somewhat delicate, had been healthy since its birth, and allowed to take almost all kinds of food with impunity, until one day she partook of some stewed duck, when she was almost immediately seized with violent cholera morbus. She was so much prostrated, that her dissolution appeared to be almost inevitable; but by the promptitude and skill of a physician, who happened to be present, the impending danger was removed, and the child rallied. She however, never recovered from the shock, but soon lost her appetite, and became emaciated. A change of air was recommended and tried, but was of no benefit. The mother becoming again pregnant, a wet nurse was obtained. The child, however gradually sank, till death took place on the 3rd of October. I will now, by way of contrast relate a case where a different course was pursued.

Case 3rd. On the 1st of June, 1857, my attention was called to a child, (a boy,) about a month old, much in the same predicament as in case 1st. The mother, soon after her delivery, having been attacked with severe inflammation of one breast, which terminated in mammary abscess. The nipple having retracted, the child could not nurse; by perseverance, however, on the part of the physician, and a considerable degree of fortitude on the part of the mother, the breast was kept permeable, and it was recommended to assist the child

by the partial use of cow's milk and water, in proper proportions, and when a little older, to substitute gruel for water; afterwards a little soft hominy and milk. This plan was adhered to, and agreed with the child. It was also recommended when the child was about six months old, to gradually accustom him to animal food before the approach of summer. These directions were complied with, and attended with success. The process of dentition was slow in commencing, and did not produce any indisposition except slight fever, when the teeth were about to appear. During the winter he had catarrh, but no affection of the bowels; he is now about sixteen month's old, and in the enjoyment of health.

Remarks.—Of these cases, the first appears to have died from the effects of inanition; the second from improper nourishment; whilst the third appeared to improve from the plan pursued. The cases are, however, too few and isolated to furnish any rule of practice. They may nevertheless suggest some hints as to the most eligible method to be adopted preparatory to the transition from lactation to other modes of nourishment. It appears to be the most conformable to nature, that animal food should be had recourse to on the first appearance of the teeth. There are, however, some exceptions to the rule. I have observed amongst our colored population, that they not unfrequently feed their children almost from birth, and apparently without any detriment; and when attended by any injurious consequences, it is generally by producing convulsions and not cholera.

As illustrating this subject the following remarks and case reported by a writer in one of our periodicals, are worthy of attention.*

"Notwithstanding the unjust odium into which the doctrine of the humoral pathologists has fallen, from the extravagant opinions entertained by the humoralists themselves, and the

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* Diet as a remedial agent in the treatment of disease, by T. N. King. Southern Journal of the Medical and Physical Sciences, January, 1853.

abuses of mercenary quacks, there is probably more real importance in a correct knowledge of the character of the blood, in health and disease, than the most sanguine humoralist had conceived. Why is it, that in a certain form of disease, the patient should express so strong a preference for a particular description, or article of diet or drink, above all others? And why should this same individual, laboring at a subsequent time under a different species of disease, crave an opposite or different kind of diet and drink? An ague patient, for instance, almost invariably relishes oysters, beefsteaks, and other highly nutritious animal diet, rich in fibrinous matter, while the same individual, in an attack of typhoid fever loathes these articles, with almost every other species of solid animal food, and selects sour buttermilk. It is not sufficient to say, that the system in the one case needs a richer blood, and that in the other, the fibrinous portion of the circulation requires to be reduced; this is not a satisfactory statement even of the fact itself. Why should an individual, who was last week eating freely of ham and beef, feel a disgust at the bare suggestion of such a diet, and crave an article of diet or drink, which perhaps he has no relish for in health? To say that the sense of taste is modified or impaired by the fever, or that such an article is more grateful to the stomach of a typhoid fever patient, is the mere statement of the fact, but offers no satisfactory solution of the cause. An infant of twelve months has been the subject of cholera infantum for the last four months, and is reduced to an extreme of anæmia and emaciation that is scarcely compatible with the phenomena of organic life. Week after week, it has been subjected to another and a different course of medication and diet, until medicines have almost entirely ceased to exert any beneficial influence upon the stomach and bowels: food of the mildest and most digestible quality, is passed through the bowels in an indigested state, and the medical attendant concludes that the functions of digestion and assimilation are so far impaired, that the little patient must inevitably die of inanition. At this juncture, he usually learns from the nurse,

that for several days or weeks past, the child has been in the habit of crying at the sight of pine-apple cheese, an article of diet, of all others, perhaps the most unsuitable for an infant, and difficult of digestion by even the strongest stomach. As an extremely hazardous experiment, but justifiable under the circumstances of the case, he consents that the little patient should be freely indulged with the cheese, and forthwith a stomach, which for months past, could not digest the mildest, and under ordinary circumstances, articles of diet the most easily assimilated, readily digests the strong diet, the bowels begin to improve, and the patient, to the surprise of all parties, is suddenly snatched from the very jaws of death, and rapidly restored to health and flesh. There seems to be a species of instinct in such cases, similar to what we observe in animals, as they select certain species of herbs and grass, and reject others. So far as the case of the child is concerned, it could not have been because it had been habitually fed upon cheese, or because the cheese, as a therapeutic agent, possessed any efficacy in curing cholera infantum. In a great majority of such cases, in the earlier stages at least, and even in the ordinary condition of infantile health, no article of diet would more likely disturb the stomach and bowels. The only rational conclusion we can deduce from such facts is, that this instinct, (if it be lawful so to term it,) is an expression of the elective affinities of the blood for certain ultimate or proximate elements contained in particular kinds of alimentative material, which are required by the blood, to restore the defective or imperfect protein element, and establish the normal relation of its constituent parts. In the case of the child with cholera infantum, had the cheese been withheld, in all probability, death would have been inevitable."

Amongst the predisposing causes of cholera infantum may be mentioned the persistent use of liquid nourishment after the stomach is capable of assimilating solid food, thus injuring its tone and consequently impairing the digestive process; for some excellent practical remarks on this subject, applicable to the case of infants, as well as adults, I refer the reader

to the report of Dr. Charles Hooker.* Another source of diarrhœa and affections of the bowels in children, is when the mother, during the term of lactation, becomes pregnant. A popular opinion prevails that she is then giving the child bad, or as some say, "poisonous milk;" but from the interesting experiments of Dr. Davis, he proved that the milk is impoverished, or deteriorated, and not affording sufficient nourishment, and consequently the child suffers from inanition. He remarks, "The occurrence of pregnancy during lactation, produces a very marked diminution of all the solid or nutritive constituents of the milk, such diminution continuing to increase as the pregnancy advances."† To indicate how the child is affected, I transcribe the two following cases from his report. "The child of Mrs. G., which was only about four months old when the mother again became pregnant, was perfectly healthy and well nourished up to that time. Soon after it began to be peevish and restless, with flatulency and occasionally green discharges from the bowels. These changes were at first slight, but they gradually increased, and at the end of six weeks it was evident that the child had ceased to be well nourished. Its tissues became soft and flabby, and the mucous surfaces irritable, as manifested by occasional vomiting, and more frequent intestinal discharges of a green color, and sometimes mixed with mucus. During the succeeding two months the child was subjected to several attacks of severe watery diarrhœa, accompanied by so much emaciation, that the parents took it from the city into the country, with the hope that a change of air would restore it. While absent, however, it was attacked with cholera morbus and died in two or three days."

"The child of Mrs. B., was healthy and well nourished un-

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* Report on the diet of the sick by Charles Hooker, M.D., of New Haven, Connecticut—Translations of the American Medical Association, Vol. 8th.

† Report on the changes in the composition and properties of the milk of the human family, produced by menstruation and pregnancy. By N. S. Davis, M.D., &c., Chicago, Illinois.—Transactions of the American Medical Association, Vol. 9th.

til it was five months old, when the mother again became pregnant, which was in the month of March, 1855. During the month of May, the mother applied to me for advice, saying that for three or four weeks her child had been unusually fretful and restless; that it was troubled with flatulency, often rejected its milk and was 'getting poor.' I examined the child's mouth, but found no swelling of the gums or other indications of irritation from teething. Although temporarily relieved by medicine from time to time, the child continued to fail in its nutrition and to become more and more subject to diarrhœa and vomiting, until during the latter part of summer, it presented the appearance of extreme emaciation and anæmia. The mother persisted in nursing it until the middle of September. Soon after it was taken from the breast, it began to gain in flesh and strength, and continued to do so for two months. At the end of this time it was attacked with symptoms of subacute meningeal inflammation, and died in about six days with evident effusion of serum on the brain."

Dr. H. F. Campbell, of Georgia, to whom the late lamented Marshall Hall has justly acknowledged his priority of claim in relation to the excito-secretory system of nerves, thus describes the phenomena of normal and abnormal dentition:* "During easy, or normal dentition, the salivary secretion is increased, the gums are swollen, the mouth hot, and the child evinces a disposition to press every substance within its reach upon the gums, in order to relieve the irritation it here suffers. Later, the gums become more swollen and softer, the irritation more distressing, and, under certain circumstances, the mouth dry and slightly inflamed. The child becomes fretful; its sleep is disturbed and feverish, its bowels become loose, which latter symptom we frequently observe accompanied by nausea and vomiting. Cases of abnormal dentition are brought more frequently under the

* See Southern Medical and Surgical Journal, Vol. 6, No. 6, June, 1850. An Essay on the Influence of Dentition, in producing Disease. By Henry F. Campbell, M.D., Demonstrator of Anatomy in the Medical College of Georgia.

cognizance of physicians, and their phenomena are hence familiar to every one. The above symptoms are exaggerated. Some, which in normal dentition were of trivial importance, becoming so severe as to threaten the life of the patient. Thus the gently relaxed condition of the bowels, which in easy dentition was beneficial, is now changed to diarrhœa, with distressing tormina, and alarming emaciation. The salivary glands, which, in easy dentition, manifested their implication only by increased secretion, now become inflamed and swollen, till finally their secretion is altogether arrested, leaving the mouth and tongue dry, parched and painful; and the nervous fretfulness of the normal process is often replaced by actual fever, sometimes attended by the most terrific convulsions."

If such is the effect of abnormal dentition, in producing such a degree of irritation, or inflammation of the mucous membranes, it may admit of a question, whether the usual routine remedies, the gum lancet, and calomel, are the most efficient means to be employed in such cases. When the gums are intensely inflamed and swollen, the teeth about making their appearance, or the child threatened with convulsions, in such cases, I believe scarification will give much relief—but on the other hand, when dentition is so retarded as to be almost imperceptible, I am not aware that lancing the gums as a matter of course, will at all expedite the process: on the contrary, it is possible, that whilst the practitioner flatters himself with the vain hope that he has at least removed one great source of distress and uneasiness, he is inadvertently implanting the very irritation he is so desirous to subdue. May not the pain, or irritation, be of a neuralgic character as well as muscular? Under such circumstances, I have known much relief obtained by frequently rubbing the gums with the finger dipped in honey or syrup; and it is in this way that the many "soothing syrups," and other empirical remedies, are too often had recourse to, to the exclusion of the regular practitioner. In respect to mercury, that it is a powerful stimulant, and therefore not well adapted to diseases of an acute or inflammatory character,

I need only refer to standard writers on the Materia Medica. Dr. Cullen thus writes:* "Universally, mercury, in its active state, seems to be a stimulus to every sensible and moving fibre of the body to which it is immediately applied; and in consequence, it is particularly a stimulus to every excretory of the system to which it is externally or internally applied. Besides its noted effects upon the excretories of the saliva, it seems to operate upon the whole of those of the alimentary canal." Wood and Bache† describe the effects of mercury as follows: "Of the modus operandi of mercury, we know nothing, except that it probably acts through the medium of the circulation, and that it possesses a peculiar alterative power over the vital functions, which enables it in many cases to subvert diseased actions, by substituting its own in their stead. This alterative power is sometimes exerted without being attended with any other vital phenomena than the removal of the disease; while at other times it is attended with certain obvious effects, indicative of the agency of a potent stimulus. In the latter case, its operation is marked by a quickened circulation, with a frequent, jerking pulse, by an increased activity imparted to all the secretory functions, particularly those of the salivary glands and the liver, by an exaltation of nervous sensibility; and, in short, by a general excitement of the organic actions of the system." In respect to the "Remote effects," ‡Periera observes: "In small and repeated doses, the first obvious effect of mercurials is an increased activity in the secreting and exhaling apparatus. This is particularly observed in the digestive organs; the quantity of intestinal mucus, of bile, of saliva, of mucus of the mouth, and probably of pancreatic liquid, being augmented. The alvine discharges become more liquid, and contain a larger proportion of bile." An objection to the employment of mercury in the bowel complaints of infants, is, that it is prescribed indiscriminately in opposite pathological condi-

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* A Treatise on the Materia Medica, by William Cullen, M.D., Vol. 2.

† The Dispensatory of the United States of America, 1839.

‡ Elements of Materia Medica and Therapeutics, Vol. 1.

tions; for example, it is had recourse to in suspension of the biliary secretion, and on the other hand, when there is a redundancy of bile, calomel is still the "sine qua non;" in such a state of the intestinal canal, the therapeutical indication is evidently to administer medicines of a bland, demulcent, or emollient quality, as best adapted to an inflamed surface. Many years ago I noticed a case, in which permanent benefit was derived from the employment of olive oil, when other medicines had failed to give any relief. Arrow root, as an article of diet, agrees well with some children, forming an excellent substitute for the mother's milk; but in others I have observed it to produce tormina, in its passage through the bowels. In such cases, rice gruel will frequently answer the same purpose, and is not so apt to become acid as gruel made from corn. Gelatine, as suggested by Dr. James Stewart,* promises to be a valuable succedaneum as preparatory to a stronger food. I have sometimes found that a broth prepared from whitings, as recommended by Dr. Chalmers,† of benefit to teething children, and the little patients appear to partake of it with avidity.

I shall conclude my remarks on the prophylactic treatment of cholera infantum, by the following brief recapitulation:

1st. When the mother, from weakness of constitution, or other causes, is not able entirely to nurse her child to assist at as early a period as possible, artificially, with cow's milk.

2d. To avoid a farinaceous diet, when it is found to disagree with or disorder the child.

3d. To avoid liquid nourishment as soon as the child is capable of digesting solid food.

4th. To immediately procure a wet nurse should the mother become pregnant during lactation.

5th. To accustom the child to animal food before the warm season approaches.

6th. To avoid excessive medication, more especially drastic purgatives.

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* See Bibliographical Notice of his Essay on Cholera Infantum, in American Journal of the Medical Sciences, for October, 1856.

† See Chalmers on Weather and Diseases of South-Carolina; Vol. 2, 1776.

ART. VI.—*Hermaphrodism: description of a supposed case, &c.* BY F. LE JAU PARKER, M.D., House Physician to Roper Hospital.

Mr. Editor:—On the 18th of June last, I was requested by my uncle, Prof. Frost, to perform a post-mortem upon the body of a negro named Jacob, slave of H. Garden, (a free person of color,) who had died of hypertrophy of the heart, and about whose sex the owner was uncertain. A superficial examination discovered that the suspicions were not without some foundation, and that the case really presented one of those singular apparent sexual anomalies usually classed, and described, under the generic term, Hermaphrodite.

Finding the specimen likely to prove interesting, I made some inquiries into the history of the individual, and learned that she* was one of a set of twins, the brother being a large robust man, and the father of a family.

She was sixty-seven (67) years of age, dressed as, and passed for a man, was called Jacob, and was bred a cooper, which occupation she continued to pursue till the time of her death.

In disposition she was reserved and retiring, seldom spoke unless addressed, seemed suspicious of all who approached her, and was more inclined to associate with women than with men. In temper she was mild and gentle, unless allusion was made to her suspected deformity, which she always resented in a manner not unbecoming her male apparel.

She had menstruated regularly to the age of fifty, each monthly return invariably giving rise to great pain, and the severe constitutional symptoms, (readily accounted for by the anatomy of the organs,) usually consequent upon dysmenorrhœa. This seems to have confirmed the suspicions of her doubtful sex; nothing irritated her more than to be called "a woman."

The age at which she began to menstruate was not known,

* Jacob was found to be a woman; as such she is alluded to throughout these remarks.

having attained her growth before she came into the hands of her last owner; nor could I learn if the clitoris was subject to erections—though it would seem so, as she (he) had been married, but the union was not a long or a happy one, whether arising from the ridicule of the men, or the dissatisfaction or contempt of his wife, does not appear.

She seems to have shown no inclination for the male, which is remarkable, as it does not concur with the general history of such persons, who usually exhibit a morbid sexual propensity, especially during the menstrual seasons.

Her height was considerably above the average among women, her shoulders were narrow, chest contracted, hips very large, thighs full and round, hands and feet small; the face was perfectly smooth, the volume of the larynx, tone of voice, expression and walk, decidedly feminine; her mammary glands were very large, much more developed than in most women at forty—in short, her general conformation was that of an adolescent female.

Having removed the specimen, I, with the assistance of Prof. Chisolm, carefully examined it. The mons veneris was rounded, protuberant, and well covered with hair; there was a well-developed scrotum with a distinct raphe; the right side, which was rather larger than the left, contained a body having the conformation of a testicle both to the sight and to the touch. Upon incising it, however, it was found to be a tumor of the form and size of a testicle, composed of fatty and erectile tissue, containing a cyst in its centre filled with a sero-sanguinious fluid. At its upper portion it tapered off, became narrow and rounded, extending upwards through the inguinal ring, being the prolongation of the right ovary, hereafter to be spoken of. There were no traces of an epididymis, vas deferens, or the tunics of the testicle. The left side of the scrotum was filled with a fluid of the color and consistency of an ordinary hydrocele. The whole scrotum was well supplied with hair.

The clitoris measured five inches (5 in.) in its entire length, having a distinct corpus spongiosum and cavernosum, and extending under the arch of the pubis to an enlargement

resembling the prostatic gland at the neck of the bladder, which will be mentioned afterwards. Externally, the clitoris measured an inch and three quarters, was furnished with a gland three quarters of an inch in diameter, with a urethral slit, or cul-de-sac, a quarter of an inch in depth. It had also a mobile prepuce, a large artery corresponding to the dorsalis penis artery, and a frænum.

The frænum, after descending a quarter of an inch was divided by a vertical slit, forming the nymphæ, and the upper extremity of the vulva; and rising on each side of it were two thin labia, with irregular edges, destitute of hair. This opening, or vertical slit, was an inch and a quarter in length, a quarter of an inch in depth, extending towards the anus; its floor being blocked up by a dense membrane, which, when pressed by the finger, felt as if it stretched across a cavity. At the inferior junction of the labia was a small opening capable of admitting an ordinary probe, forming the external orifice of the vagina, and giving exit to the urine and menstrual fluid. This canal extended backwards two inches under the arch of the pubis, where it bifurcated, or more correctly, was joined by another, one branch going to the bladder, the other being the continuation of the vagina.

Here the urethra proper began, was half an inch long, closed by a small valve, and large enough to admit the passage of a common female catheter. At this junction of the canals, there was an enlargement simulating the prostatic gland, made up of muscular and cellular tissue, exceedingly vascular, but devoid of the seminiferous tubes and ejaculatory ducts belonging to that body.

The vagina was here contracted and thickened, but soon assumed the usual dimensions, with its characteristic rugæ, and proceeding upwards was lost in the neck of the womb. The uterus was ovoidal in shape, and enlarged by five or six fibro-cartilaginous tumors, with a short narrow neck, placed quite to one side, with an os tincæ proportionally small.

On the left side, the fallopian tube was naturally formed with an ovary and fimbriated extremity. The right fallopian

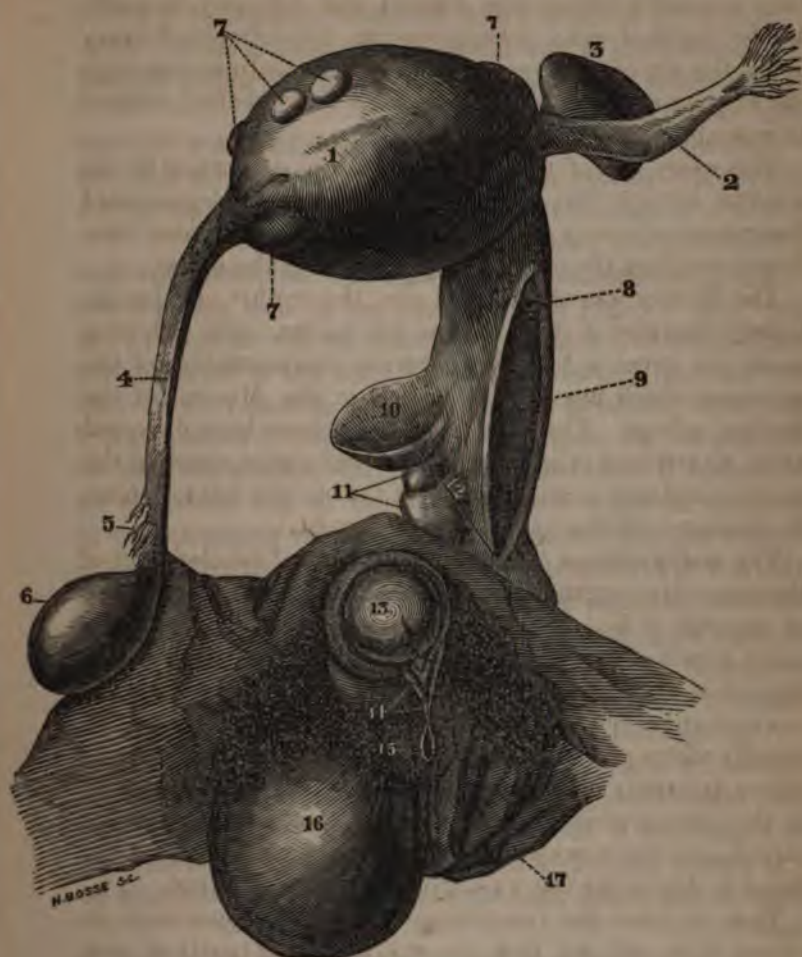
tube was much longer than natural, ran obliquely forwards to the inguinal ring, where was an hypertrophied ovary about the size of a pigeon's egg, and which continued through the ring into the scrotum, forming the tumor before alluded to resembling the testicle.

This ovary was of the same character as the tumor in the scrotum, except that it contained no fluid; a degenerated fimbriated extremity was given off from the fallopian tube before reaching the ovary as represented in the figure.

The bladder was of the usual size, the urethra opening obliquely from one of its sides into the vagina. The following wood cut gives a tolerably accurate representation of the specimen which has been deposited in the Museum of the Medical college. The superfluous parts have been dissected away, an artificial opening made into the vagina, showing the os tincæ, and only a small portion of the bladder left to exhibit its relation with the vagina. [See page 61.]

The malformations or imperfect abnormal development of the generative organs sometimes met with, in which there is, or appears to be, a close approximation of one sex to the other, admit of many varieties. In most instances a simple inspection of the parts enables the examiner to decide without hesitation the proper sex of the individual; but occasionally others present themselves more complex in character, where deformity is conjoined with obscurity and ambiguity in the arrangement of the particular organ or organs concerned, and where minute and prolonged examinations have failed to detect the exact nature of the parts involved.

Thus in some the resemblance is only in appearance, in others, it is said we find one sex partaking partly of one, partly of the other, having some of the organs common to both. The ambiguity in the case here described under the generic term, is not sufficient to warrant any extended remarks upon the cause, or on the varieties presented in these almost incomprehensible deviations from nature in her ordinary processes of formation. Development is unquestionably at fault and by changes in its successive steps, we are to account for the singular combinations and resemblances in the generative



Explanation. No. 1, the uterus; 2, the left fallopian tube with its fimbriated extremity; 3, the left ovary; 4, the right fallopian tube with 5, degenerated fimbriated extremity; 6, the right hypertrophied ovary; 7, the fibro-cartilaginous tumors; 8, the os tincæ; 9, the vagina; 10, the bladder; 11, the enlargement resembling prostatic gland; 12, the direction of the urethra; 13, the clitoris with its gland and urethral slit; 14, the frænum forming nymphæ with the labia rising up on each side; 15, the common exit of the vagina and urethra; 16, the right side of the scrotum containing a tumor; 17, the left half of the scrotum containing fluid.

organs of the sexes, of which the above example of spurious hermaphrodism in the female is the most common; these irregularities occurring far more frequently among females than among males.

Still we may add a few observations with a view of pointing out the correspondence or differences between this, and others of a similar nature, especially as there are one or two peculiarities not previously noticed.

Jacob, notwithstanding her name and occupation, her having been brought up as a man, and as such married, was without doubt a female. Possessing as she thought two of the essential characteristics of the male, and at the same time that most wonderful organ, the uterus, necessary for the conception, and the mammary gland, requisite for the support, of the infant, it is not surprising that this unfortunate and supremely unhappy creature, tortured in her ignorance by doubts and conjectures, hesitated as to her proper sphere in life, and in her uncertainty and distress vascillated between the two opinions; associating at one time with one sex, at another with the opposite, her convictions strengthened or weakened by the amount of persecution received at the hands of either party.

The supposed penis was merely an enlarged or unnaturally developed clitoris, by no means an unusually large or long one, others having been reported far surpassing it in length and similarity to the male organ; some have even reached the almost incredible length of seven, ten and twelve inches—a size which must have been the result of diseased action.

Complete erection of the clitoris could not take place in consequence of the manner in which it was bound down to the scrotum, or more correctly to the nymphæ and labia. Had the dense membrane which stretched across the floor of the vulva, been divided, during the early period of the subject's life, and the contracted portions of the vagina dilated, a free exit would have been given to the menstrual fluid, and thus have mitigated in some measure her monthly sufferings.

The narrow and constricted neck of the uterus may, however, have been the primary and more immediate cause of the difficulty and pain attending her catamenial flow, in which event the assistance of art would have availed but little. As regards conception, although there was one apparently healthy ovary, the dimensions and malformation of the womb were such, as to preclude the probability of an event which would have ended in the destruction of life; and, besides this, experience in all similar cases renders it hardly possible.

The body in the scrotum was calculated to mislead, the more especially as we read of persons having an ovary on one side, and on the other a perfectly formed testicle, and again, others in which one, or both ovaries have descended through the inguinal ring into the labia; but dissection revealed the total absence of the components of the male organ, and consequently this irregularity belonged to the latter class, being nothing more than an extension or prolongation of the hypertrophied ovary of the right side discending, into the labia, which through excess of development had united in the mesial line.

The relation of the bladder to the vagina, its very short neck, the entrance of the urethra so high up into the vagina, the shape of the womb and the situation of its neck, the degenerated fimbriated extremity of the right fallopian tube given off behind the ovary, and the very large mammary glands, considering her age, are the only points differing materially from similar recorded cases. In other respects it is not very remarkable, there being no difficulty on examination in defining her proper sex, and in recognizing all the female genitals to the exclusion of the male organs.

ART. VII.—*An Examination of the "very striking facts" offered by Professor Hume, to prove the introduction of yellow fever into Mount Pleasant, during the Summer of* 1857. By R. A. KINLOCH, M.D., Physician attending the Surgical Wards of the Roper Hospital.

To fatigue the reader as little as possible, I shall not notice the truly dramatic portion of Prof. Hume's last article, but separate the so-called "facts" from the other matter, and put them in a compact form, so that the review of each of these may be readily followed. Should any suspicion attach to the manner in which I present the facts, the Professor's article can of course be referred to. I shall quote his language, except where condensation is required to economise space.

FACT No. 1. "Mr. Lofton lost an infant on the 24th of August, and the physician who attended has given his testimony that it was not yellow fever." * * * * I am truly sorry to have at the outset to expose a piece of ingenuity on the part of my friend which should not have emanated with a scientific man. It may be considered admissible for a lawyer at the bar to keep back such testimony as might injure his client, but in our profession such conduct is not excusable, even when indulged in for the high object of supporting a theory. True, the physician who attended the infant of Mr. Lofton did not believe that it died of yellow fever; nor did he believe that any of the Lofton family, or any of the Mount Pleasant people, died of that disease. He was not familiar with yellow fever, and was under the apprehension that bilious congestive fever was killing the people. I was the first to differ with him. The learned Committee, appointed to investigate the decease, soon differed with him also. Professor Hume at first differed from both the Mount Pleasant Physician and myself, and, in his capacity of Alderman, proclaimed me before the City Council of Charleston as an alarmist, because forsooth I had mistaken his "imitative fever" for genuine yellow fever. (This, by the way, was a "deed of justice and gener-

osity, and I must ask the Professor's pardon for not having before given him the credit of it. I was remiss, because the matter had no bearing upon the scientific points discussed; but as my friend complains of my injustice, I am forced to acknowledge his act.) But the Professor afterwards became a convert to my belief, and rejected the testimony of the physician above alluded to. Now, it would appear that he accepts the testimony of the physician in regard to Mr. Lofton's infant, but rejects it with reference to all the other cases dying at the village. And why? Because the Lofton child died before fever occurred on board of the San Juan, and before the vessel took her position on Cooper river, at the mouth of Hog Island Channel! The following extracts from a letter of the physician who attended the Lofton infant, proves my assertion, and demolishes the ingenious fabric which the Professor has been more than a year constructing. "If I said to Professor Hume in person, or before the Board of Physicians, that it was not yellow fever, I did not authorise the assertion that it was not the same disease that proved so fatal in the Lofton family. If the fact be established, that Mr. Lofton did die of yellow fever, it will be an equally established fact that Mr. Lofton's child died on the 24th of August of yellow fever.

Very respectfully, yours,

D. R. WILLIAMS.

And here I might properly close my argument as to the introduction of yellow fever into Mount Pleasant; for my opponent is routed on his own ground, and by the testimony which he himself brought forward as reliable. But the readers of the Journal will remember that in my article on the Mount Pleasant fever I made no use of the testimony in regard to Mr. Lofton's infant's disease, although it might have been urged, as my friend the Professor has shown, in support of my view of the question discussed. I was willing that Mr. Aldrich's case should stand as the first one of the epidemic, because I knew that the hemorrhagic condition, and the yellow skin, which Mr. A. presented towards the

termination of his sickness, would better dissipate the incredulity of those disposed to question the character of the disease which destroyed Mr. Lofton's infant. My position has not been changed. So it will not be improper for me still to treat the Professor as though he were not "*hors de combat*," and the opportunity will be thus afforded of exhibiting the value of his remaining facts, and, I think, of exposing his desperate efforts at special pleading.

FACT No. 2. "Mr. Aldrich died on the 6th of September, of yellow fever, by transformation from bilious remittent, or bilious congestive fever—the seizure of the last named disease occurring on the 28th of August, and the transformation becoming evident on the 3d of September. This was the first death from yellow fever." * * * * * This fact is established upon the testimony of Dr. Aldrich, which is endorsed by Professor Hume. Here are the important points extracted from letters put forward: "I was *informed* that he was worse at night." "The remissions were well marked, so much so that on two occasions his mother, who was his nurse, thought now that he perspired so freely, that he was recovering. I did not, nor do I now, consider him liable to contract a disease of such character," (alluding to yellow fever,) "as he had resided in the city the year previous." Prof. Hume's own language is: "He passed the ordeal of 1856, and may be considered as having been tried, and not found wanting." This looks very pretty and reasonable on paper; but every practitioner of any experience in Charleston is familiar with the fact, that there are undulations in the excitement of the febrile paroxysm of yellow fever, which often lead physicians to believe in remissions in this disease. It is a very common thing for the patient to seem worse at night; it is no less common for him to perspire freely at times. With Dr. Aldrich's view of his nephew's case, I presume he used quinine freely; and if so, my life upon it, the sick man must have perspired abundantly. The differential diagnosis between yellow fever and bilious remittent, cannot be made to rest upon the mere fact of apparent remissions. In every disease the diagnosis must be made out by a study of the

assemblage of symptoms, and their progress. There is, strictly speaking, no such thing as a continuous febrile paroxysm, if by such we are to understand that the degree of excitement is always the same; it is not seen in any of the so-called continued fevers. But Mr. Aldrich's attack, it is asserted, must have commenced as bilious fever, because he was not a subject for yellow fever, having passed through a sickly season in Charleston! The untenableness of such an argument is known to every old woman in our city. I am surprised that this vulgar knowledge did not penetrate even to the laboratory of the Professor of Experimental Science. The recorded instances are innumerable, where persons have passed safely through one or two epidemics, and finally succumbed to one of later date. I would like to show my friends, Dr. A. and Prof. H., my own note book, disclosing the occurrences of the last season. I think they would then agree with me, that we have to admit that even one attack of yellow fever does not always exempt from a second. But the singularity of Mr. Aldrich and Mr. Lofton's infant being attacked with bilious congestive fever, is remarkable, when we remember that none of these persons at the village, who were really acclimated to yellow fever, suffered from the other disease. And my friend, Prof. Hume's, opinion in this particular, is still more remarkable, when we remember that it is only a few months since (Charleston Medical Journal for September,) that he assented to the fact of Mount Pleasant being a healthy village—nay, he even affirmed that the place was "a Sahara desert in miniature," and that there could not be found "in this village, sufficient matter and moisture of its own production to initiate decomposition, either ligneous or herbaceous, adequate to produce a case of malarial fever." While this language was green in our memories, the Professor ought not to have contended in the same Journal that Mr. Aldrich was attacked at Mount Pleasant with bilious congestive fever, for he forces his best friends to the conclusion that his change of views was brought about just in time to aid his theory in reference to the fever being introduced by the San Juan. What, too, will his friends at the village think

of him! Imprecations were heaped upon my head for affirming that Mount Pleasant was not a safe place of residence, during the fall of 1857, for persons not acclimated to yellow fever. The Professor now affirms that the place is dangerous for any white person during the summer and fall months of every year. There still remains another singularity in the Professor's conduct to be noticed under this head. He contends that Mr. Aldrich finally died of yellow fever—a transformation having been effected—but he does not conclude this from Dr. Aldrich's testimony; for, be it known to the readers, Dr. A. considered his nephew's case bilious congestive fever throughout. The Professor here plays the lawyer a second time, and keeps back part of the evidence: a portion of Dr. Aldrich's testimony is so reliable that he hangs his theory upon it; the remainder he rejects as unreliable and worthless. I pass on to

FACT No. 3. "Between the 1st and 7th of September, there occurred two cases of yellow fever on board of the Spanish brig San Juan, moored in Cooper River, near the Western extremity of Hog Island Channel." This fact is correctly, stated, but when the inference is drawn, that the San Juan was an infected vessel, many might be disposed to object. The San Juan came from a healthy port, and was in Charleston harbor over forty days before the first case of fever occurred. My friend affirms that the two men got their fever by visiting the Ciscar, an infected vessel released from quarantine. If this be so, it might be affirmed that the San Juan was not infected, and not capable of extending disease. The greater number of good observers are inclined to believe that the yellow fever poison is not, like small pox, re-produced in the body suffering; that *patients* do not infect when carried to a city, or put in a hospital, or on board of a vessel; that the atmosphere from the hold of a vessel, and the vessel's cargo, are the common carriers of the agent, or, in other words, the means of extending the infection. This, I believe, is the opinion of the Health Officers of New York harbor, where such good opportunities have been lately afforded for investigation; this is the opinion of Dr. Stone, of New Or-

leans, and Dr. J. C. Nott, of Mobile, (see New Orleans Medical and Surgical Journal, for November, 1858). My own opinion is undecided in regard to this particular, so I will not further discuss the question. I have only alluded to the point to admonish my friend of his habit of drawing hasty inferences, and regarding his own belief as a fixed fact.

FACT No. 4. "The malaria of yellow fever may, according to observers cited, be wafted to distances as far as three, nine, and twelve miles; and here, the San Juan, only two miles from Mount Pleasant, was near enough to infect the town." * * * * We have to rely upon experience alone for our knowledge of the distance to which the yellow fever germs may be wafted. Our experience in Charleston harbor certainly contradicts the probability of these germs being wafted as far as one mile. The James Island shore has never suffered, although only a mile south of the quarantine ground. The south-eastern extremity of our city is not quite as far from the quarantine as is the James Island shore, and yet yellow fever is never known to commence its ravages in this quarter. But if the Professor now credits the case of Miss Taney, that of the St. Lawrence, and that of Mrs. Fox, as quoted from the Committee's report on the yellow fever, at Norfolk, it is singular that he has only done so when trying to establish the introduction of yellow fever into Mount Pleasant. The Committee's report was before him previous to his writing an article in the March number of this Journal; and yet, in that article, he could only venture to say, in reference to the extension of the yellow fever infection, that it "reaches distances of certainly half-a-mile, and probably more."

FACT No. 5. The circumstances favoring transmission of the infecting agent from the San Juan to Mount Pleasant, and opposing transmission from this vessel to the city, were these: "A tidal current of air accompanies every nightly ebb tide. The infected air, liberated from a vessel at the mouth of Hog Island Channel, during ebb tide, takes the course of the Channel, impinges upon the curvature of the shore fronting the south-west, where the village is situated,

and here divides, a portion mounting over the bluff of the village, and the remainder following the current to Rebellion Roads. A westwardly wind would have produced the same effects, irrespective of the tides. The heavy easterly wind which did blow on the 3d of September, must have passed with too much rapidity over the city to produce its fatal effects." * * * * This fact, the Professor no doubt thinks embodies the most profound philosophy. I am disposed, from the unimaginative character of my intellect perhaps, to regard it as full of poetry. The progress of the fever germs is described as though the Professor, standing on the Mount Pleasant wharf, saw them approach. "The infected air impinges upon the curvature of the shore, and *here divides.*"—Mark! careful reader—some of the germs, the most far-sighted and intelligent, I presume, leave the tidal current, and, now preferring to travel by the westwardly wind, proceed half-a-mile over a low mud-flat, then mount the bluff and come right upon Mr. Aldrich, a prepared victim. When the easterly wind blew, the Professor can't tell us if any of the germs were foolish enough to attempt to travel, for half-a-mile only, from the San Juan to the city; but this he knows to be certain, if they did embark, they were, like birds in a storm, blown—the Professor even, knows not where.

And now, I agree with my opponent, that controversy should have its limits. I am quite willing to let our little difference stop here, provided my friend is not disposed to revive it. I have repelled the attacks made upon my views and corrected, as I promised to do, the errors into which I conceived the Professor had fallen. I am willing to leave the question, as to the origin of the Mount Pleasant fever not to the people of the town, as my opponent suggests, for their opinion is a matter of indifference to me, but to the good sense of the profession at large. I am no partisan, but a searcher after truth. It would not astonish me to discover at no distant day, that my views must be modified, because I am convinced that as yet, we know but little of the question upon which, for ages, there has been so much disputation, and should not have entered the lists as a combatant, but

from clearly perceiving that the arguments of Professor Hume, at least, were altogether untenable. I conceive that he is the worse enemy that I know of to the doctrine of the contagiousness and the importation of yellow fever. If the doctrine is true, I would have it properly supported by well observed facts, so that those who believe may have good reasons for their faith. As yet, there is not a semblance of reason discoverable in any argument that has been advanced to prove the introduction of yellow fever into Mount Pleasant.

In parting from my venerable friend and opponent, with the kindest feelings, I have a high respect for his attainments in many useful directions. But while I respect him for his knowledge, generally, I of course do not profess the same feeling for some of his particular views. In criticising these, I trust that I have not exceeded proper bounds. I know that at times I have laughed, perhaps immoderately, at his expense, but in doing so, I cannot be accused of making use of a weapon other than what belongs to the critic, and if my opponent complains, I can only remind him that when one says "funny things," we are not always responsible for the exhibition of our risibilities.

REVIEWS.

ART. VIII.—*The History of Prostitution: its extent, causes, and effects throughout the world, (being an official report to the Board of Almshouse Governors, of the city of New York,) by* WM. W. SANGER, M.D., Resident Physician, Blackwell's Island, New York City, etc., etc., etc. New York: Harper & Brothers, publishers. 1858. 8vo., pp. 685. (Russell & Jones.)

Prostitution is a great social evil, so peculiar in its nature, so repulsive in its details, as almost to forbid its consideration. But however natural may be this reluctance, to enter into the discussion of a subject so full of painful evidences of the frailty of the human race, however much it may be in accordance with public sentiment to drop the curtain before this scene of vice and sin, it is an evil which has attained such a magnitude in some portions, at least, of this fair country of ours, that duty forces us to lay aside our scruples and the restraints of a delicacy, which, the longer it is entertained, will be the more injurious to the public health. If this is indeed a necessary evil, as many contend, and one which it is impossible altogether to remove, we would do well not to ignore its existence, but to make ourselves acquainted with its conditions, causative and consequent, that some measures may be taken to protect the innocent portion of the community against the evil results of which this vice is so productive. The diseases consequent upon the promiscuous intercourse of the sexes, are no longer confined to the culpable, but by a peculiar mode of contagion, the pure blood of many a virtuous woman is contaminated, and her system incurably vitiated with the poison of a treacherous and truly fearful malady; more, the innocent offspring of vicious parents, taking their vital fluid from impure streams come feeble and unhealthy into the world, either immediately

cursed with some ugly deformity, or sooner or later developing a hideous and repulsive disease; but nature it seems has protested against the propagation of such a degenerate species, and the majority of these children hide the vice and shame of their parents in an early grave.

Is it not then, but false modesty to ignore the very existence of a vice which has grown so bold, that it seeks its victims in the bright light of day and in our most frequented streets and crowded thorough-fares. Acknowledging, then the necessity of recognising the presence and progress of an evil, which, though it cannot be wholly eradicated, may, we believe, be so modified, and regulated by a careful public supervision, that its effects upon the innocent members of society may be rendered comparatively harmless, we announce the work of Dr. Sanger, which has just been issued from the press of the Messrs. Harper. We do not think that we are too sanguine in believing it must be productive of *some* good results.

The introduction is occupied with the reasons which led to the production of this volume, and the "special and local causes which led to its accomplishment in the present form." The object of the work is to expose to the public the vice which flourishes in their midst, to point out and investigate all the evils attending prostitution and to inquire into the best means to be adopted for its healthful modification.

Dr. Sanger is resident physician at Blackwell's Island, New York City. The Governors of the Alms House of the city and county of New York, "had assigned to him, with other duties, the medical care of all persons who had contracted infectious diseases in the practice of debauchery, and who required charitable aid, to restore them to health. The result was that a large number of persons, both male and female, chargeable to the citizens of New York, through the medium of the institutions on Blackwell's Island, came under their cognizance, and they became convinced that some measures were necessary in connection therewith." In the commencement of the year 1855, Mr. Townsend, President of the Board, submitted to its consideration a list of interrogatories on the

subject, which were adopted and transmitted to Dr. Sanger. We copy them from the volume before us.

1st. What proportion of the inmates of the institutions on Blackwell's Island under your medical charge are, in your opinion, directly or indirectly suffering from syphilis?

2d. Are or are not the number of such inmates steadily on the increase?

3d. Do not patients in the different institutions, particularly in the Penitentiary Hospital, often leave before the disease is cured, so that they are liable to infect other persons after their departure?

4th. Are not the offspring of parents affected with constitutional syphilis subject to many diseases of like character which cause them to become a charge upon the city for long periods of time, and often for life?

5th. What are your views in reference to the best means of checking and decreasing this disease, and what plan, in your opinion, could be adopted to relieve New York city of the enormous amount of misery and expense caused by syphilis?

6th. You will reply in full to the above queries at the earliest date possible?

The author found that to answer these questions it was necessary to extend his investigations beyond the limits of the institutions on Blackwell's Island. The result of these investigations we have in the latter third of the volume before us. In the first and largest portion of the work, from chapter 1st to 32d, is contained a history of prostitution in different countries of the world, and in different ages, and purporting to show what had been done to check this vice and prevent the propagation of the venereal disease.

Chapter 9th gives an account of syphilis, perhaps the severest penalty inflicted for the practice of vice. This chapter contains nothing new or worthy of special notice, and is a brief history of its rise and progress in England.

In chapter 10th is presented from the authority of Parent-Duchatelet, a review of the present condition of prostitution in France. Had we not been previously acquainted

with the able and interesting work of Duchatelet, we would certainly have been induced to look into its contents from the recommendation which Dr. Sanger has given the work, by the interesting details and information which he has transcribed from it. From this source is derived the number of prostitutes in Paris—their nativity, parentage, education, age, etc.,—causes of prostitution, and the present regulations regarding this evil, by which its practice may be somewhat decreased and the spread of syphilis lessened.

In the 32d Chapter, our author enters upon the main portion of his labour, viz: to expose the condition and state of prostitution in New-York, its evils, and to consider whether any means, and what, should be adopted for its modification. Having secured the co-operation of Mayor Wood, Dr. Sanger entered upon his task; and to facilitate the execution of his plan, a very comprehensive series of questions was prepared, which it was attempted to have answered by an aggregate of 2,000 prostitutes. These queries were printed on a large sheet of paper, with sufficient blanks for answers, and entrusted for prosecution to the different officers of Police. It requires but a perusal of the contents of this volume to see how well they executed their trust.

The first question is one of interest, viz: How old will you be next birth day? On reference to this table, it will be seen that "between the ages of 15 and 20 years are found about $\frac{3}{8}$ of the whole number (2,000) embraced in this return. Between the ages of 21 and 25, nearly $\frac{3}{8}$ more of the whole number are included, giving in the first ten years of the table (between the ages of 15 and 26,) three quarters of the aggregate prostitution, while the next period of five years, or from 26 to 30, contains $\frac{1}{8}$ more. It is thus upon record, that 7 out of 8 women who came under this investigation, had not yet reached 30 years of age." Many will undoubtedly be surprised to find so large a proportion of girls, and very young women, upon the list of prostitutes. This corresponds with the account of the Parisian courtesans—the half of whom are between the ages of twenty and twenty-six. But it can be well understood, upon a little consideration, what causes produce

so large a number of youthful prostitutes. Youth is, in such a profession, at a premium, and the large demand must be supplied. These wretched women do not long retain their youthful charms, the bloom of their cheeks soon fade, and their eyes soon lose their brightness and grow dim under the continual round of excitement, stimulation, and debauch, and they sink lower and lower in degradation and misery, until they grovel in the most disgusting and filthy haunts of sin, more debased than the vilest savage. It is thus that one of these women never holds for any length of time the same position, but descends from a brothel of one grade to that of a lower, in time, relative to the loss of her attractions. Thus it is, that the places left vacant by the descent, are being continually supplied, and in the upper grades of prostitutes there is a very large proportion of the young.

Youth, as Dr. S. remarks, is a marketable commodity, and when its charms are lost, they must be replaced. Here we give some of his cases substantiating this view: "C. B. is a native of New-York, and now resides in the eighth Police District of the City. She is twenty years old, and became a prostitute at the age of sixteen, through the harshness and unkind treatment of a step-mother, her own mother having died when she was an infant. S. B. was born in Vermont; her father died while she was a child. At the age of fifteen she was enticed to the city, and became an inmate of a house of prostitution. She is described as an intelligent, well educated girl, of temperate habits." The following example is taken from the fourth Police District, the antipodes of the former locality, and exhibits forcibly the operation of this gradual deterioration: "E. S. was seduced in Rochester, N. Y., at the age of sixteen. She accompanied her seducer to this city, and for a season lived here in luxury. She was finally deserted, and now drags out a wretched existence in Water street."

"From the fact," says the author, "that youth is the grand desideratum, it is evident that a constant succession of young people will be driven into this arena, either by force or treachery. *The average duration of life among these women*

does not exceed four years from the beginning of their career! There are, as in all cases, exceptions to this rule; but it is a tolerably well established fact, that one-fourth of the whole number of abandoned women in this city die every year. Thus, by estimating the prostitutes in New-York at six thousand, (and this is not an exaggerated calculation, as will be proved hereafter,) the appalling number of one thousand, five hundred erring women are hurried to their last, long home, each year of our existence."

The next question is an inquiry as to the number of prostitutes furnished by the different States of the Union. Of the 762 women enumerated in this table as natives of the United States, a very large majority is furnished by the Northern portion of our country. Thus, 1st. The Eastern District, containing Maine, New-Hampshire, Vermont, Massachusetts, Connecticut, and Rhode Island, contributes one hundred and seventy-two women to the prostitutes of New-York city. 2d. The Middle States, New-York, New-Jersey, Pennsylvania, District of Columbia, Delaware, Maryland, and Virginia, contribute five hundred and sixty-six women. 3d. The Southern States, North and South Carolina, Georgia, Alabama and Louisiana, contribute *twelve* women. 4th. The Western States, Ohio, Illinois, Missouri, and Kentucky, contribute also *twelve.*

Dr. Sanger asks on what hypothesis can these proportions be explained? Maine sends 24 women from her population of 580,000, while Virginia, with 1,421,000 inhabitants, contributes but nine. These States being equally distant from New-York, the difference in favor of Virginia cannot be explained on the ground of distance. "The case is still further involved," as the Dr. correctly observes, "by the fact, that in all Southern cities the majority of prostitutes are from the North; and it is a well-known circumstance, that at certain periods large numbers of courtesans from New-York, Boston, and other cities, emigrate Southward." We think that the explanation of this great disproportion, so much in favor of, and to the credit of the South, must be looked for

in the different condition of her Society, which imposes, we believe, greater personal responsibility for the actions and severer penalties for the offences of its members, and in the dissimilarity of her institutions and pursuits.

The ninth question refers to the education of prostitutes, and its answer is very significant, as showing the great ignorance of this unfortunate class of women.

In the table which contains the answers to the question—what was the cause of your becoming a prostitute?—out of a total of 2,000, 513, or more than one-fourth, answered, "Inclination."

Of this reply, the author remarks, "that if these representations were borne out of facts, it would make the task of grappling with this vice a most arduous one, and afford very slight grounds to hope for any amelioration; but it is believed that the circumstances which induced the ruin of most of those who gave the answer, will prove that, if a positive inclination to vice was the proximate cause of the fall, it was but the result of other and controlling influences." And a careful analysis of this answer, in many instances, goes to confirm this supposition. Destitution is given as the immediate cause of prostitution by 525. We transcribe the following case in the words of the author:

"During the progress of this investigation in one of the lower wards of the city, attention was drawn to a pale but interesting looking girl, about seventeen years of age, from whose replies the following narration is condensed, retaining her own words as nearly as possible. 'I have been leading this life from about the middle of last January, (1856). It was absolute want that drove me to it. My sister who was about three years older than I am, lived with me. She was deformed and a criple from a fall she had when a child, and could not do any hard work. She could do a little sewing, and when we both were able to get work, we could just make a living. When the heavy snow storm came our work stopped and we were in want of food and coals. One very cold morning, just after I had been to the store, the landlord's agent called for some rent we owed, and told us that if we could not pay it, we should have to move. The agent was a kind man, and gave us a little money to buy some coals. We

did not know what we were to do, and were both crying about it, when the woman who keeps this house (where she was then living,) came in and brought some sewing for us to do that day. She said that she had been recommended to us by a woman who lived in the same house, but I found out since that she had watched me, and only said this for an excuse. When the work was done I brought it home here. I had heard of such places before, but had never been inside one. I was very cold, and she made me sit down by the fire, and began to talk to me, saying how much better off I should be if I would come and live with her. I told her I could not leave my sister who was the only relative I had, and could not help herself; but she said I should be able to help my sister, and that she would find some light sewing for her to do, so that she should not want. She talked a good deal more, and I felt inclined to do as she wanted me, but then I thought how wicked it would be, and at last I told her I would think about it. When I got home and saw my sister so sick as she was, and wanting many little things that we had no money to buy, and no friends to help us to, my heart almost broke. However, I said nothing to her then. I laid awake all night thinking, and in the morning I made up my mind to come here. I told her what I was going to do, and she begged me not, but my mind was made up. She said it would be sin, and I told her that I should have to answer for that, and that I was forced to do it, because there was no other way to keep myself and help her, and I knew she could not work much for herself, and I was sure she would not live a day if we were turned into the streets. She tried all she could to persuade me not, but I was determined and so I came here. I hated the thoughts of such a life, and my only reason for coming was that I might help her. I thought that if I had been alone I would sooner have starved, but I could not bear to see her suffering. She only lived a few weeks after I came here. I broke her heart. I do not like the life. I would do almost any thing to get out of it, but now that I have *once done wrong*, I cannot get any one to give me work, and I must stop here unless I wish to be starved to death.'"

This is the history of many of these unfortunate women who are driven to this dreadful mode of life by utter want and destitution. It is thus thought probable that from 500 to a 1000 women were added to the prostitutes of New York, by the panic and financial difficulties of '57—58. Other cases

are here related of the same nature as the above. Among the other causes assigned as determining this mode of life, are the following, viz.: Seduced and abandoned—drink and the desire to drink—ill treatment of parents, relatives or husbands—as an easy life—bad company—persuaded by prostitutes—too idle to work—violated—seduced on board emigrant ships—ditto in emigrant boarding houses. It requires only a little reflection to perceive the power of such causes to produce evil results in the minds of weak and ill-educated women.

Chapter 35th contains information concerning the manner of life among prostitutes—details the various grades of prostitutes, and the great diversity in the style and splendor of the houses of prostitution; in regard to the last, the readers of this volume will undoubtedly be startled at their tremendous expenses. "In one house in this city, the enormous sum of nine thousand one hundred dollars ($9,100) is, or was at the time of examination, paid annually for rent and use of furniture; the owner being a woman who formerly kept the place, but who is now living in the enjoyment of a large income in one of the Italian cities."

Many other houses are mentioned, the rents of which range from $500 to $1,500 or $2,000.

This fact, aside from details, too numerous to mention in a limited review, will give some idea of the large amount of capital invested in the support of this vice.

But the most mortifying facts, and those which illustrate most forcibly the moral depravity of New York, are those which are embraced in the portion of this chapter relating to houses of assignation, places of meeting for individuals of the opposite sexes, for the purpose of illicit intercourse. Many of these houses are said to be situated in the most fashionable streets of the city. "Indeed so trustworthy is the authority," says Dr. Sanger, "that it may be confidently asserted that even Fifth Avenue and Union Square, are not exempt from these resorts." No public women reside, or are even admitted into these houses, which are said to be patronized *exclusively* by women of the upper walks of life

and from the fashionable world, the men being of "all sorts of business."

In the 36th chapter, the extent, effects, and cost of prostitution, are considered very fully. Under the first head come the number of prostitutes, and amount of rents for the practice of this sin. Here is a synopsis of the result of some of these inquiries.

Houses of prostitution, 378. Houses of assignation, 89. Dancing and lager beer saloons, where prostitutes congregate, 151.

The number of erring women in New York is estimated as follows:

Known public prostitutes, 6000. Women who visit houses of assignation for sexual gratification, 1,260. Ditto, to increase their income, 400. One-half the number of kept mistresses, assuming the other half to be included in those who visit houses of assignation, 200. Total, 7,860.

Those coming under the head of visitors to houses of assignation, cannot be considered in a majority of instances as prostitutes; their position is very different from that of public women, whose intercourse is promiscuous, and who sell themselves to the highest bidder. In the case of the former, the promptings of virtue and honor are disregarded in most cases, it is here asserted, from the force of affection and *sexual passion.* We have not ourselves much fear of the potency of the last cause assigned to induce a woman to sin unassisted by other circumstances.

The most direful effect of prostitution, the propagation of a loathsome and dreadful malady, syphilis, is considered in the next place. It is estimated from various sources of information that the number of cases of venereal disease occurring annually in New York, exceeds 74,000.

The last chapter is occupied with some considerations concerning the medical measures to be exercised, but we have not space for further notice of the contents of this volume, and will merely transcribe here a recapitulation of the facts substantiated.

There are six thousand public prostitutes in New York.

The majority of these are from fifteen to twenty-five years of age.

Three-eights of these were born in the United States.

Many of those born abroad came here poor to improve their condition.

Education is at a very low standard with them.

One-fifth of them are married women.

One-half of them have given birth to children, and more than one-half of their children are illegitimate.

The ratio of mortality among children of prostitutes is four times greater than the ordinary ratio among children in New York.

Many of these children are living in abodes of vice and obscenity.

The majority of these women have been prostitutes for less than four years.

The average duration of a prostitute's life is only four years.

Nearly one-half of the prostitutes in New York admit that they are, or have been sufferers from syphilis.

Seduction, destitution, ill-treatment by parents, husbands or relations, intemperance and bad company are the main causes of prostitution.

Women in this city have not sufficient means of employment.

Their employment is inadequately remunerated.

The associations of many employments are prejudicial to morality.

Six-sevenths of the prostitutes drink intoxicating liquors to a greater or less extent.

Parental influences induced habits of intoxication.

A professed respect for religion is common among them.

A capital of nearly $4,000,000 is invested in the business of prostitution.

The annual expenditure on account of prostitution is more than $7,000,000.

Prohibitory measures have signally failed to suppress or check prostitution.

A necessity exists for some action.

Motives of policy require a change in the mode of procedure.

We have said enough we believe to show that this work of Dr. Sanger is full of interest and much information, and worth the perusal not only of the profession, but of those who have the care and responsibility of the public weal. If more minute details are desired to show the extremity to which vice can be carried, and the depth of degradation to which not only the weaker sex, but even the male members of the human family can descend, they are to be found in the more comprehensive work of Parent Duchatelet, in which such modes of animal gratification are recorded as are too gross and repulsive to be here named.

We will suppose that details of a similar character are omitted in the volume under consideration from the fact that these unnatural indulgences do not exist to any material extent in New York. It is to be hoped that that *perfection of vice*, so common in the French capital and other large cities of Europe, has not yet been attained in this country.

Trusting that this partial outline of Dr. Sanger's work on prostitution, will induce some of our readers to look into it, we must be allowed to say that it would have been more acceptable had its literary pretensions been more modest.

In its very cursory and brief notices of prostitution among the ancients, the appalling foot-notes, with which each page is loaded, are scarcely necessary, and wear an air of pedantry which had better been avoided.

There is another point too on which we cannot reflect but with expressions of disapproval.

In speaking of "houses of assignation," Dr. Sanger has not only assumed a great responsibility (we conceive) in the assertion that there are 89 of such resorts in the city of New York, and that many of them are situated in the most fashionable portions of the metropolis, but has done we fear decided injury in the details he has given, of the facts, if such they be, which he has laid bare to the public, concerning places so detrimental to the morality of society.

Many young persons before scarcely aware of the very existence of such resorts, are made conversant with their whereabouts, and are initiated into the modes of procedure to be pursued, to insure secresy and immunity in the commission of crime.

Facilities have thus been afforded which we are afraid may increase the number of faithless wives and dishonored daughters in New York, illustrating forcibly the maxim "where ignorance is bliss 'tis folly to be wise."

ART. IX.—*The Uræmic Convulsions of Pregnancy, Parturition, and Childbed.* By DR. CARL R. BRAUN, Professor of Midwifery, Vienna. Translated from the German, with notes, by J. MATTHEWS DUNCAN, F. R. C. P. E. New York: S. S. & W. Wood. 1858; one vol. 12mo., pp. 182. (McCarter, Dawson & Co.)

The above volume consists, as the Editor tells us in his preface, of the translation of a single chapter of Dr. Braun's new text-book of midwifery, which was published in Vienna, in 1857. The copy before us is a reprint from the Edinburg Edition which Dr. Duncan was encouraged to put out, in consequence of the favorable reception which the translation met with when it first appeared in the *Edinburg Medical Journal*, for 1856-57. Its uniqueness alone, will secure it attention, even should it possess no other attraction, and the deficiency of any similar treatise in our language, will create for it, we doubt not, an extensive circulation among those who are interested in the organic chemistry of pathology, and who have not the good fortune to be able to refer to the great German sources of information upon this special topic. The advantage derivable from a knowledge of the first, and the fullness and exactness of the latter have seldom been better illustrated than in the very valuable monograph which forms the subject of our notice, and which contains the most complete and lucid exposition yet offered us of a subject

which is at present, as the English editor truly remarks, "one of the most prominent in the whole range of practical medicine, and in connection with which so much research is now going on, as to render it impossible to produce a perfect account of it."

The translator has, so far as accuracy and verbal faithfulness are concerned, apparently acquitted himself creditably, but this has been accomplished at the expense of ease, grace, and clearness. The style is cramped, stiff and un-English, and sometimes the sense is obscured by the too exact rendering of the foreign idiom, and occasional inversions whose curiosa infelicitas we have seldom seen equalled. To use a metaphor, the limpidity of the stream is diminished by the quantity of foreign matter which it holds in solution, and the current frets by or creeps round, instead of sweeping over, the obstacles that may impede, but should not baffle, its onward flow.

The subject of puerperal convulsions, by whichever of its many synonyms the disease may be called, has been involved in much obscurity, by the confused and inaccurate classifications of its numerous historiographers. The violent crampings of the stomach, the obstinate vomitings, palpitations of the heart, etc., the hysterical, tetanic, epileptic, cataleptic, and even-apoplectic attacks which have been observed during pregnancy and parturition, and which occur as co-incidently but not intimately associated with labor, have all been classed under this common head. They have been considered as varieties or forms of puerperal eclampsia, though presenting the same symptoms as when arising in the non-gravid state, and are treated of as such by some of our best writers, and in some of the most classic works upon this subject. They will be modified of course by the condition of the pregnant female, and expose the patient to additional danger, but "the hysteria," as Cazeaux sensibly remarks, "does not thereby become less an hysteria, and the tetanic convulsion has not the less its characteristic persistence." They are evidently different and distinct affections. We like the term uræmic eclampsia therefore, as defining the special class of convulsions

meant, without committing us by any implication that they only occur in parturient women.

Eclampsia puerperalis, says Dr. Braun, "is an accute affection of the motor function of the nervous system (an acute neurosis of motility), characterized by insensibility, tonic and clonic spasms, and occurs only as an accessory phenomenon of another disease, generally of Bright's disease in an acute form, (diabetes albuminosus, nephritis diffusa seu albuminosa) which under certain circumstances, spreading its toxæmic effects on the nutrition of the brain and whole nervous system, produces these fearful accidents. The toxæmia (or blood-poisoning), in eclampsia gravidarum, parturientium et puerperarum, is commonly produced by uræmia i. e. by a change of the urea which has been retained in the blood, or by retention of excrementitial extractive matter of the urine. Hence according to the present state of our knowledge true eclampsia during pregnancy is designated urœmic, without implying that it is peculiar to pregnancy or child-bearing, because the same disease, with similar phenomena, may manifest itself also in women not pregnant, in children, and, even in males under certain circumstances favorable to it."

General convulsions—eclampsia properly so called—constitute by no means a common disease. Velpeau did not meet with a single case in a thousand labors, but although this proportion was accidentally small, in a list of 38,306 cases of labor, tabulated by Cazeaux, from the records of the principle accoucheurs of Great Britain, only 79 cases of convulsions are reported. But under this common appelation, as above remarked, several pathological processes have been comprehended which do not even present an identical series of symptoms. As the eclampsia arising from the defective elimination of carbonic acid from the lungs, or bile from the blood, or the retention of excreted matters, or the non-secretion of others; from meningitis, hyperæmia of the brain, spinal cord or medulla oblongata, and peripheral or centric irritations of the cranio-spinal axis. A chemically and histologically changed condition of the blood-serum and corpuscles as hydræmia, lukæmia, etc.; uræmia after large

losses of blood; many mineral, animal and vegetable poisons; the inhalation of carbonic acid, all have the power of producing conditions similar to true "eclampsia."

This affection is unusual in the early months of pregnancy; as a general rule, convulsions do not occur prior to the sixth month, but are particularly frequent during parturition, and appear most commonly from a few hours to a few days after delivery; though we have examples of their postponement for ten and twelve days. No one who has ever met with a well pronounced case will be likely to forget or confound its distinguishing features. The quick repetition of the fits, the complete insensibility during their continuance, and in the intervals, the swollen and injected face, the wild, staring, prominent eye-balls, the protruded tongue, the distorted and foaming mouth, and incessantly convulsed limbs, make it one of the most frightful and painful of all the sad spectacles which it is the daily lot of our profession to witness.

Sometimes the respiration ceases for many seconds, the veins of the neck and face are distended, the latter are of a dusky livid hue, all the respiratory muscles are contracted from spasm, and in consequence of this asphyxia may ensue. The urine and fæces are passed involuntarily, the skin is dry, or moist and clammy, and the pulse sometimes frequent, sometimes slow. After this group of symptoms, the patient lies comatose and motionless, the limbs are rigid, the respiration is frequent, laborious, stertorous, afterwards slower and snoring, and there is usually absence of consciousness and sensation. The duration of each fit is from a half hour to one or two hours, and a remission may occur, or the paroxysm terminate in death. The fits are repeated several times a day, sometimes as often as seventy. Generally, after a few fits, complete unconsciousness supervenes, and this continues till recovery or death. Such is a bold outline of the graphic picture which Dr. Braun has drawn in the opening chapter; the conclusion of it is devoted to an examination of the chemical and histological elements of the urine in women thus affected.

In Chapter Second, the pathogenesis of eclampsia is

discussed. The coincidence, at least, of albuminuria and this disease, has been observed by many distinguished physicians, our author among the number, who, in 1851, simultaneously with Frerichs, published his belief that the eclamptic convulsions of women during pregnancy were identical with the fits of adults in general, that are produced by uræmia in the course of acute Bright's disease; a hyperæmia affecting one or both kidneys, caused by conjestion of venous blood, is the primary stage of the latter malady. This is soon followed by an extravasation of fibrin into the malpighian capsules; the albumen passes off with the urine, the fibrinous matter coagulates in the tubuli of the cortical substance, and is finally discharged with the epithelium in the form of rectilinear tubes. By this inflammatory process passing through its various grades, the organ becomes gradually atrophied, the renal tubuli collapse, the volume of the kidney is reduced, and portion after portion of the nutritive apparatus is obliterated. But Dr. Braun is aware that numerous experiments have shown that urea may exist in the blood in large quantities, without giving rise to convulsions; and he quotes Frerichs, with whose views upon this point he thoroughly coincides, in support of the theory that the phenomena of uræmic intoxication are not produced by urea, nor by any or all of the excreted matters of the kidney, but by the transformation of the urea accumulated in the blood into carbonate of ammonia. Hammond has, on the contrary, recently assured us that he was unable to detect this substance by the most careful investigations, and he is of opinion that the soundness or disease of the kidneys affect the question of uræmic poisoning essentially; that at all events urea is not converted into carbonate of ammonia under any circumstances.

The almost constant presence of albumen, above noticed, in cases of eclampsia, leads us naturally to suppose a relation between them no less intimate than that of causality. But the presence of albumen in the urine, is only a sign of a local or systemic lesion, and may and often does occur without being followed or attended by convulsions. This is by no

means uncommon in pregnant women. Of twenty cases mentioned by M. M. Devilliers and Regnault, eleven only were affected by them, and these observers only tested the urine of dropsical women, while it is certain that many cases of albuminuria are not accompanied by infiltration. Moreover, it is generally agreed that the pressure of the enlarged uterus may give rise to it, though we not unfrequently meet with œdema from this cause, when no albumen can be detected in the urine. But the experiments of Mayer on Rabbits leave no doubt that ligation of the renal veins, and sometimes pressure on the inferior vena cava, by increasing the hydrostatic pressure of the blood, dilates the renal capillaries, and albumen is always found after such ligation in the urine collected from the ureter on the side operated upon. Now, M. Rayer is of opinion that the compression of the uterus by the renal vein, may eventually produce hyperæmia, and inflammation of the kidneys; Frerichs, on the other hand, strenuously denies that the exudation proceeds from this cause, nor will he admit the existence of inflammation proper. We think, therefore, that M. Blot is strongly sustained in his opinion that the two morbid conditions, albuminuria and eclampsia, are concomitant, but not consequential; but the first is certainly a valuable sign, characteristic, if not pathognomonic, of local or general lesions not easily detected, while the presence of albumen is readily discovered, and if found, should give rise, under the circumstances, to strong suspicions of an organic lesion that may give lead to eclampsia.

Chapters 3 and 4, discuss very briefly the connection between eclampsia and the pains of labor, and the influence of the first upon the life of the fœtus. Dr. Braun maintains that pain is not the cause, but the effect of the convulsion. Even with the most violent eclampsia, sometimes, no pains are observed, the fits may cease, and pregnancy go on for weeks; or during their continuance, complete absence of pain may be remarked, and the patient die before labor is ended. With the disappearance of uræmia, he asserts, the pains often become weaker; without uræmia, the most violent contractions produce no eclampsia, if the uræmia is removed during

pregnancy or labor, the later or stronger pains are not followed by fits. When the fœtus dies during pregnancy, or labor, if the mother presents uræmic symptoms, our author attributes its destruction to the poison of urea, and finds the cause of its death in the passage of carbonate of ammonia into its circulation; the presence of this substance he asserts can be demonstrated immediately after the birth, if it be born dead. Without denying this statement, or absolutely rejecting the ætiology, we are rather inclined to the opinion of Kiwisch, who supposes that the circulation in the maternal vessels of the placenta during the fits, is part of the cause of the death of the fœtus. Especially should we adhere to this view, if the pains had been inordinately violent, the position transverse, the cord unusually large or pressed on, or the waters prematurely discharged. If the child was well formed, of good size, and yet markedly anæmic, we would not hesitate to adopt the latter as the correct pathology of the case.

Chapters 5, 6 and 7, examine controversially and in detail, the ground already mapped out, as occupied and defended by the author, in regard to the etiology and pathology of the malady discussed.

In chapters 8 and 9, we have a vivid, compact, and skilful grouping of the symptoms upon which we may erect a differential diagnosis between uræmic convulsions, and the affections before pointed out by us as liable to be confounded with it; and the signs from which to draw a favorable or unfavorable prognostic.

Its fatal character depends principally on the following circumstances, which are always to be regarded as indications of profound import and danger:

"*a.* From its commencing during pregnancy, or at the beginning of labor.

b. Upon the occurrence and continuance of complete unconsciousness during the intervals of the paroxysms.

c. Upon extraordinary restlessness and exalted reflex sensibilities in the intervals.

d. Upon deficiency or gradual weakening of the pains, or from their insufficiency in the period of dilatation.

e. Upon the pulse quickly rising in frequency, and on œdema of the lungs extending after evey attack, and the dyspnœa, produced by it.

f. Upon serous exudation in the brain, effusion into the ventricles, or apoplexy, with its consequent hemiplegia, coming on during the fits, in consequence of secondary hyperæmia of the meninges.

g. Upon extensive dropsical effusions."

The rules of treatment, laid down in the last chapter are, in accordance with the pathology of Dr. Braun, directed towards the removal of the local lesion, of which he considers eclampsia to be a result. They are, first, those adopted by most practitioners in the management of Bright's disease—the use of vegetable acids, generous diet, and iron to improve the general tone of the system, of benzoic, tartaric and citric acid to neutralize the carbonate of ammonia, cathartics to obviate congestion of the head, diluents and certain mineral waters to increase the current of fluid proceeding from the malpighian bodies, and wash away the clots from the tubuli, with such restrictions and alterations as complications may require ; and, secondly, the medical and obstetrical treatment, laid down in all the modern text books and usually adopted.

The appendix contains an interesting account of two cases of chorea in pregnancy, (chorea gravidarum) reported by the translator, and published in the "Edingburg Medical and Surgical Journal," for January, 1854.

BIBLIOGRAPHICAL NOTICES.

ART. X.—*Pleiocene Fossils of South Carolina.* By FRANCIS S. HOLMES, A.M., Professor of Geology and Palæontology, and Curator of the Museum of Natural History, College of Charleston, S. C., etc., etc. Charleston. Russell & Jones: 1858. Quarto. No's. 1, 2, 3, 4 and 5.

With his characteristic industry and punctuality, Professor Holmes, in the five numbers of the work before us, has presented the public with the first fruits of his determination and promise, should the State continue her patronage, to complete the publication of the Tertiary Fossils, by describing and illustrating the interesting forms that have been discovered in the post-pleiocene and eocene formations.

The series, as we have before had occasion to mention, was commenced under the joint labors of Professors Holmes and Tuomey. The latter had received from Governor Hammond the well merited compliment of being appointed State Geologist of South Carolina, and prepared in this capacity the very able report on the Geological survey of the State which was published in 1848. Owing to some change of plan on the part of the committee in charge of the work, the very essential plates, at first designed to accompany the work, and the fossils for which had been actually arranged and prepared, were omitted. This omission occasioned general disappointment, and the solicitation of a number of our most distinguished savans, determined the above named gentlemen to attempt the publication of their collections at their own risk. Before the first series was finished, Prof. Holmes was deprived of the valuable assistance of his collaborator, who

died in 1857, and the conclusion of the work, commenced under such favorable auspices, devolved upon himself alone. How well he has performed it, the unanimous and cordial approbation of his brother naturalists best testifies.

The geological deposits of our State are, as is well known, unsurpassably rich and rare, and to their study and illustration our author has brought an enthusiasm, patience, and industry rarely surpassed; with what admirable results these have been indulged and exercised, his work is the highest proof. A hazardous experiment at the best, involving the expenditure, in no small degree, of time, labor and money, certain to be unremunerative, and worse still, perhaps, unappreciated, its very projection presupposes, and its successful progress proves the possession on the part of the author of qualities seldom united, except in the man of science, intense love of his pursuit, the capacity for severe labor, perfect self-confidence, and abnegation, and resolute will.

Of the artistic and mechanical execution of the work, it is impossible to speak extravagantly. Mr. Platen, the lithographer, has maintained fully his reputation as ranking among the very first in the country, and Messrs. James & Williams—whose imprint our Journal also bears—have turned out a specimen of typography and press-work, of unsurpassable excellence.

We wish Mr. Holmes every success in his most laudable and valuable undertaking, and sincerely trust that a liberal patronage may enable him to continue and conclude, if it cannot reward him for, his good work.

ART. XI.—*A course of Lectures on Obstetrics: delivered at St. Mary's College, London.* By WM. TYLER SMITH, M.D., with an Introductory Lecture on the History of the Art of Midwifery, and copious practical Annotations. By A. K. GARDNER, A.M., M.D., etc. Illustrated by 212 Engravings. New York: 1858. Robert M. De Witt, Publisher. 8vo., pp. 760.

The reading medical public is well acquainted with the lectures which form the basis of this volume. They appeared some years ago, seriatim, in the pages of the Lancet, were thus widely diffused and as widely studied and admired. Early in the present year, they were collected, arranged and emended, and published in a single volume by a London house.

Previous to the issue of this edition, Dr. Gardiner had been engaged in fitting them for the press, but owing to embarrassments and delays of various kinds, the American edition was postponed from time to time, and its progress, when nearly completed, was at last arrested by the appearance of the English edition, which contained several new chapters and illustrations. This necessitated some important alterations which retarded its issue materially, but we doubt if it has lost any thing in intrinsic value by the delay, and it is certainly as welcome now as it would have been a year ago.

Dr. Smith's reputation is committed to the value and trustworthiness of this work, the result of his mature reflections and experience, and it gives ample proof of the author's valid claims to the large and brilliant reputation which he has won in this department. He has given us a volume at once condensed and full, complete without being voluminous, exhibiting on every page the varied and accurate scholarship, the sound judgment, the practical skill and original force, for which he is so justly esteemed.

As a manual for students, and an available work of reference for general practitioners, we know no text-book that

approaches it, and recommend it therefore without a single reservation to the respectful attention of both classes.

Dr. Gardiner has somewhat enhanced its value by useful notes added here and there, and his introductory Chapter contains a lucid, succinct, and very pleasant sketch of the rise and progress of obstetrics as an art. The stately and solemn tread, "velut qui Junonis sacra ferret," which befits the historian, is somewhat disturbed towards its close, however, by a hasty and rather undignified rush at the homœopaths and female doctors, from neither of which classes we think our profession has much to fear.

The general execution of the book does much credit to the publisher, but we regret to add that the wood cuts with which it is "copiously illustrated," are of the commonest order. Both as regards practical and æsthetic value, they had much better have been omitted.

ART. XII.—*Selections from Favorite Prescriptions of Living American Practitioners.* By HORACE GREEN, M.D., L.L.D., President of the Faculty and Emeritus Professor of the Theory and Practice of Medicine in the New-York Medical College, etc., New-York: WILEY & HALSTED, 1858, 8vo., pp. 202.

Most of these prescriptions have already appeared in the American Medical Monthly, and many of them have been re-printed in numerous Journals throughout the country. The Editor has collected them from various quarters, and numerous individuals, through a long series of years, having been in the habit of requesting the large number of Physicians who have visited his rooms to witness, we suppose, his dexterous management of the probang, to leave behind them a memento in the shape of such favorite combinations of drugs as they have been in the habit of using in special diseases. In this way he collected two large manuscript

volumes of prescriptions, and the present work is a well sifted digest of the aforesaid folios.

The book may have its uses, and will be most specially valuable in its suggestive character, but it will be more powerful for evil, if used carelessly, or wherever it falls into incompetent hands. We look upon it as a dangerous weapon, inoffensive in the grasp of an expert, but deadly if toyed with by the ignorant and reckless. Heaven knows, "valuable formulæ" are abundant enough. Dispensatories are not the least bulky of our medical furniture, and sound principles, scientific laws, are more valuable than "practical directions," or rules of art; it is upon the first alone that any rational medicine can be founded. We advise the student to handle this book as he would hot iron; the old practitioner may examine it more carefully, and make a note or two from its pages of substantial value. The latter, however, will, we think, in the majority of instances, have either arrived at that stage of case-hardened skepticism which will regard the book with an incredulous curiosity not likely to put its formulæ to the test, or be so wedded to his own favorites as to turn a deaf ear to the praises others may lavish upon their own, "charm they never so wisely."

ART. XIII.—*Brief Exposition of Rational Medicine, to which is prefixed the Paradise of Doctors, a Fable.* By JACOB BIGELOW, M.D., Physician of the Massachusetts General Hospital, etc. Boston; Philips, Sampson & Co. 1858. 12mo., pp. 69.

This wise and most admirable brochure is fittingly dedicated by its author to Sir John Forbes, the great English champion of Rational Medicine, who, for nearly half a century, has lent the powers of his rigorously trained and acute mind to the establishment of a sound medical philosophy; who has investigated, with a sagacity rarely surpassed, the claims of

nature as our great nurse and physician, and combatted the baseless pretensions and boasted powers of drug-givers and drugs with a zeal, honesty, and success, never equalled. In this field, Dr. Bigelow has himself already been a hard worker, and has had the rare gratification of enjoying the matured fruits of a seedling planted long years ago, when soil and climate appeared equally uncongenial to its nurture or developement. We refer to the paper on Self-limited Diseases, read by him twenty-three years ago to the Medical Society of Massachusetts, and afterwards incorporated with others of striking value in a work entitled "Nature in Disease." It did then, and has since done, more, we believe, toward establishing something like a rational method—negatively at least—in the treatment of those maladies, than any other single effort; it was, indeed, so far as we are aware, the first bold and uncompromising denunciation—just and proper eminently—hurled against that pernicious diligence, that meddlesome care of uncontrollable diseases, which physicians practiced and patients clamored for. All honor to Forbes, Bigelow, Gould, Cotting, Hooker, and their co-laborers every where, who have made so early and well a stand against the progress of error and darkness, taught us that medical, like all other truth, is to be discovered by a reverent and humble attention to the lessons nature preaches, not in headstrong opposition to, or blind defiance of, her powers and actions; in obeying, not commanding her; in following, not forcing her divinely appointed steps; shown us how and where to look, and made us resolute "to strive, to seek, to find, and not to yield."

The fable prefixed to the argument in the book before us, and entitled the Paradise of Doctors, is a "springe for woodcocks," and well managed. The tale runs, that once upon a time, in the quiet old State of Massachusetts, a conviction fell upon every man, woman and child, in the great Bay State, that he, she, or it, should, in sickness or health, proceed to take thrice as much medicine as they did before. The result of this new revelation was an awakened activity in every branch of industry connected to the remotest degree with

the great art of drug-giving. The rattling of doctor's wheels, and the braying of mortars, shook the cities; every available laborer was converted into a nurse; physic was the conversation, amusement, luxury, occupation, news, atmosphere, and food of the day. Bile was the watch-word. Clergymen inveighed against it, politicians denounced it, doctors lived on it. Everybody had it; no two were agreed what it was, what it meant, what it did, but they haranged, wrangled, disputed, and fought about it. Physicians grew rich, every body else poor; they were the idols, these the worshippers who bowed down to images made with pills.

But now, doctors hearing of the success of their brethren in Massachusetts, flocked from every quarter, followed by their long trains of apothecaries, nurses, messengers, etc., etc. The market was glutted, pills no longer rolled their rollers to fortune, makers of potions became portionless, and medicines were a drug in the market. The revival, however, rose, and the people sank; the drugs were doubled in quantity and quality, and they died the faster; still audiences were harangued, spirits were evoked in testimony, and the brethren admonished to be faithful even unto death.

At last, one day a meeting accidentally took place between two ship-masters, one of whom had lost his barrel of beef, and the other his box of boluses, over-board, in a gale of wind. In a comparison of the condition of their crews, the contrast was so marked between the ruddy faces of the beef eaters and the lantern jaws of the pill takers, that a general mutiny sprang up against the further tolerance of the physic swallowing part of their duty. The contagion spread; the following night several medicine chests were thrown into the harbor by men in the disguise of South Sea Islanders; the disaffection increased, a mass meeting was called on Boston Common, each speaker in turn became more violent than his predecessor; some advocated prohibition of the traffic in drugs, another that the meeting should resolve itself into a vigilance committee to warn the doctors to leave in twenty-four hours, or swallow their own potions; but a more moderate and humane citizen, "one touch of nature makes

the whole world kin," said that though he rose in support of the general sentiment, he would offer an amendment, "that in the contemplated destruction, an exception should be made in favor of Bourbon whiskey."

After several violent and inflammatory speeches of the above tone had been uttered, and multifarious "experiences" unburdened in proof of the vileness of the prevalent system, and the hatred and detestation in which it was generally held, an old gentleman moderately endowed with common sense got up and inquired if there was no such thing in the world as rational medicine, whether nothing could be made acceptable to the public but extremes of absurdity?

This proposition and its corrollaries, he developed with such astuteness, honesty and force, and in such downright, manly English, and plain good sense, enforced by arguments, which we have not space to follow, that the meeting, after passing certain most commendable resolutions to the effect that it was unbecoming a free, and enlightened, &c., people to be drug-ridden, &c., &c., and recommending recourse to temperance, exercise, good food and rational medicine, whenever medical treatment was required, quickly dissolved. The revival was over, drugs fell, the foreign quacks wended their sullen ways homeward, the surplus indigenous medical men went off to California, the country resumed its wonted cheerful indications, and healthy aspect, and the new *regime* was fairly and forever inaugurated.

We have lingered so lovingly over the doctor's humor, that we must dispose of his more serious presentation of this important subject very hastily, with the earnest request to all, "buy and read."

The "exposition" opens with some general remarks on the medical ultraism of the day, physicians being addicted, for the most part, to the practice of over-drugging, on the one hand, and an inert, nurgatory practice on the other.

The methods which, at the present day, are most prevalent in civilized countries, in the treatment of disease, may be denominated as following:

1. The *Artificial* method, which, when carried to excess, is commonly termed heroic, and which consists in reliance on

artificial remedies, usually of an active character, in the expectation that they will of themselves remove diseases.

2. The *Expectant* method. This consists simply in non-interference, leaving the chance of recovery to the powers of nature, uninfluenced by interpositions of art.

3. The *Homœopathic* method. This is a counterfeit of the last, and consists in leaving the case to nature, while the patient is amused with nominal and nugatory remedies.

4. The *Exclusive* method, which applies one remedy to all diseases, or to a majority of diseases. This head includes hydropathy, also the use of mineral waters, electrical establishments, etc. Drugs newly introduced, and especially secret medicines, frequently boast this universality of application.

5. The *Rational* method. This recognizes nature as the great agent in the cure of diseases, and employs art as an auxiliary, to be resorted to when useful or necessary, and avoided when prejudicial.

These methods are severally treated of, hastily but appreciatively, and their rules, methods, and tendencies enumerated and illustrated. To the latter alone it is argued, we should give an adherance and lend our aid; its support, progress and triumph, is the work which young Physic has before it to aim at, and accomplish.

What its objects and duties are, we let the author explain in his own language.

It is the part of rational medicine to study intelligently the nature, degree and tendency, of each existing case, and afterwards to act, or to forbear acting, as the exigencies of such case may require. To do all this wisely and efficiently, the practioner must possess, first, sufficient knowledge to diagnosticate the disease; and, secondly, sufficient sense as well as knowledge to make up a correct judgment on the course to be pursued. In the first of these, if properly educated and experienced, he will be able to make an approximation to the truth sufficient for practical purposes. In the second he will have to depend mainly on his well-ordered and logical powers of self-direction; for he will find in the recorded evidence of his predecessors quite as much to mislead as to guide him rightly. He will find many existing cases, in which for a time he will know not what to do, and in which his safest course will be not to do he knows not what. It is better to resort to a little expediency than to rush into blind and reckless action. Nature, when not en-

cumbered with overwhelming burdens, and when not abused by unnatural and pernicious excesses, is, after all, "the kindest mother still." Art may sometimes remove those burdens, and regulate those excesses; but it is not by imposing new burdens and instituting new excesses that an end so desirable is to be attained. Before commencing any contemplated course of treatment, in a given case, two questions should always be asked: 1. Will it do good? 2. Will it do harm? A right answer to these questions will not fail to produce a right practice.

To the pamphlet is affixed, in form of an appendix, a recapitulation of the principle remarks of Sir John Forbes, submitted by him as things to be reflected and acted on by the medical profession; a becoming close for a work, which, though a brief one, will prove no mean auxiliary in the promotion of the great task which both have assigned themselves as a worthy life labor, and whose inception, already accomplished, holds out a brilliant and enduring promise of sublime triumphs to be won in the great hereafter.

Art. XIV.—*On the Morbid Anatomy and Symptoms of Cancer of the Pancreas:* by J. DaCosta, M.D. (Extracted from the proceedings of the Pathological Society of Philadelphia). Philadelphia: J. B. Lippincott & Co. Pamphlet. pp. 29.

We have condensed into this brief space an erudite and able essay upon one of the most interesting and obscure of all the organic lesions which result from that terrible and malignant condition known as the cancerous diathesis. Perhaps we ought to modify the pathological views which that phrase commits us to, however, viz: that malignant diseases are always secondary,—an opinion heretofore held and maintained very generally,—since the cases reported by DaCosta seem to prove that cancer may commence and terminate in the pancreas, or extend itself merely to contiguous organs.

Cancer of the pancreas usually attacks the right extremity of that viscus first, the other portions, if not remaining healthy, becoming indurated or undergoing a fatty degeneration. The lymphatic glands of the neighborhood are next attacked, and form a hard conglomerate mass with the head of the organ, which, by pressure obliterates the ducts or changes the structure of the liver. The pyloric extremity of the stomach and duodenum are from their proximity also liable to become the seat of morbid changes of varying character and leading to different terminations; of which latter the most frequent and fatal is peforation.

The majority of cases present themselves, in conformity to the general law, after the fortieth year; their subjects are chiefly male; the duration of the affection is variable and uncertain, sometimes lasting for years. The mode of death is usually by exhaustion, or peforation of the stomach or intestines.

The symptoms are extremely inconstant, and usually produced by the effects of the disease on other organs. The most prominent of which are, "a tumor in the epigastric region, pain there or in the back, constipation, progressive emaciation and debility and obstinate jaundice, and, occasionally vomiting, as the disease advances. The diagnosis is possible if these symptoms are present, provided we are able to exclude with certainty the diseases of the stomach and of the liver.

The author has carefully tabulated 37 of the best authenticated cases of cancer, from the reports of physicans, living or recently dead, together with the age, sex, duration, local signs, general symptoms, phenomena attendant and consequent, and *post mortem* appearances of each, and educed from their comparison, a most interesting, instructive and valuable monograph. The report fully sustains the high character of the body at whose request it was prepared, and is worthy in every way of its author, whose industry, patient observation, extensive information, and original talent are daily adding to his already worthy and well-earned reputation as a pathologist.

ART. XV.—*The Physician's Visiting List, Diary and Book of Engagements*, for 1859: Lindsay & Blakiston, Philadelphia.

We need do no more than announce the appearance of this universally known and used note-book. It is complete in its arrangement, can be procured of any wished for or necessary size, is neatly and strongly bound, and is altogether an unimprovable and most indispensable companion of the physicians daily round. It can be obtained of the publishers, and from most book-sellers everywhere.

ART. XVI.—*Annual Report to the Legislature of South Carolina, relating to the Registration of Births, Deaths and Marriages, for* 1857, by DR. R. W. GIBBES, JR. Columbia, So. Ca. 1858. Pamphlet. pp. 98.

Dr. Gibbes deserves the highest credit for his persevering industry in collecting and arranging the returns, made by the tax collectors of the various districts, of the deaths, births and marriages occurring throughout the State. A law requiring such returns was passed by the Legislature of South Carolina, in 1853, but as the collectors received no pay for this additional duty and labor, many of them refused to perform it. The statute was amended, therefore, in 1856, and 3 cents allowed them for each registration made; the good effects of this stimulus were immediately apparent, for a very full return was handed in the very next year, and the report before us exhibits a decided improvement on its predecessor in point of completeness as well as numbers. They amount, in the aggregate, says the reporter, to 32,796 births, deaths and marriages, which shows an excess of 1,668 above the number received for 1856.

The labor expended in the preparation of this report, must have been immense, and the tables exhibiting the rela-

tive fecundity, average ages at death, and comparative mortality of the black and white races will, when a number of such computations shall have been made, prove of great practical value, as they are now of immediate interest. The returns of course are very incomplete, but the system is being gradually perfected, and as the people are becoming every day more and more aware of the importance of such statistics, more reliable, accurate and full data, will from year to year be collected. Towards this important public work the suggestions and labors of the Drs. Gibbes—father and son—have contributed largely. The report consists, besides some general observations characterized by the author's usual acumen and good sense, and exhibiting no little study and consideration of the subject, of ten tables, occupying a space of 68 closely printed pages. The subjects of these are as follows :

Table I. (A)—Showing the population, the number and sexes of children born, the number of sexes of those dying, the number of marriages, the proportion of births, deaths, and marriages, the white and free-colored population, and the average at death.

Table 1. (B.)—showing the same facts for the slave population, and also the proportion of births and deaths, and the average age at death in the whole population.

Table III.—Plurality cases—showing the number and sexes of twin births in each month and district.

Table IV. Still-born—the number of each sex in each month and district.

Table V. Deaths—the number of each sex at different ages.

Table VI. Deaths—the number in each month.

Table VII. Deaths—causes in each race, alphabetically arranged for each district.

Table VIII. Deaths—classified causes; nomenclature of Am. Med. Association.

Table IX. Deaths—each cause arranged in classes, and percentage in each race and district.

Table X. Marriages—the number of each sex at different ages in the several districts.

We recommend the report cordially to the attention and support of physicians, in the State especially ; and while we hope and wish that Dr. Gibbes may receive more general and effectual co-operation hereafter, and thus be enabled to offer us tables more comprehensive and precise, and therefore of greater value and reliability ; we cannot refrain from expressing our surprise as well as pleasure, at the degree of success he has already achieved, and the interest he has excited in the objects of a system so happily inaugurated and carried out.

ART. XVII.—*The late Prof. J. K. Mitchell, M.D., Inaugural Lecture to the Course on the Practice of Medicine, in the Jefferson Medical College of Philadelphia, delivered October* 12, 1858, *by* PROF. SAML. HENRY DICKSON, M.D., LL.D. *Preceded by Preliminary Remarks by* PROF. ROBLEY DUNGLISON, M.D., LL.D. Published by the class. Philadelphia. Joseph M. Wilson. 1858. Pamphlet. pp. 40.

At a meeting of the Faculty of the Jefferson Medical College, held soon after the death of Prof. Mitchell, his former colleagues, while recording, in terms as glowing as sincere, their estimate of his rare intellectual and social worth, resolved that an obituary notice of the deceased be delivered before the class of next session, by one of their number, and for the performance of this delicate and difficult task, Prof. Dunglison was fittingly chosen.

It was proper, unavoidable, however, that Dr. Dickson, in entering upon the duties of an office which the subject of the appointed *eloge* had so long and well discharged, should make some allusion to the life and character of one who had been his companion in youth, his tried and true friend through a

long and sundered life, and in whose accustomed place he now stood, while the air was yet instinct with the presence, and the familiar hall echoed with the tones, of a form and voice which had forever passed.

It is scarcely necessary to say that all can anticipate the eloquence and feeling with which Prof. Dunglison would have offered the melancholy tribute ; but it is delightful to record the fine instinct which prompted him to yield this privilege to one whose elder friendship constituted almost a claim, and the circumstances of whose position gave an eminent fitness to the transfer. And this he does, in the introductory remarks prefixed to the address before us, in language so elegant, so terse, so massive, in a strain of such mournful retrospect, yet glowing anticipation, softened by such tender memories and brightened by such happy auguries, the shadows and lights of whose mingled eulogy and compliment are blended beneath his pencil into so perfect union, that we cannot conceive of an installation more reverential to the memory of the old, or more grateful to the feelings of the new incumbent.

We have elsewhere given a sketch of Professor Mitchell's life and services, and expressed our sense of the loss his death has been to the whole profession. Such an analysis of the matter of Prof. Dickson's Introductory, as our space would permit, would only be a repetition of this, filled in with the details which his more perfect and long intimacy supplied. But the feeling, the profound tenderness, the surpassing purity of style with which he has clothed the rich body of thought suggested by his fruitful and congenial theme, are beyond all reproduction. How beautifully he has developed the graces and lofty virtues of character for which his friend was loved and respected. How perfectly from the story of his struggles and achievements, he has educed, and held up for imitation, those qualities of decision, energy, perseverance, enterprise, ambition, whose sum made up the character of the lamented Mitchell. There is no mere dull delineation, no exaggerated coloring here ; but the faithful likeness whose every feature is preserved yet idealized, from the

combination of whose outer form one reads and knows the soul within, is drawn with an accuracy which leaves no line untraced, and yet is infused with that true genius which seizing upon the salient points of character, stamps the portrait with an individuality and unity of composition that constitutes pre-eminently a perfect picture.

The poetry and energy of the boy's character, the failing health of the man, foreboding ruin on the very threshold of active life ; his determined and successful effort to regain the strength for labor ; the assiduity, the habits of observation, shrewd apprehension, thoughtful investigation, ingenuity, originality, and sympathy, which he brought to the study and practice of his profession ; his early successes, his steady progress, and lofty triumphs in more than one of the noblest departments of human thought ; his commanding air and person ; his overflowing hospitality, generous as the sun ; his conversational brilliancy ; his genial cheerfulness ; his kindliness of heart, and friendly sympathy, which not the shadow of the coming darkness could chill ; his indomitable spirit and courage, which not all the powers of disease could crush ; the stern resolution with which he met every assault of the inexorable foe, while there was a duty to be done, or a kind office performed ; the calm submission with which this Christian gentleman met the final summons, and with a thanksgiving upon his lips walked fearlessly down into the land of shadows, are all pourtrayed with a power and pathos, as rare as exquisite, and with that excelling majesty, that overwhelming abandon of a real grief which sweeps away all minor feelings before its impetuous torrent, and rises from the depths of its profound passion with the despairing cry of an irreparably shattered friendship, " for Lycidas is dead, and hath not left his peer !"

We regret that we have space for but a single paragraph, taken at random, from what we esteem one of the most classic productions of Prof. Dickson's always polished pen. He is speaking of the influence such an example as the one just held up, should exercise on the younger members of a profession whose honors and rewards are the approbation of

one's own conscience, and the esteem and remembrance of one's peers alone.

Nay, if any one should, by whatever error, be induced to enter among us, who does not feel himself warmed and spurred forward by such examples; who does not anticipate with glowing spirit the day when he too may join these shining ranks as a volunteer, and if need be, a martyr, I would tell him plainly that he has mistaken his calling; that he is not one of us; that he is not worthy to unloose the shoe-latchet of those, and such as those—knights-errant of philanthropy—and let them never be alluded to without profound reverence—who went down to die for the sake of humanity at Norfolk and Sandusky; and who, to the honor of our profession and of our common nature, are in every age and nation so numerous, that their biography would fill another library of Alexandria. He may be gifted and useful, but it must be in another sphere. He may found a city or invent some new and wonderful mechanism; he may win a naval battle, or ride red-hoofed over the mournful field of conquest; but unless he can possess his soul in tranquil cheerfulness amidst the busy solitude of a lazar-house, and perform with alacrity the most menial and repugnant offices of kindness to an unknown pauper, he cannot, he never will be a physician.

ART. XVIII.—*The Physician's Hand-Book of Practice for* 1859. By W. ELMER, M.D. W. N. Townsend & Co. New York: 1858.

This new Hand-Book of Dr. Elmer's, is one of the most convenient of its class now published. In addition to the usual list of poisons and their remedies, published in most of its kind, we have contained in it a classification of diseases with their most prominent symptoms, an alphabetical list of medicines, with their remedial properties, doses, etc., an account of Marshall Hall's "Ready Method," &c., &c. On one side of the blank page are ruled spaces for the record of patients, daily visits, &c., and on its opposite, room for the annotation of symptoms and treatment. The portability of the book has been much increased by a reduction of its size. For prices, address of editor, and details generally, we refer the reader to the advertisement published in this number.

ABSTRACTS AND TRANSLATIONS.

FROM FOREIGN AND DOMESTIC JOURNALS.

PATHOLOGY AND PRACTICAL MEDICINE.

Tyhoid Fever in Children. From the Second Edition of the Traité des Maladies des Enfants, by MM. F. RILLIET and E. BARTHEZ. Translated for the Charleston Medical Journal and Review. By DAVID E. EWART, M.D., Newberry, S. C.

[Continued from Vol. XIII, No. 6. pp. 834]

ART. II.—SYMPTOMS.

The symptoms of typhoid fever in children as in adults, consist of a more or less decided disturbance of all the organs of organic life, and the life of relation. The nervous, digestive, circulatory and respiratory systems present signs of derangement. The symptoms furnished by the digestive apparatus, from their number, importance, and persistence, will first demand our attention.

Sec. 1. *Digestive functions. Alvine evacuations.*—Among these symptoms, diarrhœa is the most prominent. We must not, however, expect to find it always at the commencement of the disease, for at this stage we have frequently found (in a fourth of the cases) the bowels to be constipated, and sometimes though rarely, the evacuations remain natural for the three or four first days. Whatever may be the condition of the digestive tube at the outset of the disease, spontaneous or excited diarrhœa always eventually comes on, generally on the tenth or twelfth day; and when once established, persists during the whole course of the disease, though varying much in quantity.

In those children who recover, the stools are generally voluntary; unless they are very young, they nearly always make known their desire to evacuate the bowels, and even in very grave cases, we have noticed that notwithstanding their extreme debililty, they would endeavor to get out of bed to satisfy their wants. We have never seen involuntary stools

take place on the first day; they seldom occur before the fourth or fifth day, and generally at a much later period.

2nd. *Vomiting* occurs in about half the cases. It usually comes on at the commencement of the disease, from the first to the eighth day, but it is generally slight on the first, second, and third days. We have however, seen it occur frequently, and with the spontaneousness of the vomiting which takes place in meningitis, in a boy, four years and a half old, who died on the ninth day. Vomiting occurs especially in those cases in which there is constipation at the outset of the disease, and it is important to remember this coincidence, as in certain cases it may throw some doubt on the diagnosis. It is ordinarily in grave cases that it is observed to take place.

This symptom was very distressing in some cases which we have treated in our practice. We will cite particularly the case of a boy four years old, who, on the eighth day, was seized with almost incessant vomitings, accompanied with involuntary stools; his features became very much altered, and he was so debilitated by it, that we feared it would kill him. In another boy of nine years of age, affected with a very grave ataxo-adynamic typhoid fever, vomiting was so frequent, and was so easily excited, during eighteen days, that we were apprehensive that he could not take sufficient nourishment to keep him alive.

3d. *Abdominal pains* usually accompany the diarrhœa, and when this occurs at the commencement, they generally also appear at that period; but this is not invariably the case, for we have noticed abdominal pains both in cases in which there was constipation, and in those in which the alvine evacuations were natural. It is not always an easy matter to ascertain their existence in children; for when very young they do not voluntarily complain of them, and they can scarcely be detected by pressure on the abdomen, as children under such circumstances are apt to cry when touched by a stranger on any portion of the body. In the great majority of cases, these pains are transient, their duration being at most only four or five days. Though ordinarily slight, we have in some few cases, seen them very intense, and amongst others, in a child eleven years of age, who died on the twenty-first day of the disease; but in this case the fever was complicated with a very grave enteritis. We will however again speak of these abdominal pains when we come to notice the complications. We have rarely been able to ascertain the part of the abdomen in which the pain commences when it exists from the first few days of the disease,

the statement of parents not being alone satisfactory on this point. When we have noted them ourselves, they were sometimes general, sometimes confined to the epigastrium or umbilicus, but oftener in the right iliac fossa.

The intensity of the pain is in some cases so extreme as to give rise to serious apprehensions of the invasion of a peritonitis. As an example, we will cite the following case :

In our private practice, we attended a boy, aged eleven years, affected with a typhoid fever of medium intensity ; the disease was progressing regularly, when, suddenly, there supervened extreme abdominal pains, occupying especially the epigastrium ; the countenance became profoundly altered, but there was neither hiccough nor vomiting, nor much distension of the abdomen ; the bowels were checked. These pains were felt for thirty-six hours, though with intermissions, and were of such intensity that in the highest of the paroxysms, it was impossible to prevent the child from springing from his bed and rolling on the floor. The brain was not affected ; all his distress seemed concentrated in the abdomen. After having tried every anodyne application we could think of, we at last had recourse to leeching, notwithstanding our dislike to do so, and the patient was entirely relieved by it.

4th. Shape of the Abdomen.—At a variable period after the appearance of diarrhœa and abdominal pains, the external shape and configuration of the abdomen is changed in consequence of meteorism ; but this symptom is far from being constant. In fact, the abdomen frequently remains natural in appearance. In rare cases, and in which the disease is exceedingly grave, we have seen the abdominal parietes become retracted and hardened ; yet even in cases of this kind, the abdomen does not assume that excavated appearance which is observed in cerebral affections. It is seldom before the sixth or fourteenth day that tympanitis is noticed ; it sometimes persists during the whole course of the disease ; but generally it disappears in five or eight days. In some grave cases, when the tympanitis has existed for several days, we have noticed the abdominal parietes to become so thin as to allow the intestinal convolutions to be distinctly seen beneath the almost transparent skin. We have in no case, however, observed the meteorism to take place to such an extent as to impede the action of the diaphragm, and render respiration difficult.

5th. Abdominal gurgling is a transient phenomenon, and one which we have far from always observed (half the cases) ; it

occurs at very variable stages of the disease, and nearly always in the right iliac fassa.

6th. Condition of the spleen.—We have seen in the preceding article that enlargement of the spleen is not always met with in the disease; one will not then be astonished when we state that in a large number of cases we have not been able to detect any morbid increase in the size of this organ. The results of our observation in this particular, differ entirely from those of M. Saupin, who, in one hundred and twenty-one cases. discovered considerable enlargement of the spleen in one hundred and nine, and moderate enlargement in ten. In a thesis published by one of us some time ago, we stated that enlargement of the spleen had not been noticed by us but in eighteen cases in every sixty-one. New observations which we have since made, have confirmed the result which we then obtained. Thus, out of forty-four cases, at the *Hospital des Enfants*, in which we carefully examined the condition of the spleen by *purcussion* and palpation over the left hypochondriac region, we discovered this organ to extend below the ribs for at most two inches in only ten cases; and in eight, the hypogastrium was dull to a greater extent than in health. In short, in twenty-six cases, we did not observe either tumefaction or dullness at any stage of the disease. Enlargement of the spleen is seldom seen before the seventh or twentieth day. In an exceptional case which came under our observation, it was enlarged on the third day; and in another case, on the contrary, it did not take place until between the thirty-second and fortieth day. The latter case was in a child who had a relapse of the fever. Generally, the enlargement is but slight; it seldom lasts more than five or ten days, and often a much shorter time. Our observations in our private practice confirm all these results.

7th. Tongue—Teeth—Gums—Breath.—The condition of the tongue varies according to the different periods of the disease. Sometimes of a natural color, but often of a bright-red, or covered with a thick yellow fur, it remains moist during the entire course of the disease, especially when this is light or of short duration, and even in a small proportion of grave cases. Dryness, when it exists, never appears before the seventh day, often soon after this period or still later. The tongue is then dry at its point or constricted, and in some rare cases, covered with a brown crust of variable thickness. In cases, entirely exceptional in children, we see the tongue parched, brown, dark or fissured; it is also rare to see the teeth covered with thick or fuligenous crusts. The gums, as in all acute diseases, are more or less swollen; and

they are often covered with soft pseudo-membranous patches, but are rarely superficially ulcerated. The tongue and teeth are cleared of their coating or reassume their normal moisture at the expiration of a period varying from the fifteenth to the twentieth day, sometimes later (the thirtieth). It is principally in cases in which the tongue, teeth, and lips are fuliginous, that the breath is disagreeable; and it is more decided in proportion as these characters are more marked; the odour is peculiar. The lips are nearly always dry or slightly encrusted. We have only once noticed at the posterior part of the lower lip, a thick, yellow, adherent pseudo-membranous layer; it did not disappear until after the expiration of several days.

8*th*. *Appetite—Thirst.*—There is generally a loss of appetite from the commencement of the disease, and at the same time, there is an increase of thirst; though this is never so great as in certain cases of pneumonia. The loss of appetite, and the thirst, continue throughout the entire course of the disease; though in certain cases, there is considerable appetite from five to fifteen days before the patient is entirely cured.

9*th*. *Urine.*—We have never observed a retention of urine in our cases, though in many, the urine was passed involuntarily.

Sec. 1. *Nervous System.* 1*st*. *Positions which the body assumes —Features.* When called to a child affected with typhoid fever, we generally find him lying upon his back, and rarely on the side; he is restless, and frequently changes his position; he is annoyed at being compelled to lie abed, especially during the first stages of the disease; for at a later period, if the disease is grave, he remains quiet, except when affected with a restless delirium. The general expression of the countenance presents no fixed characters for the first few days: when the fever is very high, the face is generally flushed and animated; the features, as is the case at the commencement of all acute diseases, are expressive of solicitude or dejection. If the disease is grave, the countenance indicates anxiety and distress, and the eye is restless; later in the disease, the eyes are sunken, the nostrils dry or pulverulent, and the lips parched; the face but slightly flushed, is expressive of dejection in slight cases, and of stupor in grave ones. At a still more advanced stage, this stupor disappears to give place to that expression of contentment, to that air of cheerfulness, which are the certain forerunners of the re-establishment of health.

The expression of countenance is, perhaps, of all other

symptoms, the one to which the physician should attach the most importance in forming his prognosis. The first smile, or even the shadow of a smile, noticed in the midst of symptoms still alarming, possesses great value. But, for this symptom to give what it promises, the hope of recovery, it must be spontaneous and not forced, pleasing and not sardonic, natural and not accidental. If the first smile marks the commencement of convalescence, the open smile, and especially the laugh, are signs that the patient has escaped all danger; it seems as if it was from a consciousness of this happy event, that his countenance indicates this sentiment of grateful joy, which, in the convalescence of no other acute disease, is found so expressive.

2d. *State of the Strength.* This may often be measured by the expression of the countenance: thus, when the countenance is expressive of great dejection, one may be pretty sure that there is great prostration of muscular strength. However, it is not always easy to ascertain the loss of strength, and especially to estimate its degree. Yet there is always more or less prostration at the commencement of the disease; generally the children are bedridden in the course of the first week, and they seldom come to the hospital afoot. This prostration of strength augments in proportion to the advance of the disease, and remains during its entire course; but as convalescence takes place, the children generally gradually recover their strength, though they sometimes do this even suddenly; so much so, in fact, that the physician is frequently much astonished by finding in his morning visit a patient sitting up, who, on the previous evening, appeared considerably prostrated.

3d. *Headache, Vertigo, etc.* The symptoms which we have just examined, are all easily recognized in the child. This is far from being the case with the different functional disorders of the brain, the existence of which, in a large number of cases, it is impossible to detect. M. Saupin has endeavored to overcome these difficulties by pointing out certain characters of the countenance, and positions of the body, by means of which a person may ascertain the existence of headache, vertigo, tinitus-aurium, etc., in young children. We fear, however, that the legitimate desire to clear up an obscure subject, has carried M. Saupin too far, and that he has admitted the existence of these symptoms from a delusive appearance. Since we have read his work, we have in vain sought to ascertain the existence of headache by slightly shaking the patient's head, or by seeing them frown or half-close their eye-lids. For in twenty different diseases, we may

see children make wry faces when the head is shaken; in twenty different diseases they may be seen to half-close the eye-lids or frown, without there being a possibility of asserting that headache is the cause of these phenomena. Let us then hold to the facts of the case, which every one may prove; and putting aside the subtleties of diagnosis, let us admit that, in a large number of cases, it is impossible to ascertain the existence of headache. When it exists, it generally takes place at the commencement of the disease; its duration is ordinarily short, disappearing from the seventh to the tenth day; though, in an exceptional case, we have noticed it to last for seventeen days. It is never very intense, which is an important particular in diagnosticating it from cerebral diseases. Let us, however, state that M. Saupin has observed it to be very intense in four cases. When we have been able to ascertain its seat, it generally occupied the frontal region.

We have rarely observed vertigo. Generally, the youngest children are unable to express their sensations; the oldest even do not often comprehend the questions addressed to them. In a few cases in which we have been able, with any certainty, to ascertain the existence of vertigo, it very rarely manifested itself on the first day; (this, however, may be owing to a want of proper insight); it is from the eighth to the twelfth day that we have usually noticed it.

4th. *Delirium—Agitation.* What we have said concerning the difficulty of ascertaining the existence of headache, is equally applicable to the disturbances of the cerebral functions.

In young children, true delirium, consisting of a perversion of the intellectual faculties, is replaced by other signs, which denote a disturbance of the cerebral functions. These symptoms are anxiety, agitation, sharp and causeless cries, etc.

In cases in which the disease is very light, disturbances of the brain are wanting entirely; at least, so long as the patients have been under our observation, we have not noticed either delirium, great somnolence, or sharp cries, and the parents have assured us that these symptoms had not existed. In the cases in which we have observed cerebral symptoms, and this was in the majority, they were variable, and displayed themselves at different stages of the disease.

Delirium very rarely exists at the commencement of the disease; it generally manifests itself from the seventh to the fifteenth day. It occurs oftener at night than during the day. Its duration is very variable; we have very rarely seen it last longer than fifteen days. In a child of eight

years of age, who had been treated by bleedings, it remained a considerable time beyond the twenty-fifth day; and only disappeared when the patient was sufficiently recovered to take nourishment. The delirium is only intense in grave cases; and it is very seldom that the straight jacket has to be used.

M. Saupin has described the different forms which the delirium may assume in the following manner: "Sometimes," says he, (loc. cit., p. 180,) "the children are listless and dreamy, asking for something to eat, and always the same food, calling their parents, speaking of their customary occupations, of their trades, of their education; at other times they throw themselves about violently; and if the precaution is not taken of binding them at night, they will get out of bed, run about, and play or lie down with their sleeping companions. Others wander about the room without any object in view; and some cases have been seen in which, during the delirium, the patients strode about, resisting the control of strong persons; and who, when the exacerbation had ceased, were not able to drink or sit up in bed without assistance. The next day they seem to know nothing of what transpired during the night; during the delirium, they do not answer questions addressed to them, but keep up a disconnected conversation with themselves. Others have a tranquil sub-delirium; these cases are seen lying on their backs, perfectly quiet, but continually muttering something in a low voice; if interrupted, they stare about with a look of astonishment, and after a few moments hesitation, give an answer."

5th. *Somnolence.* The delirium is often replaced by somnolence, which disappears to re-appear again. This symptom, like the delirium, is not a common phenomenon at the commencement of the disease. It alternates, or is coincident with the delirium, succeeds agitation, and sometimes comes on at an advanced stage of the disease, without having been preceded by other nervous symptoms. The children can generally be roused from their somnolence by strongly exciting them, and they then cry or utter hollow groans, and again relapse into their drowsiness. We have sometimes observed a coma as profound as that which occurs in the adult. When the somnolence is strongly marked, the speech is much embarrassed.

A remark, which we have often had occasion to make, and which is especially applicable to the subject of which we now treat, is that to estimate fully the value which should be attached to nervous symptoms in diagnosis or prognosis, it is

necessary to understand thoroughly the habits, characters, and even the training of the patients whom we examine. A capricious, irritable, badly raised child, will appear much more seriously affected by these nervous symptoms than he really is; and if allowance is not made for these circumstances, the physician is liable to commit a great mistake in his diagnosis or prognosis. We have too often permitted ourselves to be deceived by this fallacious appearance, not to put our young confreres on their guard against the rock upon which we ourselves have stranded.

6th. *Disturbances of Motility.* Headache, delirium, and somnolence, of all nervous symptoms, are the most frequent; those connected with perturbation of the muscular system are much more rare. They are almost exclusively observed in grave cases: they are subsultus tendinum, carphologia, contractions of the extremities, rigidity of the trunk, and convulsions. Here again the results of our experience are entirely different from those obtained by M. Saupin.* We have noticed carphologia in only 7 cases out of 107, and always in grave cases; we might say the same of subsultus tendinum, which we have noticed in but 4 cases out of 107 patients. Let us here observe, that we have examined 46 new cases since the first appearance of M. Saupin's work, and that the proportion affected with subsultus tendinum, has little differed from the results which we had previously obtained. These two symptoms have generally been observed in children who died between the fifth and the fourteenth day from the commencement of the disease. We have never observed contractions of the extremities; rigidity of the trunk is a rare symptom, but an exceedingly grave one; we have noticed it 5 times out of 107 patients, and in every case the child died; this symptom came on at a period very near the fatal termination of the disease. Convulsions are still more rare than rigidity of the trunk; we have noticed this symptom twice as the last phenomenon; in these two cases they hastened the death of the patients. In another case they supervened on the seventh day; this one recovered. In our general practice, two children were affected with convulsions, and both died.

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* M. Saupin has evidently committed an error in saying, "*In nearly every case there is carphologia.*" It is true he gives this name to carphologia proper, to subultus tendinum, and rigidity of the members. But to mention only the subsultus tendinum, we see that this author has noticed this symptom in 79 cases out of 121; whilst, at the hospital, we have noticed it in only 4 cases out of 107, and in our general practice scarcely ever.

One was in a litttle girl, aged fourteen months, who, on the twenty-third day of a grave, but not ataxic, typhoid fever, was seized with an attack of eclampsia. This nervous crisis, supervening at a period at which the disease appeared to be yielding, was preceded by irritability, agitation, and suppression of the stools and urine. The eclamptic attacks were frequently repeated, and were followed by drowsiness, strabismus, general rigidity, and afterwards general relaxation. This cerebral condition continued for three days, and then terminated in death. In our autopsical examination, we discovered an arachnoidean and sub-arachnoidean sero-sanguineous effusion.

The other case was in a child aged ten years; on the eleventh day, of a typhoid fever, which appeared benign, there suddenly supervened delirium, then coma, then convulsions, at first limited to the face, but soon becoming general, followed afterwards by trembling of the members, a slight distension of the right labial commissure, retraction of the abdomen, and constipation. The convulsions again appeared on the day the patient died, which was the seventeenth. The autopsy revealed a slight general congestion.

We will here mention, that we have observed a case of chorea to follow a grave, ataxic typhoid fever. A similar case is to be found published in the *Journal de Vandermonde*, (1759, vol. x., p. 129): this case occurred in a girl aged eight years, affected, says the author, with a putrid continued fever; on the twelfth day symptoms of chorea arose, with a loss of sight and speech; at the end of eight days these symptoms had ceased.

7th. *Pains, Neuralgias.* Many children complain of pains, which, from their situation, their intensity, or the stage of the disease at which they appear, may lead to mistakes. It is during the prodromes of the fever, or during the first four or five days of the disease, that we have noticed this symptom. Its duration is seldom beyond the first seven days. In some it was seated in the side, and resembled intercostal neuralgia, or pleurisy. In the greater number, the pain was in the side and back part of the neck, as in cervical neuralgia or torticollis. Though transient, and of moderate intensity in most cases, it has yet sometimes been so great as to attract the attention of the attendants to the exclusion of other symptoms. It was thus, amongst others, in a boy of four years of age, in whom the disease commenced as a pharyngo-bronchial catarrh. The pain was first felt on the third day of his illness, and was very violent, especially during the febrile exacerbation, at which time the patient would

scream out from the excess of his suffering, and carefully retain his head in a straight and fixed position, not allowing any one, on any account, to make the slightest movement of it: this pain persisted thus during nearly the entire course of the disease, that is to say, for fifteen days. It attracted so much attention for the first eight days, that a diagnosis was not made until the tenth or twelfth day of the disease, at which time a lenticular eruption and a persistent diarrhœa appeared. It gradually wore off, without leaving any trace of its existence.

[TO BE CONTINUED.]

SURGERY.

Account of a Case of Elephantiasis of the Scrotum, accompanied by some observations on this affection.. Translated for the Charleston Medical Journal and Review, from the Spanish of Dr. DON FREDRICO GALVEZ, by Dr. J. P. CHAZAL, Charleston, S. C.

We seize the first opportunity of giving an abstract of a pamphlet of 29 pages published in and received from Havana, and containing a clear and detailed account of a case of Elephantiasis of the scrotum successfully operated upon by Dr. Fredrico Galvez, of that city, in May, 1857. Two excellent lithographic plates accompany the pamphlet.

Guillermo, a *carabali briche*,—a name, we understand, applied to a warlike tribe imported to Cuba from the Gold Coast,—is over 50, robust, well developed and of high stature, and was brought to Cuba as a slave, between the years 1826 and 1828; since which time he has been employed as a field hand. Enjoying good heatlh up to 1830, he was seized with typhoid dysentery, then epidemically prevailing, and was successfully treated. He has since suffered from no general affection.

After the attack above mentioned he noticed that the scrotum became affected with an indolent swelling which went on increasing until the penis disappeared. This must have taken place since 1836, as Guillermo has a son (1856) 21 years old.

Many remedies, the emetics and purgatives of Leroy in particular, had been in vain employed when, Dr. Galvez examined him, for the first time, at his father's plantation, in March, 1857. He found a pyriform tumor weighing 57 pounds, having a horizontal circumference of 46 inches and a vertical perimeter of 38, with its summit implanted between the pubis and the anterior margin of the anus. The anterior aspect of the skin in its upper half was smooth and soft, the hairs of the pubis being much scattered. Four eminences were observable in this region and corresponded with the corpora cavernosa, the two spermatic cords and a reducible inguinal hernia of the right side. Commencing midway down the front aspect was a vertical slit 3-4 of an inch in depth, which extended to the base and seemed to divide the mass into two lobes. At the upper part of this slit was the opening which gave passage to the urine; it admitted the thumb and was lined by an indurated mucuous membrane; and taking the patient's sensation for a guide, the glans penis was reached with a catheter at a depth of somewhat over three inches. The skin of this region was hypertrophied, dull in color and so distended that the *pores*, says the author, measured an inch between. The base of the tumour presented, besides cicatrized ulcerations, an ulcer at the lower end of the slit and still another farther back, the result of friction from the pantaloons, or the ground when the patient assumed a sitting posture. The posterior surface of the tumor was rugous, soft, less distended, presenting many folds which, commencing at the perinum, gradually disappeared about the middle of the surface.

The patient accepted with delight the idea of an operation, and with returning hope, his appetite also returned. He left the plantation for Havana in April.

Guillermo recollected having seen many such cases in his native country, and that they eventually terminated in sphacelus and death in the midst of intense agony.

Dr. Esdaile, with an experience of 161 cases, never spared the testicles when the weight was over 50, unless the subject was unusually vigorous. On the other hand, in one case only was he obliged to sacrifice the penis. Profuse hemorrhage during a long and protracted dissection constituted the main danger; but, thanks to present hemostatic means, more parts can be spared than formerly; a fact demonstrated by the cases of Delpech, Velpeau, Caffort, Lenoir and Casaldi, cases in which, though the tumors weighed less than

50 pounds, the testicles and penis were spared, in consequence of the hemostatic remedies employed.

We shall now proceed to give a condensed account of the operation as performed on the 8th of May, 1857.

The patient was placed in the same position as for lithotomy. A number of sponges for imbibing the hemostatic solution of perchloride of iron having been provided, chloroform was administered, and insensibility produced in ten minutes. A horizontal incision five inches long and involving the healthy skin only, was made at a like distance from the arch of the pubis. It intersected at a point supposed to correspond with the glans, and bordered close upon the region where the skin presented incipient degeneration. The hemmorrhage began at once by the section of a vein at each extremity of the incision; these were tied. A vertical incision was then carried from the groin, at an inch outside of each eminence formed by the spermatic cord, to the corresponding extremity of the horizontal incision. The interspace formed by these three incisions was dissected upwards and produced a rectangular flap of healthy skin, joined to the pubis by a line five inches broad and on a level with the suspensory ligament of the penis. It was intended for a covering to this organ in the event of its preservation. A rather profuse capillary hemorrhage followed, which was speedily subdued by the perchloride of iron. Here ended the first step.

The anterior wall of the adventitious urinary passage was next incised with the aid of a grooved director, and the upper extremity of the incision made a point of departure for the isolation of the corpora cavernosa, the plan being to use these as a guide for farther progress. The glans was reached at the depth of three inches, towards the centre of the mass. It was pale and atrophied, but healthy. Serving as a guide, the corpora cavernosa were exposed to their very roots. During this process a prodigious number of enlarged vessels were implicated, mainly venous, among which the dorsal artery of the penis was to be seen tripled in size. In reaching the present stage of the operation a full hour had been consumed, mainly in controlling the hemorrhage; thirty ligatures had heen applied and the perchloride of iron had not ceased to bathe the exposed surfaces. Notwithstanding, however, the loss of at least a pound of blood, the pulse had remained firm. The nausea supervening was attributed to the chloroform, the administration of which was, on that account, suspended.

The operator then sought the right testicle, the successive incisions being carefully made with the assistance of a grooved director, until he reached a fibrous cord of an inch in diameter, of a dull white color, a moderate degree of resistance, and whose fibres diverging downwards and separated at points, allowed, on being exposed, the protrusion of what appeared to be hernial formations. They turned out, eventually, to be a double hydrocele, each containing four or five ounces of fluid, and which proved useful afterwards in guarding the testicles against injury during the progress of the dissection. Another hour, however, was spent in isolating these and in subduing the hemorrhage.

While isolating the left testicle, the pulse fell, the vomiting became continuous, the surface cold, and fainting imminent. A glass of aniseed brandy was administered, which revived the patient and even produced a state of intoxication that still existed thirty hours afterwards. It was resolved to finish the operation in all haste by taking from the posterior surface of the tumor a flap of healthy skin. The one obtained proved to be seven inches in diameter and met the two anterior and vertical incisions at their upper extremity. This flap was intended for a scrotum. In order to protect the testicles from external shocks, it was resolved not to puncture the tunicæ vaginales, a precaution, which, in the midst of the difficulties attendant on the first dressings, there was much reason to applaud.

After removing the tumor, the anterior flap was brought down over the penis, and a line of sutures was made to unite, along the inferior surface of the organ, the edges of the vertical side of the flap, brought in apposition. The inferior border was united by five sutures to the edge left around the glands after excision of the prepuce. Another longitudinal line of sutures converted the posterior flap into a scrotum. Pledgets of lint over the member, with a Maltese cross for support, were fixed by a circular bandage; the scrotum was covered by pledgets and a suspensory bandage maintained by a T strap. The patient being put to bed, anti-spasmodics were from time to time administered.

On examination of the tumor, the epidermis presented nothing very remarkable. The dermis one inch in thickness, indurated, without retractility, with areolæ of much increased size and number, appeared to incorporate itself with the subjacent cellular structure, where the most remarkable changes were to be found. This structure constituted the remainder of the mass, as examined. Of a resist-

ing fibro-cellular character, it contained well defined cells, which in some cases, attained an inch in diameter, and enclosed fluids of variable nature; here a transparent serum, there a thicker fluid of gelatinous aspect; at some points the contents were white and opaque, whereas, at others, the cells had apparently acquired a lardaceous structure. The dartos was much enlarged, as well as the cremaster, whose fibres, which have already been alluded to, were hypertrophied to a high degree and wore a fibrous aspect. As regards the genital organs they presented a healthy appearance, bating an enlargement of the spermatic cords and the accumulations of fluid above mentioned.

Treatment.—Evening and night of May 8th, date of the operation. Depression, uneasiness; pain around the waist, probably from constrained posture during the operation; nausea and vomiting; pulse small and irregular. This state being referred to the long nervous excitation produced by the chloroform and aniseed brandy, a portion was administered, hourly and by the spoonful, composed of opium, cherry laurel water and a decoction of linden flowers, alternating with broth and a little wine of bark. Wakefulness, restleness and anxiety; a slight hemorrhage in the night easily controlled by saturating the dressing with a strong solution of perchloride of iron.

9th. Less vomiting and less lumbar pain; the pulse rises; the vomiting ceases towards night; agitation; sense of fullness in the head; reaction begins. The broth and wine are suspended; the antispasmodic continued, with lemonade in addition. Urine abundant and ammoniacal; the wakefulness continues.

10th. Same state; towards evening the febrile symptoms abate; at night the patient has a sleep of two hours duration and awakes without fever; pulse weak and depressed, broth and wine resumed; suppuration is established.

For the three first days the dressing was renewed every twenty-four hours. These and the succeeding dressings were effected with dry lint.

11th. Pulse steady and normal; no appetite and a little thirst; broth and lemonade.

12th. A few points of the suture of the penis and of the lower part of the scrotum, become detached and several gangrenous spots are noticed, which, when becoming separated, give the wound a scolloped appearance; extraction of some plugs of sphacelated cellular tissue; liquor of Labarraque applied; sensation of fullness in the abdomen; sense of fatigue and anxiety during the night.

13th. Same state; attributed to bowels not having been open since the operation was performed; they are emptied by two emollient clysters; all unpleasant symptoms cease and the patient has a refreshing sleep of four hours. The slough falls; suppuration diminishes; application of oil of turpentine, this and the following day. Soup, broth and wine.

14th to 21st inclusively. State of patient satisfactory; suppuration healthy and steadily diminishing. Cicatrization progresses rapidly; is complete at the anterior part of the scrotum, and nearly so around the glans. Patient is contented, and is put on a more generous diet.

June 12th. Patient increases daily in strength, is comfortable and shows great agility in walking.

The author ends his pamphlet by expressing his great satisfaction at having been the first to perform this operation in the Island of Cuba.

Statistical Table of the different Operations for Elephantiasis of the Scrotum, performed in different parts of the World.

Where performed.	Operators.*	No. of operations.	Penis and testicles preserved	Penis alone preserved.	Neither penis nor testicles preserved.	Cured,	Died,
Calcutta, Bengal, Alexandria and Madras,	Dr. Esdaile,	161	unknown.	unknown.	1	153	8
Paris,	Dr. Velpeau,	1	1			1	
Paris,	Dr. Lenoir,	1	1			1	
Montpelier,	Dr. Delpech,	1	1			1	
Paris,	Dr. Delonne,	1	1			1	
Paris,	Dr. Caffort,	1	1			1	
Paris,	Dr. Ceccaldi,	1	1			1	
Barbadoes,	Dr. Wilks,	1					1
Calcutta,	Dr. Titley,	1	1				1
Egypt,	Dr. Larrey,	1	1			1	
Edinburgh,	Dr. Liston,	1		1		1	
London,	Dr. Key,	1		1			1
Egypt,	Dr. Clot Bey,	1		1		1	
Calcutta,	Dr. Goodeve,	1		1			1
Havana,	Dr. Galvez,	1	1			1	
*New Orleans,	Dr. Picton,	1				1	
Charleston,	Dr. Michel,	1	1			1	

* These are the only two cases operated on in this country, so far as we are informed. We add them to complete the above very full table.—[Ed.]

EDITORIAL AND MISCELLANEOUS.

SALUTATORY.

We enter upon the second year of our editorial responsibility and privileges, with an humble consciousness of having, as far as lay in our power, discharged the obligations and fulfilled the pledges assumed and offered at the inception of our enterprise, and with a modest determination to do more ample justice both to ourselves and patrons in the succeeding issues of this New Year, which brings with it, we trust, more fully developed powers, larger aims and experience, better opportunities, and less contracted means than its predecessor. In the performance of our arduous and delicate duty, we have, consciously, spared no labor, shrunk from no sacrifice, and omitted no precaution which might ensure success. We are, indeed, fully sensible of the faults and imperfections with which our work is justly chargeable, but with no wish to screen ourselves from their penalty, or to offer impotent excuses for their existence and occurrence, we may be allowed to plead in extenuation the embarrassments to which we have been subjected from many sources, and the heavy demands made upon our time by other equally important and imperative duties; more especially during the visitation of the calamitous epidemic which has lately afflicted our beloved city.

We cannot refrain from adding how cheered and gratified we have been, by the commendation, public and private, which our work has received from various quarters and individuals; and this good opinion to which our friends have been pleased to give expression, has received the highest possible endorsement in the undiminished support and sym-

pathy of our old, and the accession to our list of not a few new subscribers. How little of this pleasant testimony to the worth of the Journal has been elicited by our personal efforts, we are well aware, and to those collaborators who have so kindly and generously given us the influence of their names, and enriched our pages with the products of their freely bestowed time and talents, we return our most cordial thanks. They will permit us to indulge the hope, that this friendly aid will not be withdrawn at a time when the most urgent efforts are being made to place the Journal upon a lofty and secure basis, and make it—what it ought to be—the most reliable exponent of Southern medical opinion and progress.

Of late, too large a proportion of space has been occupied —to the exclusion of interesting matter of a more widely acceptable character—with the discussion of the origin and history of a disease in which the majority of our readers in the interior are much less interested than those residing near the seaboard. Now, although our geographical position affords us perhaps the best opportunity for the study of yellow fever, and the determination of disputed points as to its character, mode of extension, etc., etc., and imposes on us, therefore, an obligation, neither to be refused nor neglected, to contribute somewhat largely towards this important work, we shall hereafter restrict both the number and length of essays upon this topic within appropriate bounds, and thus do more impartial justice and give greater satisfaction both to our contributors and readers. We take occasion, moreover, to state, in this connection, that although we will gladly insert, without favor or prejudice, any expression of opinion whose scientific value we can commend, *personal polemics will henceforward be carefully and absolutely excluded.*

The absence of the accustomed portrait which, for some years past, has constituted so marked a feature in each number of the Journal, cannot fail to be noticed, and may, perhaps, excite some surprise. But the cost of such embellishments forms no inconsiderable item in the current expenses of the year, and as the most rigid retrenchment has become

necessary in order to live and "pay the printer," we have commenced the reform by cutting off such luxuries, until a replenished treasury will warrant a re-indulgence. To be straightforward, the Journal's expenses have so far fallen *far short* of its receipts, and although we never anticipated any pecuniary recompense for our labor, the res angusti domi, make it imperative that it shall support itself or perish. We cannot afford to purchase the privilege of working. When we undertook the conduct of this periodical, our very first step was to cut off a large number of non-paying debtors, who would take no notice of repeated appeals to their sense of fairness and honesty, and we determined, thenceforward, to erase from our list the names of all those who became indebted for *two* years and over; except in particular cases where satisfactory reasons for further indulgence were offered. This is the only method of self protection left us; collection is well nigh impossible where the party indebted refuses to respond to demands made by letter. This rule we have faithfully kept thus far, and shall rigidly adhere to in the future. If the Journal fails in consequence, the shame will not be ours.

In every material regard, the present number is, we think, a decided improvement on any previously edited by us, and its successors shall certainly not fall below the standard here attained.

If you agree and are ready to co-operate with us, readers all, whose accounts are still unbalanced for the year whose burial you have, we trust, solemnized with that cheerful melancholy and pomp of woe which befits the time-hallowed memories and associations of the season, send, we pray you, some material acknowledgement of your satisfaction with the new features of the old friend who bears you, one and all, a most sincere and hearty greeting. Let each subscriber, as he receives this New Year salutation and entreaty, ask himself the question, "have I contributed my share towards the encouragement and support of this work, in whose preparation, for *my* pleasure and profit, another has expended no small proportion of his time, labor, and means?"

If you can answer in the affirmative, accept the Editor's most grateful thanks and acknowledgements; if not, then let each one of such, before passing days and impressions have made him forgetful or careless, forward the trifling sum for which he is indebted, and thus, his own conscience approving and rewarding the honest act, may *we* stand upon the threshold of the next editorial year, not, as now, oppressed with doubts and fears as to its final issue, but invigorated with new hopes supported by past assurances, and excited to larger and nobler exertion by the stimulus of certain appreciation and reward. Upon your actions, remember, the alternative depends!

THE YELLOW FEVER AT FORT MOULTRIE.

To the Editor of the Charleston Medical Journal and Review:

DEAR SIR:—Under the Editorial head of the November number of your Journal, page 844, I find the following, viz: "On the 28th of August the Catawba again arrived from Havana. One of the passengers, Holden, a private in the United States army, who had just obtained his discharge from Key West, bribed a boat to take him to Sullivan's Island, went up to the Fort and enlisted. He was taken sick on the same day, (the 1st of September,) and died on the 5th with black vomit. Of the two nurses who attended him, Ray died on the 12th, and Woolf on the 13th or 14th of the same month, both having thrown up black vomit. Bright, a patient suffering from chronic Rheumatism in the same room with Holden, was taken sick on the 10th, and died on the 17th with black vomit. These were the only three men who had come in contact with Holden during his sickness; the physician of course excepted. The disease afterwards spread through the garrison and proved extremely fatal."

The Editor of the Southern Episcopalian has copied your

editorial in fall into the December issue of that periodical. The statement that the yellow fever was introduced into the garrison of Fort Moultrie,* *and consequently on Sullivan's Island*, by Holden, who had escaped from the Catawba, then at quarantine station in the Harbor of Charleston, will be circulated through the medium of these two journals, throughout the United States and Europe. Allow me the opportunity, through your columns, to correct this part of your editorial, and say that Holden was not the first case of yellow-fever in the garrison.

On the 12th of August, Jones, a private of Company E, 1st Artillery, serving as Orderly to the commanding officer, was taken sick, and died on the 16th of a *malignant type of yellow fever.* He threw up black vomit and died in convulsions.

The second case was a servant girl living at *Col. Gardener's*, who died a few days after Jones,—about the 21st or 22d of August.

Third case. "Sergeant McMahan of Company H, 1st Artillery, was attacked on the 20th of August, and although the disease was well marked, *it was a mild type and he recovered.*"

The fourth case, "Sergeant Renahan of Company H, was attacked on the 26th of August. The disease, in this case, made its incursion with very violent symptoms, but *soon yielded to treatment.*"

The next case was Holden taken on the 1st and died on the 4th of September.

It will be seen from the above cases, that before the steamer Catawba, on which Holden was a passenger, arrived in the port of Charleston, there had been four cases of yellow fever on Sullivan's Island, three in the garrison, and the female servant of the Col. Commanding; and one of them, Jones, even before the city of Charleston was supposed to be infected, certainly before the fever was considered epi-

* The italics are our own.—(Ed.)

demic. The first admissions into the Roper Hospital were on the 11th, and the second on the 12th of August. And in private practice some thirteen cases between the 1st and 12th. Many of these proved to be imitative, for they died of black vomit from subsequent attacks towards the close of the season.

At some future period I will give you a few facts in relation to Abbott's case which occurred in July. Strange to say, that on the premises, next to Abbott's residence, there were three cases of yellow fever in 1857, one of these proving fatal.

In justice to those entrusted with the execution of the quarantine laws, in the harbor of Charleston, and the diffusion of correct medical statistics, I respectfully request that you will give this paper a place in your valuable journal.

Very truly your friend,

ROBERT LEBBY,

Chairman of Committee on Quarantine, City of Charleston.

It gives us great pleasure to insert the above letter, whose objects are purely impersonal, and which we gladly print as information. Without wishing to provoke, or intending to become a party to any controversy, we must be permitted to correct one or two of its statements and inferences, in bare justice to ourselves.

Imprimis, the paragraph above quoted by Dr. Lebby, contains all that was said in our last number, of the yellow fever epidemic of 1858, on Sullivan's Island. The careful reader will observe that the *Garrison of Fort Moultrie* is alone spoken of. Holden's and the immediately successive cases reported, are offered merely as another and excellent proof of the *contagiousness* of this disease. Cases may or may not have previously occurred on the Island, but being careless of details we should always be satisfied of their source, if yellow fever existed antecedently in the city; a depot of infection within a short distance, of most easy access, and from which, in summer, a stream of passengers, with their attendant baggage, is continually pouring to the former. No cord-

on sanataire, however carefully drawn, or strictly enforced, can be perfectly effective. As an opportune example we may offer the one above quoted, of Holden, who escaped from the Catawba *two* days after his arrival in our harbor, from an infected port, *in spite of our rigid quarantine.*

2d. *Jones was not taken sick in the garrison but at Col. Gardener's* house, where he was residing. He was sent to the Fort, after his attack, and when far advanced in the disease. At the date of our editorial, or at the farthest, but a short time before it was written, his case was entered upon the hospital books as *Bilious Fever.* From his medical attendant we afterward, received the same information in regard to the character of the fever with which he was attacked. We certainly had no right—whatever doubts we might have entertained—to go behind this explicit record, and personal voucher. We may mention, as information, however, that according to the hospital steward, he had visited the city more than once.

"The second case," we pass by, as from the first our statements related to, and we are still only concerned with the garrison.

3d. Of Sergeants McMahan and Renahan we have had no previous account. Their names were not mentioned, when careful and close examination of the earliest cases occurring in the garrison, was made by our informer, a medical gentleman of rare accuracy both of observation and statement, and not usually baffled by either the ignorance or reticence of examinees. We will only add, in regard to them, that, if cases of yellow fever, they are singular exceptions to the usual mortality which marks, almost invariably, the earliest cases of a malignant epidemic. Exhibiting a special susceptibility to the influences or agents—whatever they be—which produce yellow fever, they show an equal power of withstanding and recovering from the baneful effects of these, after the first shock; contrasted conditions not easily reconciled. It is interesting to observe in this connection that the three soldiers attacked after *Holden's* introduction into the garrison, *all died.*

ABSENCE OF UREA IN THE URINARY SECRETIONS IN YELLOW FEVER—RECLAMATION OF DR. PORCHER.

We have received from Dr. F. Peyre Porcher, a paper—which will appear in our next—qualifying one of his observations, recorded in an article previously published in this Journal, respecting the absence of urea in the urinary secretions of yellow fever. Examinations made during the past summer have convinced him that it is not absolutely, nor always absent in that disease; i. e., the crystals of nitrate of urea formed upon adding nitric acid to the urine. He contends, however, that the general course of reasoning adopted in the first paper, respecting the pathology and treatment of the disease, is by no means invalidated; and that the error was not absolute, but only one of degree. Want of space alone has compelled us to defer the publication of this and several other interesting essays, which have for some time been awaiting the type and ink of the printer.

NARCOTISM IN TRISMUS NASCENTIUM.

Mr. Editor:—A case of trismus nascentium recently fell under my care, which recovered entirely. I submit it to you for publication, merely for the benefit of others, claiming no credit for my success, as I used the remedy recommended by Drs. Gaillard and DeSaussure; though I must say it was pushed rather farther than they advised. A principle was established by the treatment, which I think, important in this disease, viz: as it is strictly a nervous disease, and usually a fatal one, we are justified in pushing anodynes to the furthest extent, short of poisoning, and compatible with life.

A child, well formed and thrifty, of nine days old, was

seized with all the symptoms of trismus. It was immediately put on one drop of Black drop and this repeated every three hours until symptoms of complete narcotism were produced, which occurred after the third dose. The remedy was now intermitted for four hours, and then continued pro re nata, so as to maintain the system under its influence for 72 consecutive hours. During the intervals of administering the anodyne, cannabis indica, in the largest doses the system could bear, was given in mucilage. The child was nourished with milk extracted from its mother's breast, and fed by spoon, as it was too much narcotized to draw it.

In 24 hours, the spasms became less violent—in 36, decidedly less so, and in 48 hours they entirely disappeared.

The treatment was maintained for 24 hours longer, making 72 hours in all, to ensure against a recurrence of the spasms. The infant is now doing well.

Respectfully yours,

S. M. DeSaussure, M.D.

Beaufort, S. C., Dec. 6, 1858.

PAINE'S INSTITUTES OF MEDICINE; FOURTH EDITION.

In the July number, 1858, of this Journal, we reviewed the erudite and elaborate volume, whose title we give above, which on reaching its fourth edition, by reason of the emendations, and important additions made to it, had risen almost to the dignity of a new work.

The first copy was slightly marred by a few errata, enumerated in a slip attached to the volume; they had by some accident escaped notice in the body of the work. Prof. Paine has therefore been at the trouble and expense, with his usual carefulness and methodic nicety, to have a new impression struck off, in which the errata are all corrected, and the lat-

ter, of which the author has been kind enough to forward us a copy, is now offered to the public without a blemish. It gives us great pleasure to notice this rare example of an author's regard and respect for the critical eye and taste of his readers.

MEDICAL COLLEGE OF THE STATE OF SO. CA.

The most sanguine friends of this Institution could not have anticipated that it would occupy so gratifying a position as it has this session maintained, in the face of a combination of misfortunes, numerous and severe. The withdrawal of two of its most eloquent, attractive and honored teachers from its professorial corps, was itself a shock severe enough to have staggered profoundly the most ably supported college, and it would have been matter of no surprise if years were required to recover that full measure of confidence in its resources and capacity which changes, however trivial, always impair.

But when to this element of detraction was added the injurious influence of an unustually malignant epidemic—possessing, above all others, a terror not to be subdued for those who do not reside within its territorial domain—which maintained its sway through a singularly protracted period, and for some time after the lecture term had commenced, it was generally agreed that the chances in favor of even a moderately respectable class were extremely few.

To the surprise and delight of all interested, however, these expectations were not fulfilled, and it gives us pleasure to announce that the class for 1858 and '59 already numbers nearly as many as that collected after the summer of 1854, when the ravages of yellow fever were much less terrible, and ceased at an earlier date. A good number of students, too, who had attended a previous course of lectures in this

College, but whom the above mentioned circumstances had induced to visit other institutions, have, after a personal trial and comparison of their respective advantages, returned to their first mother, with an unconcealed satisfaction which speaks trumpet-tongued her influence, and is the highest possible testimony to her worth.

Our predictions respecting the ability and success of the new Professors have moreover been abunduntly verified, if the warm enconiums, uniform attendance, and respectful attention of the Students can be considered proofs. It required, indeed, no prophet to anticipte this, when the enthusiasm, zeal and diligence which they have brought to their new labors were foreknown.

The means for clinical instruction have also been improved, enlarged, and so methodized as to be rendered of the greatest possible value, and in every department the Medical College of South Carolina has neglected to avail herself of no accident which might ensure that success which she has always deserved, and we trust may long continue to enjoy.

STATISTICS OF THE PREVALENCE OF RICKETS AT DRESDEN.

Dr. Kuttner states, that rickets is extremely common in that city. Out of 9000 patients admitted into the Children's Hospital for the last twenty years, 1654 were rickety or had been so, the latter presenting unmistakable traces of the disease. Thus it would appear that the proportion of rickety children was 20 per cent.; and by leaving out from the total number of 9000 such children who were less than one year old, and could not as yet be affected with any symptoms of rickets, the proportion rises to 25 per cent. Dr. Kuttner thinks that the cause of this state of things is the wretched condition of the laboring classes of Dresden, as that city is

not worse off, in a hygienic point of view, than other capitals. It is rather remarkable that out of these 1654 rickety children, 198 belonged to journeyman shoemakers, and 162 to journeyman tailors, being 25 per cent of the whole number. Twins and children prematurely born were rather numerous. As to age, it was found that the largest number were from two to three years old. The influence of the mother's milk does not seem to have been well marked; for 387 had not had the breast, or had been suckled but a very short time; 360 were nursed for the ordinary period; and 455 for a longer time than usual. As vaccination has been accused of inducing rickets and scrofula, it will be interesting to notice that out of 1258 rickety children, 759 had not been vaccinated; 491 had undergone the operation; and 35 had had the small-pox. The winter months were noticed to bring the largest number of rickety children into hospital. Dr. Kuttner finally considers that rickets may be owing to the early use, with children of much bread and potatoes.—*Journal fur Kinderkrankheiten*, Nos. 7 and 8, 1858.

IMPORTANT TO SUBSCRIBERS.

We would respectfully invite the attention of our subscribers to the change in our terms, by which only those paying their subscription, *before the issue of the May number*, will be entitled to the Journal for the entire year, *free of postage*. As heretofore, *five* dollars will be invariably charged where payment is withheld until after the publication of the July number. Every inducement is thus held out for early payment; we trust that all of our subscribers will avail themselves of the advantages to be thus secured.

RETURN OF DEATHS IN THE CITY OF CHARLESTON,

For OCTOBER, 1858.

WHITES.		BLACKS AND COLORED.	
Abcess of Throat,	1	Abscess,	1
Apoplexy,	2	Apoplexy,	5
Birth, Premature	1	Brain, Congestion of	1
Brain, Congestion of	1	Cancer of Penis,	1
Cancer,	1	Convulsions,	7
Cancer of Uterus,	1	Consumption,	2
Cholera Infantum,	1	Colic,	2
Convulsions,	2	Croup, Membranous	1
Consumption,	8	Debility,	1
Croup, Membranous	1	Dropsy,	4
Debility,	1	Epilepsy,	1
Delirium Tremens,	1	Fever, Remittent	1
Dropsy,	1	Fever, Congestive	3
Dysentery,	1	Fever Catarrhal,	1
Enteritis,	1	Fever, Typhus	1
Epilepsy,	1	Fever, Bilious	1
Fever, Intermittent	1	Fever, Yellow	9
Fever, Remittent	1	Hemorrhage,	1
Fever, Congestive	1	Hydrothorax,	1
Fever, Typhus	1	Jaundice,	1
Fever, Typhoid	6	Lungs, Congestion of	1
Fever, Country	1	Marasmus,	4
Fever, Scarlet	2	Old Age,	3
Fever, Yellow	156	Pleurisy,	1
Gastritis,	2	Pneumonia,	2
Gastro-Enteritis	1	Tetanus,	1
Heart, Disease of	1	Teething,	1
Marasmus,	5	Trismus Nacentium,	4
Old Age,	6	Tuberculosis,	1
Paralysis,	1	Unknown,	1
Pneumonia,	1	Wound, Pistol	1
Trismus Nascentium,	3		
Want of Vitality,	1	Males, 36; Females, 29. Total,	65
Males, 129; Females, 86. Total,	215		
Ages.—Under 1 year,	14	Ages.—Under 1 year,	13
1 to 5,	22	1 to 5,	7
5 to 10,	7	5 to 10,	2
10 to 20,	25	10 to 20,	5
20 to 30,	66	20 to 30,	9
30 to 40,	49	30 to 40,	3
40 to 50,	12	40 to 50,	5
50 to 60,	4	50 to 60,	6
60 to 70,	8	60 to 70,	4
70 to 80,	5	70 to 80,	6
80 to 90,	2	80 to 90,	2
90 to 100,	1	90 to 100,	2
		100 and over,	1
Total,	215	Total,	65
Natives of Charleston,	48	Natives of Charleston,	36
Natives of United States,	40	Natives of United States,	28
Foreigners,	127	Foreigners,	1
Total,	215	Total,	65

J. L. DAWSON, M.D.,
City Registrar.

RETURN OF DEATHS IN THE CITY OF CHARLESTON,

For NOVEMBER, 1858.

WHITES.		BLACK AND COLORED.	
Anasarca,	1	Apoplexy,	4
Apoplexy,	1	Brain, Congestion of	2
Brain, Congestion of	2	Brights Disease of Kidney,	1
Brain, Softening of	1	Cancer,	1
Convulsions,	7	Convulsions,	1
Consumption,	7	Consumption,	7
Croup, Membranous	2	Croup, Membranous	2
Debility,	1	Debility,	1
Diarrhæa,	1	Dysentery,	1
Dropsy,	1	Dropsy,	5
Dysentery,	1	Elephantiasis,	1
Drowned,	2	Enteritis,	1
Enteritis,	1	Fever, Typhus	1
Fever, Typhus	1	Fever, Scarlet	1
Fever, Typhoid	1	Fever, Yellow	4
Fever, Country	1	Gastritis,	1
Fever, Scarlet	3	Gastro Enteritis,	1
Fever, Yellow	16	Hepatitis,	1
Gastritis,	1	Jaundice,	1
Heart, Disease of	1	Lungs, Congestion of	2
Hemiplegia,	1	Marasmus,	3
Hepatitis,	1	Old Age,	2
Injury,	1	Paralysis,	1
Jaundice,	2	Pneumonia,	2
Mania a Potu,	1	Teething,	1
Old Age,	4	Tetanus,	1
Paralysis,	4	Trismus Nascentium	1
Rheumatism,	1	Ulcer of Stomach,	1
Teething,	2	Unknown,	1
Unknown,	1		
Males, 50; Females, 20. Total,	70	Males, 27; Females, 25. Total,	52
Ages.—Under 1 year,	10	Ages.—Under 1 year,	7
1 to 5,	10	1 to 5,	10
5 to 10,	5	5 to 10,	2
10 to 20,	4	10 to 20,	5
20 to 30,	14	20 to 30,	4
30 to 40,	9	30 to 40,	4
40 to 50,	5	40 to 50,	6
50 to 60,	3	50 to 60,	6
60 to 70,	3	60 to 70,	5
70 to 80,	5	90 to 100,	3
80 to 90,	2		
Total,	70	Total,	52
Natives of Charleston,	31	Natives of Charleston,	35
Natives of United States,	17	Natives of United States,	15
Foreigners,	22	Foreigners,	2
Total,	70	Total,	52

J. L. DAWSON, M.D.,
City Registrar.

METEOROLOGICAL ABSTRACT FOR OCTOBER, 1858.

BAROMETER.

Maximum, 30.544, - - Minimum, 30.000, - - - Range, 0.544

THERMOMETER ATTACHED.

Mean.

7 A. M., 68.54, - - - 2 P. M., 73.83, - - - 9 P. M.,71.41

THERMOMETER DETACHED.

Mean.

7 A. M., 66.64 - - - 2 P. M., 74.22, - - - 9 P. M., 69.86

Highest Degree.

7 P. M., 75, - - - 2 P. M., 82. - - - - 9 P. M., 77

Lowest Degree.

7 A. M., 57, - - - 2 P. M., 67, - - - - 9 P. M., 63

REGISTER THERMOMETER.

Mean.

Night, 64.32, - - - - - - - - - - - Day, 75.58

DEW POINT.

Mean, - - - - 62.19

WINDS.

Sun Rise—N. 1 day; N E. 17; N.W. 1; S.E. 4; S.W. 7; E. 1. Prevailing N.E.
4 *P. M.*—N.E. 13 days; N.W. 1; S. 2; S.E. 4; S.W. 7; E. 4. Prevailing N.E.

WEATHER.

Fair, 20 days—Cloudy, 6 days—Rain, 5 days—Prevailing, Fair.

QUANTITY OF RAIN.

Inches, - - - 0.77.

JOHN L. DAWSON, M.D.

METEOROLOGICAL ABSTRACT FOR NOV., 1858.

BAROMETER.

Maximum, 30.460, - Minimum, 29,840, - - - Range, 0.620

THERMOMETER ATTACHED.

Mean.

7 A. M., 53.66, - - 2 P. M., 57·66, - - - 9 P. M., 56.30

THERMOMETER DETACHED.

Mean.

7 A. M., 49.30, - - 2 P. M., 57.23, - - - 9 P. M,, 52.90

Highest Degree.

7 A. M., 70, - - - 2 P. M., 76, - - - - 9 P. M., 71

Lowest Degree.

7 A. M., 35, - - - - 2 P. M., 44, - - - 9 P. M., 39

REGISTER THERMOMETER.

Mean.

Night, 44.83, - - - - - - - - - - - Day, 58.60

DEW POINT.

Mean, - - - 44.40.

WINDS.

Sun Rise—N.E. 10 days; N.W. 9; S.W. 6; E. 1; W. 4. Prevailing N.E.
4 *P.M.*—N. 1 day; N.E. 10; N.W. 8, S.E. 1; S.W. 6, W. 4. Prevailing N.E.

WEATHER.

Fair, 18 days—Cloudy, 3 days—Rain 9 days—Prevailing—Fair.

QUANTITY OF RAIN.

Inches, - - - - 2.93.

JOHN L. DAWSON, M.D.

TO CORRESPONDENTS AND PUBLISHERS.

Communications have been received from Drs. Kollock, DaCosta, Porcher, Ogier and DeSaussure, and will appear in our next.

The following books and pamphlets have been received:—

Watson's Practice of Physic, Blanchard & Lea, Philadelphia.
Moreland on the Urinary Organs, Blanchard & Lea, Philadelphia.
Wilson's Human Anatomy, Blanchard & Lea, Philadelphia.
Green on Bronchitis, Wiley & Halsted, New York.
Prescriptions of American Practitioners, Wiley & Halsted, New York.
History of Prostitution, Harper & Brothers, New York.
Paine's Institutes of Medicine, Harper & Brothers, New York.
Tyler Smith's Lectures on Obstetrics, Robert M. DeWitt, New York.
Post-Pleiocene Fossils of South Carolina, Russell & Jones, Charleston.
Harris on the Climate and Fevers of the Southern States; pamphlet.
Introductory Lecture to the Course of 1858–59, in the University of Nashville, by Prof. Lindsley, pamphlet.
Hints to Craniographers, by Prof. G. A. Meigs, pamphlet.
Report of the President of the Howard Association, Charleston, pamphlet.
Upham on Typhus Fever in Great Britain, pamphlet.
Taylor on the treatment of Paralysis of Motion, pamphlet.

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The plan of the work is such in the present issue, that the name of the patient requires to be but once written for the whole year; and though the book is smaller than last year, by over fifty pages of the blank work, the physician's purposes are much better served, and a larger business can be recorded. Two sizes of the work are published, the same as last year; price for the small size, $1,25; for the large size, $1,50. W. A. Townsend & Co., 377 Broadway, New York.

N. B. The book ordered of the author, No. 68, West 26th street, New York, will be sent to any part of the country, postage paid, on the reception of the price, in current money or postage stamps.

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H. P. DEWEES.

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I remain, yours respectfully, JAS. R. BOARDMAN, M.D.,
Resident Physician and Surgeon.

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Baptisin,	Baptisia Tinctoria,	1 00
Caulophyllin,	Caulophyllum Thalic,	0 75
Cerasein,	Cerasus Virginiana,	1 50
Chelonin,	Chelone Glabra,	1 25
Chimaphilin,	Chimaphila Umbellata,	1 00
Columbin,	Fraseria Carolinensis,	1 0
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Erythroxylin,	Erythroxylum Coca,	0 00
Euonymin,	Euonymus Americanus,	1 50
Euphorbin,	Euphorbia Carolata,	1 50
Eupatorin, (Perfo.)	Eupatorium Perfoliat,	1 00
Eupatorin, (Perpu.)	Eupatorium Purpureum,	1 50
Galegin,	Galegæ Virginiana,	0 00
Gelsemin.	Gelseminum Sempervi,	2 00
Geranin,	Geranium Maculatum,	0 62
Hamamelin,	Hamamelis Virginica,	1 00
Helonin,	Helonias Dioica,	1 75
Hydrastin,	Hydrasis Canadensis,	1 25
Hyosciamin,	Hyosciamus Niger,	2 50
Irisin,	Iris Versicolor,	1 00
Jalapin,	Ipomœa Jalapa,	1 00
Juglandin,	Juglans Cinerea,	0 75
Leptandrin,	Leptandria Virginica,	0 75
Lupulin,	Humulus Lupulus,	1 00
Lycopin,	Lycopus Virginica,	1 00
Macrotin,	Macrotys Racemosa,	0 62
Menispermin,	Menispermum Canad.	1 00
Myricin,	Myrica Cerifera,	0 62
Phytolacin,	Phytolacca Decandra,	1 00
Podophyllin,	Podophyllum Peltatum,	0 75
Populin,	Populus Tremuloides,	0 50
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Rhusin,	Rhus Glabrum,	1 00
Rumin,	Rumex Crispus,	0 75

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Strychnin,	Strychnos Nux Vomica,	3 00
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Physician to the Demilt Dispensary, formerly House Physician to Bellevue Hospital.

AND

WM. R. DONAGHE, M. D.,

Surgeon to the Demilt Dispensary, formerly House Surgeon to the Bellevue Hospital.

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N. B.—The Winter Session will commence October 15th, and continue during the Terms of the Colleges.

CHARLESTON

MEDICAL JOURNAL AND REVIEW.

VOL. XIV.] CHARLESTON, S. C., MARCH, 1859. [No. 2

ART. I.—*A Case of Cerebral Neuralgia terminating fatally by Serous Apoplexy, with Remarks on the occurrence of Serous Apoplexy.* By J. DA COSTA, M.D., Lecturer on the Practice of Medicine in the Philadelphia Medical Association, etc.

Mrs. S., aged 30, was admitted into my ward at the Episcopal Hospital, on the 8th of August, 1858, for attacks of neuralgia and headache, to which she had been subject for four months. There was no reason to suppose that she had ever had syphilis, nor did she admit having suffered from intermittent fever. Her health had commenced to give way under the continuance of the disease; she was pale and feeble and had a distressed look of countenance. Her throat had been sore for two weeks; but no signs of any important organic disease were discovered. The lungs were healthy; the impulse of the heart not distinct, but the sounds normal. The tongue was covered with a yellowish fur, the appetite was bad, and the bowels constipated. Slight tenderness on pressure existed over the epigastric region; the percussion dulness over the liver extended below the margin of the ribs, and was accompanied by a greater feeling of resistance than is usual. The pulse ranged between 75 and 80 and was small and weak. The skin was relaxed with a disposition to perspire; the eye was clear, the pupils dilated. Neither motion nor sensation were in the slightest degree impaired. Her

intelligence was good, and excepting restlessness and peevishness, no symptoms of disturbance of the nervous system were noted. The spinal column was at no portion sensitive on pressure. The urine was copious in quantity and, as far as it was tested, not abnormal. She complained of amenorrhœa and of leucorrhœa, and displacement of the womb; but a careful per-vaginam examination made by the then resident, Dr. H. Y. Evans, disclosed a perfectly healthy condition of the parts, with little discharge and no displacement. The attacks of headache and neuralgia were observed to be neither persistent nor equal in severity. At times they lasted only for a few minutes, and hours elapsed before their recurrence; at others they were of longer duration, and occurred at such short intervals as to seem continuous. They appeared mainly at night, between the hours of nine and two, and were marked by great pain in the forehead, by intense restlessness, by a disposition to talk, to scream, and had, indeed, many of the characters of an hysteric paroxysm. The pain was accompanied by a slightly increased sensitiveness of the skin of the face, and sometimes by irritability of the stomach. It was acute, was seated above the brows, shooting to the hair and back of the head, and down over the face and neck, and although felt on both sides, seemed much more severe on the right. From this reason, joined to the fact that the usual painful spots of neuralgia of the fifth nerve were absent, that there were no signs of an affection of the facial nerve, or of any chronic cerebral lesion, the conclusion arrived at was that the case was to be viewed as one of neuralgia, or cephalalgia, connected with disordered stomach and liver, occurring in an hysteric person, and approximating, if a name were to be applied to it, to hemicrania, the neuralgia cerebralis of Romberg.

A week after her admission, under the use of astringent gargles and purgatives, her sore throat had disappeared, her tongue cleaned, and her bowels became less constipated. She took tincture of hyoscyamus in camphor water at night, and was placed upon nourishing diet and six drops of the solution of the arsenite of potassa three times daily, but no benefit

being perceived from the latter, pills of carbonate of iron and sulphate of quinia were substituted. The attacks became less frequent, whilst an improvement in her appetite and general health occurred.

During the night of the 23d, she had again a very hard paroxysm, and sat up nearly all night, evidently in great pain. A mustard plaster, and subsequently a blister, were applied to the back of the neck, but without effect. Towards sunrise she became composed, and slept quietly during the forenoon of the following day. At eight o'clock, P. M., she was again seized with intense headache; her eyes were fixed, she screamed aloud, was trembling and frightened; her skin was cold, and covered with a profuse perspiration. For several days, her attacks were of the same nature—always bad at night, but always mitigating in severity, and almost entirely disappearing in the morning; leaving her in the intervals tired and excitable, but free from pain, excepting that at times she complained of a feeling of soreness at the back of the head and neck. In one of these paroxysms on the 27th her head was drawn to the right shoulder, her masseter muscles were rigidly contracted, and her mouth could only be opened by applying ice to the cheeks. Mustard pediluvia and hot frictions to the spine did not allay her evident distress. Yet at my morning visit on the 28th, she was quiet and disposed to doze, although she felt nauseated, and had vomited her breakfast. I directed her a mixture of Acid hydrocyan. gtt. xxiv., Aq. Camph. ℥iij., Fluid Ext. Valer., ℥iij., Morph. acetat., gr. i.; to be given in table-spoonful doses every two hours, if the attacks recommenced. In the afternoon they did, and a few doses of the mixture were administered to her with the happiest results. She became calm, and slept well all night. On the 29th and 30th, she was exempt from attacks. The iron and quinia pills were continued, and wine added. Her appetite improved markedly, and up to the 5th of September a continual amelioration of her health took place. She walked about daily for hours in the garden attached to the hospital, and complained of no pain, except occasionally of a little over her eye, and on the

right side of the face, but showed strongly that disposition to deceive, which is so strikingly manifested in some hysteric patients.

On the 5th of September, the cephalalgia resumed its former intensity. It was noted that she was slightly feverish, and could not retain anything on the stomach. On the 6th, the spells were very frequent, and she was much excited. The prescription of the 28th of August was used, but without benefit. Vomiting was not a prominent symptom, but the next day it became so, and resisted all the remedies employed for its removal. She passed a miserable night, talking, screaming, and keeping all the patients in the ward in a state of alarm. Her cephalalgia was excruciating. She tried, and seemingly with some success, to relieve it by twisting a towel around her head, and several times, when asked as to its seat, she exclaimed, "there, there, inside," pointing to her brows, "the doctor thinks it is outside, but it is not." Seizing hold of persons standing by, her features distorted, her teeth grinding, her head drawn to the right shoulder from rigid muscular contraction, she implored for help against the death which she believed imminent.

In the morning, when I saw her, she was, however, sitting up in bed perfectly collected. The skin and tongue were dry, the bowels constipated, the pulse a few beats above eighty. The same old restlessness was marked, and the same fear of coming death. Occasional vomiting of a darkish matter still continued. The wine and tonic pills were stopped, and purgatives administered. She had several attacks in the afternoon and evening. During the ninth, a slight fever, if such a dry skin and a pulse of eighty can be called, was, as the day before, observed in the morning. The paroxysms succeeded each other rapidly, but I do not think that except in their frequency they differed from those she had had weeks previously. The impression of the Resident, who watched her very carefully, is, indeed, that they were less severe. During the day of the 10th, she had few and very slight attacks, but appeared excited and restless, although she talked less than usual. The bowels had been opened, the urine was

natural, and she ate her dinner with some relish. The oft-repeated question, "doctor, am I going to die?" and her desire not to let the Resident leave her, were the two main thoughts to which she gave utterance. At about 7, P.M., she stretched herself out in bed, and declared that she felt death creeping over her. But she soon got up again, and appeared as usual. At nine o'clock, she was sitting erect in bed, with her feet hanging out, expressing herself strongly on the fact that her husband, whom she had expected in the evening, had not made his appearance, when suddenly, again stating that she felt death approaching, she laid herself down deliberately in bed, and to the jesting remarks of the patients, who, accustomed to hear her declare she was dying, attached little importance to her actions, maintained an unbroken silence.

Finding that she did not answer, they became alarmed. The House physician was sent for; he found her perfectly unconscious, her face and forehead quite cool, her eyes fixed, the pupils contracted, the limbs extended in bed. She drew a few long breaths, then her breathing became so quiet as not to occasion any perceptible movement of the thorax. The pulse was distinctly felt, and was only beating forty times a minute. The mouth was slightly open, but there were no signs of paralysis of the facial muscles, nor of foam on the lips. When the arm was lifted up, it appeared somewhat rigid, but dropped back immediately to its former position. It seemed soon as if the respiration had ceased, for a mirror applied over the mouth was not in the slightest degree clouded. She remained in this condition for upwards of twenty minutes, not a limb moving, and without a gleam of consciousness. No involuntary evacuations took place; indeed, an injection that was administered was retained. Attempts at artificial respiration by Marshall Hall's method were kept up for ten minutes, with the sole result of quickening the pulse, which rose at one time up to 70, and then diminished again in frequency and strength until it disappeared. The pupils did not dilate until the heart's action could no longer be distinctly distinguished. It was impossible to fix the exact moment of her death, but it occurred in less than half an hour after the seizure.

AUTOPSY FOURTEEN HOURS AFTER DEATH.

Thorax.—The lungs were healthy, and rather pale. The heart was small, its walls firm. On the right ventricle was some fat, but it did not extend into the muscular structure; the valves were perfectly sound. The heart contained no clots; on cutting the vessels around it, the blood which poured out was observed to be very dark and very fluid.

Abdomen.—The liver was large and heavy ; on its capsule were a few thickened spots; the liver structure itself was healthy. So were the intestines. The stomach presented evident marks of chronic inflammation, its mucous membrane was prominent in parts and slate-colored. A few points of more recent injection existed—they were probably post mortem. The kidneys were small, rather firm; both cortical substance and tubules were perfectly healthy.

Head.—Upon removing the thick and dense scull-cap, the dura mater was found sound. The arachnoid, and pia mater were injected with a darkish blood, yet by no means to a great extent. The brain itself was perfectly healthy, not vascular; at no part was there extravasation. The cerebellum was examined with care, and no signs of increased vascularity detected. The base of the brain, and the nerves proceeding from it, were in every respect normal. The roof of the left lateral ventricle was strongly arched. On cutting through it a large quantity of clear serum ran out. The ventricle was much distended, especially at its middle corner; so was the right ventricle, but not so much as the left. The two ventricles communicated freely. In one, the choroid plexus was engorged, in the left it appeared empty. On pricking the lateral sinus, a large quantity of a dark fluid blood escaped, exactly like that contained in the veins within the thorax. The quantity of serum the ventricles contained was nearly four ounces; certainly more than three. There was no effusion between the convolutions of the brain. The brain substance itself was firm; at no portion was the slightest trace of previous disease met with. The wall of one of the ventricles appeared softer than the surrounding texture, but it was

evidently caused by the imbibition of the serum, since, examined with the microscope, the tissue was not perceived to deviate in any way from the normal structure.

The points of interest in this case are derived partly from the symptoms prior to death, and partly from those which attended the fatal termination. I have described it as a case of neuralgia; in so doing I have used the term in a wide sense. It is evident that some of the symptoms were those of an hysteric female, but it is also evident that actual intense pain existed, referable to some positive disturbance of the brain. The pain, as already stated, was neither that of tic douloureux, nor of an affection of the facial nerve. It was, however, disdistinctly paroxysmal, and its radiating character seemed to indicate its neuralgic nature. Although not strictly confined to one side, the attacks resembled much those of hemicrania, to which nervous females, with enfeebled digestive organs, are liable. The pain could evidently not have been owing to a permanent organic affection of the brain, or of its membranes. It was too intermitting in its nature, and neither the motor or intellectual functions were in the slightest degree affected; nor did the autopsy reveal any traces of a chronic cerebral lesion. All speculations as to its immediate cause must of course be futile; yet it may be proper in this connection to suggest, whether the attacks were not preceded or accompanied by active congestion of the brain. In Dr. Copland's work on Palsy and Apoplexy,* several cases are given of suffering from neuralgia of the head and face, evidently connected with hyperæmia, and terminating in apoplexy. In the essays read by Sir Henry Halford at the Royal College of Physicians, similar instances are recorded, and as my patient died by apoplexy, although not sanguineous, and as certainly some of the symptoms were those witnessed in congestion of the brain, there seems to me nothing improbable in the assumption that the paroxysms were connected

* P. 177, Am. Ed.

with vascular injection, which led to the transudation from the vessels, toneless and filled as they were with a watery blood, of the large amount of serum which caused her death. I will not dwell on the curious psychological fact, that she seemed so certain of her approaching doom, nor on the circumstance that death in attacks of this nature are rare, nor will I discuss in how far a tonic, and in how far a derivative treatment ought to be pursued in similar instances, nor is it necessary to do more than direct attention to the muscular contractions which acompanied the paroxysms, and to the singular phenomena witnessed during the act of dying, but I shall pass on at once to a consideration of the chief medical interest which surrounds the case—the form of apoplexy by which the disease terminated.

Serous apoplexy, or death from the rapid effusion of serum, is an affection which is denied altogether by some authors, and only admitted in a spirit of scientific tolerance by others. The works of the older writers are filled with a description of such cases, but the large number of these are not such as would satisfy a rigid critic of the present day. Of all those which Morgagni in his fourth letter has collected, few can be considered undeniable specimens, and even these few, such as the case of Baptista Anguissola the prelate, or of Perrarinio the priest of Verona, or of the ostler, who was twice the subject of an apoplectic seizure, are not free from doubt. The great difficulty consists in every case, in determining how much of the effusion is cause, how much effect, how much attributable to post mortem exhalation, and, again, where to draw the line between death from serous, and death from simple congestive apoplexy.

Dr. Abercrombie, in his well-known work on Diseases of the Brain, although he cites several cases of apoplexy with serous effusion, infers that in them the presence of the fluid cannot be considered as the cause of the apoplectic symptoms, but is to be regarded as the result of that peculiar derangement of the circulation, which constitutes the state of simple apoplexy. He further adds that the distinction between serous and sanguineous apoplexy is not supported

by observation. If this statement be applied exclusively to the symptoms, such as pallor of face, weak pulse, it is true that a diagnosis is uncertain, although even on this point further clinical research is needed. But the first proposition is widely different. Where is the difficulty in admitting that the compression of the brain, which would occur from a serous effusion, be it either in the ventricles, or as sometimes happens, under the arachnoid, may cause the apoplectic symptoms? There is in this assumption nothing more than that it is the effused blood, and not the state of congestion in sanguineous apoplexy, which produces the signs of disturbance. To exchange the explanation of a process which may seem obscure, for another, like death from simple congestion, which is certainly not a whit less so, is not a gain, and it seems strange that nearly all of the present writers should have so unhesitatingly adopted Abercrombie's opinion. There is, moreover, an affection of the lung, acute œdema, which presents strong analogies to serous apoplexy, and surely no one can maintain that it is the congestion of the lung which proves fatal, and that the effusion of serum is the unimportant accompaniment.

Believing, then, that serous apoplexy exists as a separate disease, it is far more difficult to determine the cases to which the term is *post mortem* strictly applicable. Rokitansky,* in the last edition of his Pathological Anatomy, asks the question—Is there really a serous apoplexy, and are we able to diagnose it by an autopsy, without reference to the symptoms which preceded death? He somewhat reluctantly inclines to the view, that such an affection exists, and speaks of its being recognizable as a cause of death:

If the effusion is considerable, and accompanied by a hyperæmia of the meninges;

If a swelling of the brain from œdema exists in the immediate vicinity of the ventricles;

If the individual has not been previously appreciably affected with disease of the brain;

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* Third Edition, Vol. I., p. 453. Vienna, 1856.

If the frequently accompanying anomalies of other organs, especially of the lung, such as hyperæmia, hypostasis, œdema, represent nothing more than the consequences deducible from the phenomena and appearing during the agony of the paralysis of the brain, and are not, therefore, themselves to be regarded as having produced death, nor as having been the cause of the hyperæmia, and transudation within the cranium.

The case just reported answers in every particular the very strict requirements laid down by the distinguished pathologist quoted. There was hyperæmia and considerable effusion; there was a slight œdema immediately around one of the ventricles; there were no lesions in the lungs or in the other organs, which could account for death, and the patient had not had any signs of an undoubted cerebral lesion. In this respect the case is particularly valuable, since the history could be clearly ascertained prior to the fatal event. The short duration, too, of the attack, and the phenomena connected with the respiration and circulation, are remarkable.

In Andral's Clinique Medicale* will be found the account of a young mason, who was admitted into the Charité for headache of several days' duration. The pulse was not frequent, and the headache was the only symptom of note. The next day his intelligence seemed duller, and became more and more so, but his pulse was not rapid, his skin not hot, the pupils were sluggish, and only moderately dilated. They became finally more dilated, the pulse rose, the skin became dry, and profound coma set in. On opening the head, a large quantity of limpid serum escaped. No other appreciable lesion existed in the nervous system, nor in any of the organs of the body. Here evidently the serous effusion had been forming for several days. In the two observations which follow (XXI. and XXII. *loc. cit.*) death was much more sudden. In the one, the patient, who was being treated for a pulmonary catarrh, suddenly lost consciousness, and remained

* Tome V., p. 88. Maladies de l'encephale. Observation XX.

for some hours, until death took place, with the same absence of all motion, slow pulse, opened mouth, as described in my case. In an instance which Cheyne* has detailed, paralysis was confined to the left side of the body, the face was pale, the pulse slow. One lateral ventricle was very much dilated, and full of clear serous fluid; the other contained but a small quantity of fluid, and was of natural size.

One point more requires discussion before the case that I have recorded can be completed. Did the effusion really take place suddenly, or had it been gradually forming? in other words, may it be supposed that the disease was acute hydrocephalus, which commenced perhaps a week or ten days before death, and ran on to a fatal termination? Now it is extremely difficult to say, what the symptoms of uncomplicated acute hydrocephalus are, for modern research has shown conclusively that in the large majority of instances, acute hydrocephalus is merely another name for tubercular meningitis. The main difference between pure cases of acute hydrocephalus and serous apoplexy, consists in one being a comparatively gradual effusion, and in the coma of the first being of more slow occurrence, and preceded by symptoms like those of acute meningitis. If the characteristic of any case of apoplexy be its suddenness, this may surely claim to be such. Nor can it be said that any symptoms existed which were exclusively those of an acute affection of the brain. It is true, there was intense headache, nausea, vomiting at one time, grinding of teeth, always extreme restlessness; but any of these symptoms may be the result of mere cerebral congestion. They were, moreover, intermitting, and not accompanied by a distinct fever, and whether or not slight effusions of serum occurred during the rapidly succeeding paroxysms of the last ten days, the symptoms of cerebral irritation and congestion alone were marked, and the final coma and signs of compression of the brain were as rapid as they were unlooked for. Let us add to this, that not the slightest signs of

* Cases of Apoplexy and Lethargy. London, 1812. p. 125.

an inflammatory process were after death detected, that the attacks were not probably different from those the patient had when she first came into the hospital, and as far as could be ascertained, not different from those prior to admission, and a diagnosis of acute hydrocephalus loses all probability.

ART. II.—*Case of Chancre of the Uterus, reported for the Pee Dee Medical Association at its Annual Meeting in Cheraw, June 9th*, 1858. By C. KOLLOCK, M.D., of Cheraw, S. C. *Published by request of the Association.*

On the third day of March last I was consulted by S—— J——, aged thirty-two years, unmarried, had had one accouchement. She was of rather a short frame and healthy in appearance. The patient complained of a pain, or rather uneasiness, through the loins and hips; stated that she had been for nine months past troubled with Prolapsus Uteri, and that she had at that time a very annoying discharge from the vagina. From her account of her feelings, and her description of the vaginal discharge, I was induced to suspect some uterine affection of an organic nature, and accordingly proposed an examination "p. v."

As soon as the finger was passed into the vagina, I discovered a considerable degree of Prolapsus Uteri, and upon touching the "os tincæ," it presented a roughness and unevenness of surface, different from what I had ever before experienced in a vaginal examination. In order to make clear what was then dark and obscure, I proposed the use of the speculum. On the following morning a bivalve speculum was introduced, and I discovered two well-defined ulcers, one on the parietes of the vagina, about an inch from the vulva, the other occupying the anterior lip of the uterus. The ulcers had, so far as could be ascertained by ocular inspection, every characteristic of the Hunterian chancre. They were of a grayish color, excavated, with margins irregular and ele-

vated; the vaginal discharge was puriform. Not being in search of any venereal disorder, and knowing nothing at the outset derogatory to the character of the patient to warrant even a suspicion of such a thing, and being aware of the extreme rarity of cases of uterine chancre, I must confess that I was slow to believe that I had found one. But the ulcers were so decidedly chancrous in appearance that I could not divest my mind of the idea that they were syphilitic in their origin. I was induced to question the patient as to her previous history; and upon being informed that she had never been married, but had given birth to one child, I proceeded with more care and greater strictness in my investigation, and obtained from her a statement as to the time when she had had a suspicious connexion. This was a full confirmation of my opinion, for the time that had elapsed since the last sexual intercourse was just sufficient to bring the chancres to the stage in which they were when I first saw them. But to make assurance doubly sure, and to remove all chance for cavil at the correctness of my diagnosis, I took pus from the chancre on the uterus, and inoculated both thighs. This was done on the fifth day of March. On the seventh the points of inoculation were red and somewhat swollen. On the eighth, vesicles made their appearance, which on the ninth were filled with pus. On the seventeenth of March the patient had on each thigh a genuine Hunterian chancre. While the chancres produced by inoculation were going through the several stages peculiar to this specific form of ulceration, the one on the uterus, and also that on the parietes of the vagina, were treated by the internal use of the bi-chloride of mercury and an external application of caustic, in the shape of the acid nitrate of mercury. In the course of eighteen or twenty days they were entirely healed. Those on the thigh were treated in a similar manner, and in due time were well cicatrized.

That chancre of the uterus is of exceedingly rare occurrence, every member of the profession, who has had any experience, can testify. From time to time, there are reported what are reputed to be cases of uterine chancre, but upon inquiring into their history and pathology, they often prove

to be cases of uterine ulceration, perhaps of syphilitic origin, but not veritable chancres. The late Cullerier, who was for many years Physician to a venereal Hospital in Paris, reports but three cases as having occurred in the whole of his career.

Gibert, who was for a long time Physician to the Lourcine, states, in a pamphlet that he published some years since, on "Uterine Diseases," that out of five hundred women who he examined with the speculum, one hundred and forty presented "granular erosions" of the uterus, the greater part of which he considered syphilitic in their origin, but not one of them presented the characteristics of the true chancre. Gibert also remarks, that up to the time of the publication of the pamphlet above alluded to, he had seen but three instances of veritable chancre of the uterus. Duparcque admits their extreme rarity, and although he was for a long time in the enjoyment of an extensive practice in the treatment of uterine disease, he is obliged to borrow from other authors the few cases which he gives in his work to illustrate syphilitic chancrous ulceration. Emery, of the "Hospital of St. Louis," who is also one of the Physicians to the "Dispensaire," and makes a weekly examination of the women of the town, makes a similar report. Ricord, who above all men would be the most apt to meet with cases of uterine chancre, mentions a very small number as having occurred in his private and public practice. Acton, who was for a term of years a pupil, and the intimate companion of Ricord, and in constant attendance upon the wards of the "Hôpital du Mide," states, in his excellent work on "Venereal Disease," which, by the way, is merely an epitome of Ricord's clinical lectures, that uterine chancres are scarcely ever met with in the wards of the Hospital. Balbirnie, on the contrary, asserts, that that during his connection with the "Hôpital du Mide," he saw in twelve months' time, many beautiful cases of chancre of the "os tincæ." This statement is obviously incorrect, and Balbirnie must have wholly misinterpreted the pathology of the cases which he saw. Is it probable that he saw many cases of uterine chancre in the wards of the "Hôpital du Mide," and that these cases should have escaped the observation of one

so experienced and skilled in such matters as Ricord? Lisfranc, in his Lectures on Diseases of the Uterus, makes no mention of chancre of the uterus; the inference therefore is, that he never saw a case of it. This fact, however, is not so remarkable; for, although Lisfranc had as much, if not more experience, than any man of his day, in this branch of practice, the speculum was not in vogue then as it is at the present time. The speculum being the only means by which a correct diagnosis can be formed of organic uterine affections, and may we not include most of the functional disorders, for many of them have their origin in some organic lesion of the viscus, it is not strange that a man, even with Lisfranc's opportunities, who made little or no use of the speculum, should fail to see cases of chancre of the uterus. This very rare affection, whenever seen, must be viewed through the blades of the speculum. Bennet, Physician Accoucheur to the Western General Dispensary, in London, and formerly House Physician (by concours) to the Hospitals "Notre Dame de la Pitie," and "La Salpetriere," in Paris, mentions but three or four cases as having occurred in the whole of his career. Columbat, Ashwell and West have all had most ample experience in the investigation and treatment of uterine diseases, and they have added to the literature of the medical profession very able volumes on the subject, but neither of them reports a case of chancre on the uterus.

Why it is that uterine chancre is of so very rare occurrence? is a question of much interest, and calls for a good deal of conjecture and speculation. Can it be, as has been contended by some, that its peculiar characteristics are so modified by the locality as to change entirely the appearance of the ulcer, thereby deceiving the eye of an experienced practitioner, and causing him to mistake it for an affection altogether different in its nature and consequences. Gibert, whose opinion it must be allowed is entitled to great respect in matters of this kind has expressed himself to this effect. With deference to Gibert and other distinguished advocates of his views, we must be allowed to affirm our inability to see any ground on which such an opinion can be based.

Chancre retains in perfection all its characteristics, when situated on the prepuce, glans penis, in the urethra, vulva, anus and mouth. The celebrated Telemacho Mitaxa, of Rome, mentions a case of chancre at the internal canthus of the eye, and also one in the meatus auditorius, both of which retained throughout the entire progress of the affection all of the distinctive features of chancre in its most ordinary location. That it is robbed of its specific attributes, simply by being situated on the uterus, is a point which we think may be safely controverted; and it devolves upon Gibert and others who advocate it, to show cause for their belief in the doctrine. Nothing has yet been discovered in the structure and arrangement of the component parts of the uterus, that warrants a belief in its power to modify a specific form of ulceration, to such an extent and in such a manner as to change entirely its appearance and nature. All the ordinary forms of inflammation and ulceration of the uterus obey with great regularity the laws governing these affections when situated on other parts of the body. We see no reason for believing that syphilitic chancrous ulceration forms an exception to the rule. Bennet, of London, in his valuable work on the uterus, offers, we think, the most plausible explanation of the extreme rarity of Uterine Chancre. The virus, he contends, is rarely deposited on the uterus, from the fact of its being brushed off by the walls of the vagina before the uterus is reached. This view, he adds, is corroborated by the rarity of chancres in the superior portion of the vagina, which must proceed from the same cause. But may there not be still another reason why the uterus is so seldom contaminated with syphilitic virus. We know that there is a very low state of vitality and sensibility in the "cervix uteri," and that absorption is less active there than at many other points of the body. Establishing this fact, is it not fair to presume that, should syphilitic virus have been deposited on any portion of the uterus, so much time being necessary for it to take effect, it may by some means or other be removed before the organ has become contaminated. The practice of using the vaginal syringe, so common among females, would prove a very

effectual barrier against the uterus being the seat of chancrous ulceration. And this practice of syringing is more common with loose women, in whom we would expect to find chancres, than with those of more virtuous habits.

The case described above is interesting, from the fact of the extreme rarity of such accidents, and instructive inasmuch as it calls to the mind of the practitioner the great importance of an examination with the speculum, when there is the least reason for suspecting the existence of an organic uterine affection. It too often happens that because a case presents no rational signs of serious disorder, the patient, after a simple verbal examination, is put off with an astringent wash and a stimulating application to the sacrum, when the trouble is one requiring a local application of powerful caustics—perhaps the actual cautery, or, it may be, the ligature or knife. The patient whose case we have just described, was, to all appearances, but slightly diseased; no rational sign indicated a serious termination of her trouble. She only complained of an uneasy sensation through the hips and loins, and stated that she had "Prolapsus Uteri," and an annoying vaginal discharge. Believing, as we have for a long time, and as was intimated in another part of this paper, that most of the functional disorders of the uterus have their origin in some organic lesion of the viscus, we resorted at the very outset to the use of the speculum, and found the uterus, as above represented, contaminated by chancrous syphilitic ulceration—a genuine "Hunterian chancre," occupying the anterior lip. Had this case been turned away without having undergone a thorough investigation, and an energetic course of treatment, the disease would, in a short time, have invaded the whole system, and ended the patient's days, or have caused her to drag out a loathsome and wretched existence.

Most of the disorders to which the uterus and its appendages are liable, are of such a nature as to require a local treatment. In order to exercise discretion as to the kind of application to be made, it is all important to ascertain the nature of the disease requiring treatment. To do this properly there must be ocular inspection of the parts. What physician

would think of prescribing for a sore throat without first making an examination of the fauces, in order to know whether it was a simple inflammatory sore throat, an ulceration of the mucous surface—a Cynanche or a Diptheritis. Here is an additional reason for a more frequent resort to the speculum. Granting that by any rational signs, uterine affections can be diagnosticated with accuracy, in order to treat them properly, the speculum must be used; for no local application of energy can be made to the uterus without it. It is with great difficulty that many physicians can bring themselves to the use of the speculum, from the fact of its being unpleasant to a modest female. True, it is disagreeable to a patient of this class, and it is equally so to a practitioner, if he is a person of proper sensibility; but at the same time, that he has a due regard for the feelings of his patient, he should not be unmindful of the deep responsibility of his office. The interests of his profession should be considered paramount to the squeamishness of false delicacy. Let us remember that our calling is a noble one—none more so—that the duties incident to it are at the same time arduous and perplexing, and that it is only by bearing constantly in mind the responsibility and high trust imposed upon us, that we can be nerved to their faithful discharge.

Yellow Fever treated as an Exanthem where the eruption has been repelled. By T. L. Ogier, M.D., Charleston, S. C.

It has frequently been observed that some cases of Yellow Fever are attended with an eruption. But as this has been noticed occasionally only, I do not know that the disease has yet been considered an Exanthem, or treated as one. Dr. La Roche who has published the most thorough and scientific work on Yellow Fever, says, "it is not uncommon to see cases attended with a full crop of miliary pimples," and "Dr. Barton in his report on the sanitary condition of New Orleans

in 1853, says, that during the epidemic of that year, *a fine miliary eruption* was usually seen on the skin within twenty-four hours of the attack. It was the harbinger of safety *as long as it kept out;* its repulsion, the signal of great danger, if not fatality." This certainly is precisely what takes place in Exanthematous diseases. It is true we know of none in which the eruption does not make the most prominent feature of the disease, or at least one of the most prominent, which is not the case in Yellow Fever.

During the past season, the eruption was very plain in many cases. It covered the sides of the neck, chest and shoulders, particularly of children and women. In every case where this was observed, the gastric symptoms and vomiting were much relieved, and the patient got well without those aggravated symptoms which often portend a fatal termination. This eruption was generally observed about the end of the first or the beginning of the second day. From these facts I determined early in the season, to treat the disease, not like an ordinary Exanthem, but like one in which the eruption had suddenly disappeared, or, where, from congestion of the internal organs or some other cause, the eruption could not show itself upon the skin. With this view, I pursued the following treatment.

Considering it important that the bowels should be evacuated of all irritating matter, I ordered five or six grains of calomel, to be followed, four hours after, by a dose of castor oil; soon after the calomel had been taken, the feet and legs to be put into a hot mustard bath, and kept there for ten or fifteen minutes; this bath to be repeated every six hours, during the first twenty-four, and continued longer, if the head is much affected. The head is kept cool with moist cloths, and *croton oil linament* rubbed on the neck, chest, epigastrium and spine. This is done as soon as the bowels have been evacuated, and is continued every six hours until an eruption appears on those parts. One rubbing will often be sufficient on children and persons of delicate skins. Others require two or three applications before the object is attained. The patient is kept perfectly quiet,

and allowed to make no bodily or mental exertion, even so far as to have his attention called to any particular subject. He is allowed to see, if possible, no one but his nurses and physician. He is permitted to sip iced water often, but a desert spoonful only at a time. No nourishment is given until the fever ceases, and then strained gruel only with boiled milk, from time to time. To determine to the skin and also to stimulate the kidneys slightly, and moderate violent arterial action, I prescribe the following mixture:

℞	Potass Cit.,	ʒii.
	Tinct. aconiti rad.,	ʒss.
	Sacch alb,	qs.
	Aquæ,	℥vj.—m.

A table spoonful to be taken, every two hours for eight or ten hours, and then every four hours, until the fever goes off. To children half the dose is given, or even less, in the same manner.

If the stomach is very irritable, I do not continue the mixture, but apply a blister to the epigastrium, notwithstanding that it may be covered with the croton oil eruption. If the bowels require opening, resort is had to enemata of strained gruel with a little salt. I never give a purgative, or disturb the stomach with any thing irritating, after the dose given in the beginning of the disease. In some cases where nausea and vomiting have continued until the patient is much exhausted, I give very minute doses of morphine in camphor water, every one or two hours until relief is obtained. The following is the proportion in which it is administered,

℞	Sulph. Morphiæ,	gr ½
	M. Aquæ Camph.	℥ii.

Dose, a teaspoonful of the mixture.

The success of this treatment has been in every way, most satisfactory. In eighty-six cases consisting of strangers,

(adults,) - - - - - - - - - -	32
Native children, - - - - - - - - -	26
Blacks and colored strangers, - - - - - -	28
	86

there have been but *two* deaths. I have never seen so many recover under any other treatment.

It may not be proper to consider the disease as one of the exanthemata, but I think there is no doubt that irritating the skin, and producing artificially an eruption where the natural eruption is often found, will always moderate many of the most distressing symptoms, and in many cases entirely relieve them. I believe that if we assist nature only by such simple means, the disease will subside more readily than when treated with active medicines. If it be an exanthem, it is clear that active medicines should not be given. What would be thought of violent purgation or salivation in small pox, or measles, or scarlet fever? Some cases would recover, but the majority would die. So it is with Yellow Fever; some get well under the most active treatment, but many must die.

An interesting question relating to Yellow Fever is that referring to acclimation. It is often asked, whether one attack of Yellow Fever acclimates a stranger, and if after one attack he may expose himself as freely as a native? In examining the subject, we may trace some resemblance between this fever and some of the eruptive diseases. An incident occured during the summer which may illustrate my meaning. Three young gentlemen, not natives of this city, but permanent residents, were here through the season. The eldest was taken ill with the Yellow Fever, about the middle of August. He was nursed by his two brothers and recovered. A week after, the second brother was seized with the disease; became very ill, and was nursed by the youngest. The second brother recovered, but he was hardly up, before the third was taken sick with the fever, just as the other two had been. But the fever of the third, instead of continuing three or four days as in the two previous cases, was ephemeral only. In twenty-four hours his fever was gone, and on the third day he was well. Now this youngest, or third brother, had been sick with Yellow Fever in 1854; the two others were absent that year. The two absentees had the disease severally, and were very ill, the

other was sick twenty-four hours only. The fever seemed not to be able to take hold of his system. Instead of Yellow Fever, the complaint became ephemeral only. He had Yellow Fever before, and was inoculated, as we may say, in 1854—instead of *small pox*, he had *varioloid* only, and that mildly, notwithstanding his extraordinary exposure.

The Fever proved more fatal than usual to children, during the summer just past. But as far as I can learn, those who had the disease in 1854, for the most part escaped last year, or had it very lightly.

I am aware that there are exceptions to the facts above stated, and that persons have died of a second attack of Yellow Fever. These cases, however, are rare, and are exceptions only to the rule ; this I believe to be, that one attack from yellow fever exempts the patient from the disease, and in this way persons who have once had it, may properly be said to be acclimated.

Art. IV.—*Observations on the Properties of Arnica Montana.* By A. N. Talley, M.D., of Columbia, S. C.

In the eager pursuit after new discoveries in science we too often overlook or unduly appreciate those truths, for our knowledge of which we are indebted to the patient and sometimes painful researches of our predecessors. And while this remark applies alike to all the departments of medical knowledge, it is, perhaps, most forcibly exemplified in the history of the Materia Medica.

The general demand for specifics has oftentimes caused to be undervalued or neglected those remedies which do not arrogate to themselves so high a rank.

These remarks are intended as apologetic for calling the attention of the profession to an old and well-nigh obsolete remedy, which I apprehend is familiar only in name to the generality of modern practitioners; I allude to the Arnica Montana, or Leopards-bane.

For the botanical characters, habitudes, and chemical composition of this plant, I refer the reader to the various treatises on Materia Medica, as it is my purpose, at present, to dwell only on its therapeutic application.

Its active principles are said to reside in all parts of the plant, and my experiments with the flowers and root satisfy me of the correctness of this conclusion, though I am apt to think that they exist in a higher state of concentration in the former than in the latter.

Its properties, as represented by different writers, are acro-narcotic, tetanic, diaphoretic, diuretic, and stimulant. Its effects upon the animal economy, when taken in poisonous doses, we are told, are those of an irritant narcotic. The roots and flowers, and especially the latter, when chewed, impart to the mouth and fauces an acrid, burning, and disagreeable sensation. When taken into the stomach in large doses, it is said to occasion griping and colicky pains in the bowels, epigastric uneasiness, and sometimes nausea and vomiting, though from my own observations, I am persuaded that these effects are not at all to be apprehended if administered with ordinary caution. And even when exhibited in poisonous doses, I should expect to find the head symptoms predominating over the gastric and intestinal disturbance.

When given in appreciable quantity, its effects upon the system are manifested chiefly in an exaltation of nervous power, differing, however, from nux vomica, in this, that while the energy of the latter is spent upon the motional nerves, the influence of the former is exerted for the most part upon the nerves of sensation. Though reckoned by writers among the tetanics or excito-motory stimulants, I have witnessed none of those spasmodic twitchings of the muscles from its use, which usually accompany the action of irritants of that class. But that such effects may be produced from its exhibition in excessive doses, I am not prepared to deny.

That it is capable of augmenting and modifying the susceptibility to external impressions of the sensational nerves, is evinced by its action in asthenopia, in which it is said to have been successfully employed, and in that form of tinnitus

aurium which depends upon an impaired or perverted sensibility of the auditory nerve. Professor Wilde, of Dublin, says, that "to relieve tinnitus aurium after the inflammatory action has been subdued, or the original disease which produced it has subsided, and particularly when we find this symptom present without any apparent lesion of the parts we are able to inspect, I have found the preparations of the arnica montana of decided benefit; indeed, it is the only medicine with which I am acquainted, that seems to possess a specific power over this annoying and usually most intractable complaint."

With regard to the diuretic properties of arnica, I am not prepared to express an opinion, never having prescribed it with a view to such effects, though from the copious diuresis which I have occasionally witnessed in patients under its influence, I can readily believe the testimony of others in its favor in this respect.

Of its diaphoretic effects, I speak with greater confidence, for frequent observation has convinced me that it is capable, under favorable circumstances, of augmenting in a great degree the secretion of the cutaneous exhalants; but whether it acts directly upon the secretory tissue, or secondarily, by inducing an engorgement of the vascular capillaries, others may determine. Of this, however, I am satisfied, that this property does not depend on that relaxation of the cutaneous vessels, which attends the action of nauseants.

But it is as a stimulant in nervous disorders, and adynamic diseases, rather than to any influence it may exert in either of the foregoing respects, that the arnica merits an especial consideration. It cannot of course be regarded an exception to the rule, that where there is manifest cerebral or spinal irritation, stimulants are inadmissible; though even here its range of application should not be too narrowly circumscribed; for that impairment of function which so often persists after inflammatory action has subsided, is no bar to its employment; such, indeed, being the cases in which its happiest effects are said to have been realized. In neuralgia, hysteria, hypochondriasis, chorea, and in short in most of the

abnormal conditions of the nervous system, its virtues have been tried and highly commended.

In the latter stages of typhoid pneumonia, I have given it in combination with carb. ammonia and decoct. of senega, with decided advantage. In those fugitive neuralgic pains, which are a frequent and distressing accompaniment of this form of disease, but which the stethoscope has shown are not necessarily, nor even generally, located in the seat of inflammation, I have found it of much benefit.

On some parts of the continent of Europe, the arnica enjoys, we are informed, the reputation of being a medicine of much efficacy in continued fever, and my experiments with it in typhoid fever convince me that its virtues in this respect, so far from being exaggerated, are scarcely appreciated. The first case of this disease in which I employed it, was one which, having resisted the influence of mercurials, quinine and turpentine, and the more usual remedies, threatened a fatal termination. In placing my reliance upon this article, I was influenced not only by its admitted action upon the brain and nervous system, but also by observation of its effects when locally applied to abrasions of the surface, bruises, superficial burns, and indolent ulcers; inferring that what had proven beneficial to these cases, afforded a reasonable prospect of advantage in that diseased and ulcerated condition of the entire glands which is thought to constitute this affection. The patient above alluded to had for some days been taking a mixture of quinine and turpentine in the proportion of two grs. of the former to ten drops of the latter, and a like quantity of paregoric, in a table-spoonful of mucilage of gum arabic, every two hours. This course had been pursued, not only without visible amendment, but with a palpable declension of strength, and aggravation of symptoms. Omitting the turpentine, I added to every dose of the mixture ten drops of the tincture of arnica, and directed milk-punch, and beef tea, *ad libitum*, as before. Forty-eight hours under this treatment sufficed to have wrought a decided change in the general aspect and condition of the patient. The pulse had lost much of its frequency, and regained some-

what of strength; the tongue, from being red, dry, and fissured on its edges, and black and furred in the middle, adhering to the roof of the mouth so as to render difficult its protrusion, now assumed an appearance of moisture; the belly, before hard and tympanitic, became softer and less tender; floccitation ceased, and stupor gave place to the returning light of reason. The medicine was continued, giving as before milk and brandy with beef tea, until convalescence was established. As the case progressod towards recovery, the interval between the doses was increased successively to four and six hours, and the remedy discontinued only when the tongue had assumed its natural appearance, and the bowels regained their healthy tone.

The same course of treatment, with immaterial variations, I have since frequently pursued, and sometimes in cases not more promising than the foregoing one, and always with a like success.

It may be objected, that by the combination which I employed, to wit., quinine, beef tea and brandy, the agency of the arnica in bringing about the result is rendered doubtful, inasmuch as the remedies in conjunction with which it was administered, are by some relied on exclusively in the treatment of typhoid fever. The answer to this objection is contained in the fact before stated, that until the arnica was given, and notwithstanding the use of these means, the tendency of the disease was to death, that after its exhibition the improvement was decided and rapid. Nor does the necessity for such adjuvants in any wise invalidate the claims of the arnica, for an observation of its effects demonstrates that its influence is spent not upon the circulation, but upon the nervous system, that it cannot be considered an arterial stimulant, (though it may, and probably does affect secondarily the circulation and respiration) but rather as invigorator and sustainer of nervous power. And when it is remembered, that the disease under consideration presents symptoms, as well of nervous (and primarily in this respect) as muscular debility, the propriety, and even necessity of such a combination becomes apparent.

In this connexion I may be permitted to add, without entering into the pathology of typhoid fever, that antecedently to those enteric manifestations which have engrossed the attention of writers and are regarded as pathognomonic, the disease consists originally in a defect or suspension of those nervous influences which in health pervade the system, and preserve an uninterrupted harmony between the various organs of the body and their respective functions.

It is to the advanced stages only of the fever that the arnica is applicable; to that period when the characteristic atoxic functions of the disease are developed, and when we have reason to believe that the intestinal glands are in a state of ulceration. When listlessness and hebetude have usurped the place of reason, and when the vital forces are held in abeyance by the torpifying influence of this stealthy and subtle malady, whatever else we may employ, our chief reliance must be placed in the alcoholic stimulants, and preferably of these, brandy. In such cases my practice is to give fifteen or twenty drops of arnica in a table-spoonful of brandy and repeat it as the urgency of the case may require.

In advocating the claims of the arnica, I would not be understood as attempting to supersede the use of turpentine in this disease. In their mode of action and effects upon the economy, these remedies are, I suspect, very closely allied, and may doubtless sometimes be advantageously employed the one for the other.

It does not consist with the purpose I have in view, to enter more at length into the treatment of typhoid fever, my object being briefly to lay before the profession the conclusions deduced from actual observation, in relation to the remedial virtues of a plant which has been permitted to fall almost exclusively into the hands of charlatans.

In typhoid dysentery, I have given it with marked benefit, my own experience tending to confirm the opinions of others. Some cautions are necessary, however, in using the remedy in this disease, for while the inflammatory action is yet undiminished, the stools frequent, bloody, and distressingly painful, the abdomen acutely tender, the pulse full and quick;

anti-phlogistics, with mercurials and anodynes are our main sources of dependence, and cannot safely be exchanged for any other treatment. But when typhoid symptoms begin to manifest themselves, which we recognize in the failing pulse, tympanitic abdomen, a skin alternately hot and dry, then cool and clammy, dejections consisting of depraved mucous and purulent matter, streaked, it may be, with blood, the tongue presenting a red, dry, and varnished appearance as if deprived of its papillæ, the arnica, under these circumstances, may be given with a fair prospect of advantage.

The mode in which I have used it in such cases, is a drachm of the tincture to six ounces of mucilage, of which a table-spoonful is given every three hours, alternating with a powder composed of five grs. of tormentil and one of opium. If there is irritability of stomach, a pill of four or five grs. of acetate of lead with the opium is substituted for the powder. Of course the usual anodyne and astringent injections are employed to keep the disease in check, and relieve the irritation of the lower bowel.

In paralysis, I cannot say that I have derived any decided benefit from it, though the condition of the nerves and nervous centres are so very different, and even opposite in this affection, that we may readily understand its efficacy in some forms of paralysis, and its entire inertness in others. From its acknowledged mode of action, however, I should expect the most favorable results from those cases which depend upon a want of tone in the sources of nervous influence, or to a diminished power in their channels of communication.

In the treatment of chronic rheumatism the Germans have ascribed to the Leopard's-bane supernatural powers, and for its virtues in this affection, have classed it among their sacred remedies. With its external use, conjoined with rubefacients and anodynes, I have reason to be satisfied, though internally I prefer to rely on those agents the efficacy of which is more generally admitted. The formula which I have prescribed for its external application, when a mild and soothing one was required, is as follows :

℞. Tinct. Arnicæ,
Tinct. Sapo. Comp. aa. ℥ i.
M. Chloroform, ʒ ii.

When local irritation is needed, I use the following, with decided and unmistakeable advantage:

℞. Tinct. Arnicæ,
Aq. Ammon. fort., aa. ℥ i.
M. Tinct. Camph., ʒ ii.

To which chloroform may be added, if desired.

In chronic nervous affections, I have given 20 drops of the tincture, and, by gradually increasing the dose, watching its effects, even 60 drops three times daily in a tea-spoonful of comp. tinct. cardamom. Its success in these cases has been variable, at times acting with promptness and energy, affording speedy and permanent relief, while at others no benefit whatever has been observed, though the dose has been pushed to the limit above indicated.

Dr. Parker, the physician to our State Lunatic Asylum, informs me that in cases of the kind just alluded to, he has been very favorably impressed with its effects.

The tincture which I employ is made by macerating two ounces of the leaves in a pint of dilute alcohol for fourteen days. In its preparation the process of displacement may be used to equal or greater advantage, as by the latter method the necessity for a screw press, to force out the last portion of the liquid is dispensed with.

Dr. Tweedie, in his System of Practical Medicine, has given a formula for preparing an infusion from the flowers in which he directs a drachm to the pint of water, of which a table-spoonful is the dose. To the use of this he annexes a caution, but to what end, or wherefore, he leaves a matter of conjecture. For my own part I am disposed to think that its poisonous qualities have been greatly exaggerated, having myself taken, with a view to ascertain its effects upon the system in health, 20 drops of the tincture as above prepared three times daily for two successive days, without other inconvenience than a sense of fulness about the head, with slight gastric uneasiness.

Professor Wilde recommends a tincture made of an ounce and a half of the flowers to a pint of dilute alcohol. He also prepares a tincture of the leaves in the same proportions, his custom being to prescribe them in equal parts. Of this combination he directs fifteen drops three times a day in some aromatic tincture, and increases the dose one or two drops daily until vertigo or giddiness is induced, when the dose is diminished or the medicine pretermitted.

In making a tincture of the root, Dr. Neligan directs two ounces to the pint, of which he gives from ʒ ss. to ʒ ij.

Of the leaves I know nothing but what we have on the testimony of writers.

Art. V.—*The Treatment of Dysentery.* By W. S. Forwood, M.D., of Darlington, Md.

Dr. J. P. Evans, of Tennessee, has done good service to the profession by his articles on the pathology and treatment of this painful and dangerous disease, in the July, September and November numbers (1858) of this Journal, *(Charleston Med. Jour. and Rev.)* It might be said that he has travelled over the whole field,—indeed his articles are almost too lengthy (altogether about 84 pages) for a journal, and to be thoroughly read by practical men, whose time is occupied in discharging the duties of the profession, and who require the most condensed views on such practical subjects as their incessant labors generally exclude them from the perusal of those articles that treat diseases *in extenso.* But Dr. E's. certainly possesses great value. I so entirely coincide with Dr. Evans in the leading principles of his treatment of Dysentery that I would not offer one word on the subject, were it not to claim some credit for this treatment for three worthy men, who are not now here to speak for themselves. Considering the many authorities, in books and journals, old and new, that Dr. E. has examined on this disease, it is somewhat surprising

that he should have overlooked an article, by Dr. Thos. Worthington, of this county, (Harford,) which appeared in the *American Medical Recorder*, in July 1828, (vol. XIV, p 199,) in which nearly the identical treatment advocated by Dr. E. is laid down; with the exception that Dr. W. considered a mild purgative occasionally necessary. The following is a synopsis of Dr. Worthington's article:

Prior to 1825, he says, *vitrum antimonii ceratum* was successfully used by the "late Dr. Richard Sappington, of this county," and he also speaks of his own "unfailing success" with this remedy until the above named year, when the disease "appeared to assume a new aspect, and in the hands of the successors of Dr. Sappington, as well as my own, it could not be arrested in its progress by this, or any other mode of treatment generally practiced. There was an early tendency to sinking of the system, this year, which rendered the use of antimony, ipecac, and calomel, peculiarly inadmissible." He speaks of patients dying under the operation of an antimonial emetic, and of purgative doses of calomel. Dr. W.'s "friend and former preceptor, Dr. Robert Archer, of this county," informed him that, for some time past, he had been using *camphor* with opium as an anodyne, by the recommendation of the late Dr. Miller, of Cecil county, Md., and with the happiest effects." "From that time," continues Dr. W., "I have given it, I believe, in every case of the disease I have met with, and have never yet, that I remember, had cause, in a single instance, to regret having used it. A pill of gr. i. of camph. and gr. ss. of opium is administered every six hours, and the bowels preserved in a soluble state by the daily use of pretty free doses of Sulph. Magnes., vel Sodæ." At this point the following note is appended to Dr. W.'s article, by Dr. Archer: "Every six, eight or twelve hours, according as the griping, tenesmus, or frequency of the stools, is more or less severe; taking special care, that every twenty-four hours, two or three discharges from the bowels be procured by Sulph. Magnesia, whose appearance, as respects color, is natural. Without this precaution, the camphorated anodyne will not only fail in its effects, but in very many instances be injurious."

Dr. W. proceeds to say, "one other observation which I have made, it may be worth while to notice, viz: when the disease is followed by diarrhœa, as it very frequently is, the camph. anodyne is by far the most efficacious remedy I have ever tried. Astringent remedies (as Kino, &c.) I have experienced to be very apt to promote a renewal of dysenteric discharges. I may further observe that some trials were made with opium alone, with a view to compare its effects with the opium and camph. combined; the difference was most strikingly observable." He compared the one to "locking the thief in the house," &c., and the other to "supplanting him by skilful manœuvering," &c. I am warranted in stating, in connection with the above, that Dr. W. afterwards found it necessary to give the anodyne pill much more frequently than once in six hours.

These opinions of Drs. Miller, Archer, and Worthington, on the anodyne treatment of Dysentery, were published by the latter, as we have already seen, over thirty years ago. At that time, however, the calomel and emetic treatment was so much in vogue that Dr. W.'s article did not receive the attention it deserved; and, as he never after published anything on the subject, (to my knowledge,) the valuable remedies that he then recommended were soon forgotten, or neglected, by those who had read his views. Dr. W. continued the use of this pill (Pulv. Opii. gr ss., Gum Camph. gr. i.) with unabated success, throughout the remainder of his valuable life, which lasted, to the happiness of his many friends, until about three years ago. His pill was celebrated even beyond the confines of his extended practice, and universally relied upon by all his patients. Had he kept its composition a secret, as some practitioners have very improperly done with other medicines, it would have been greatly to his pecuniary advantage. It is to be regretted that he did not in after life, with his manifold experiences, publish some further views on this subject.

I may add that I have used the same pill, myself, in dysentery, and with the same good results. I find, however, that one pill "every six, eight or twelve hours," as recommended by Dr. Archer, will not answer, at the present day, in my

hands. I administer them much more frequently. When the tormina and tenesmus are violent, and the patient strong and but little susceptible to the narcotic influence of opium, I generally begin by giving two pills, and afterwards one every two hours, night and day, until decided relief is obtained. Even to delicate females I have given the medicine thus often for two or three days in succession, keeping them, for this length of time, in a positive state of narcosis; having frequently found it necessary to produce and maintain this state before the prostrating dysenteric irritation could be controlled and subdued. In ordinary cases one pill every three hours is sufficient. As convalesence takes place, one pill every four or six hours will meet the indications; and I find that the last named dose is frequently called for several days after the patient has so far recovered as to be out of the house, since spells of griping and purging are liable to recur sometime after the general symptoms have disappeared. I have not practiced this treatment to the exclusion of purgatives, as is advised by Dr. Evans. I am in the habit of exhibiting from one to two teaspoonfuls of Magnes. Sulph. every day, or every other day, according to indications. I cannot rid my mind, although it may be a mistaken idea, that the inflammation and excessive irritability of the colon prevents, by its spasmodic action, the discharge of the feces from the ileum, without some stimulation being directed to the latter; and also when the contents of the small bowel are retained for several days, they must surely undergo the process of solidification; if so, when they pass into the colon in this state will they not produce a violent effect upon its denuded surface? By administering moderate doses of Magnes. Sulph. (which is mild in its action) an accumulation is prevented, a soluble condition maintained, and perhaps congestion and fever lessened. I am constrained to believe, though I am unable to explain why that Magnes. Sulph. possesses some peculiar curative virtues in the treatment of Dysentery, independent of its purgative properties. Indeed some practitioners, particularly in the more Southern States, have relied *entirely* upon it; giving it in teaspoonful doses every two or three hours; and

numbers of their patients have actually recovered speedily *without any other medicine.*

Nearly all serious cases of the disase are accompanied with more or less fever; which, after a few days, generally assumes an intermittent form. When the least intermission is observable I depend upon Quinine Sulph., as is advised by Dr. E. and have but little difficulty in checking the fever. I give several small doses, say to the amount of gr. vi. or gr. viii. altogether, during the three or four hours immediately preceding the expected chill, or the return of the fever, if, as is sometimes the case, the chill should be imperceptible. I administer the Camph. and Opium pill at all stages, and under all circumstances, without regard to complications, and uniformly with the happiest results; for during the last four years, through which I have depended upon this remedy, I have not lost a single patient with the disease under consideration.

In ordinary Diarrhœa I know of no medicine, or combination of medicines, that acts so promptly and efficiently in cutting short the disease as Dr. Worthington's Pill (as it is popularly called in this vicinity.) If there is not much pain in the latter affection, I precede the remedy by a full dose of Pil. Hyd., or Hyd. Chlor. M.; if pain is a prominent symptom I advise the anodyne to be taken first, and followed, when the irritation is overcome, by one of the above mentioned preparations, with a view to their action on the chylopoiëtic viscera, upon the derangements of which Diarrhœa frequently depends. From three to five pills, given one or two at a dose, every two or three hours, will in the great majority of cases, where administered early, entirely subdue this disease.—In cases of Diarrhœa or in young children, I depend altogether, and with the most gratifying success, upon the Tinct. Opii. Camph. (U. S. P.) in doses of gtt. vi. to gtt. xv., according to age, &c., three or four times a day, and from gr. iij. to gr. viij. Hyd. cum Creta once or twice a day. I consider these two medicines almost specific in these infantile complaints.

In conclusion, I can recommend to the profession, with the

utmost confidence, in common with Dr. Evans, the Camph. and Opium treatment, as referred to in this paper, and as extendedly detailed by Dr. E., in all the *present* forms of Dysentery and Diarrhœa. The fact that Dr. Worthington adhered to this pill through the last thirty years of his life, without finding a better or so good a remedy, of itself speaks volumes in its favor; particularly when it is known how successfully he combatted this much dreaded disease. I do not hesitate, when an alterative is called for, by such symptoms as jaundice, &c., which sometimes supervene in the course of this malady, to give *small* doses of Pil. Hyd., (preferably,) or Hyd. Chlor. Mit., in connection with the anodyne pill. This comprises (with the exceptional use of Sodæ. Bicarb., and Potass. Nit., either separately or combined, which I occasionally give as a *placebo*, as much as with any other view,) my entire treatment in all ordinary forms of Dysentery.

ART. V.—*Pathology and Treatment of Yellow Fever. Epidemics of* 1854 *and* 1858 *in Charleston, S. C. Observations respecting absence of Urea contradicted. Urea not always entirely absent in the Excretion from the Kidneys.* By F. PEYRE PORCHER, M.D., Physician to Marine Hospital, and Lecturer on Materia Medica and Therapeutics, Charleston, S. C.

I deem it my duty to give the most prominent position in this communication (made principally for the purpose) to a correction of an observation published by myself in the January No., 1858, of the Charleston Medical Journal and Review. This was, that urea, together with uric acid, as far as my examinations up to that date permitted me to pronounce, was absent in the urinary secretions of Yellow Fever, save during the period of convalescence.

Having had charge of the Marine Hospital this summer, I have examined the specimens much earlier and with more special direction to this point, and also with more extended experience, and I *do* find nitrate of urea, by evaporating the fluid in a watch-glass over a spirit-lamp, in some of the cases

of fever. Crystals of uric acid, ready formed, which I had pointed out as a characteristic feature among the negative signs, I have never yet found in the urine in Yellow Fever, and I regard this as indicative of its diminished amount.

Being much more occupied with the treatment of the disease this season than during '54 and '56, and having been unavoidably absent a portion of the summer, I have of course had fewer opportunities for investigating with the microscope the excretions of the disease. But I have examined several specimens, searching for other substances which might be peculiar to it, viz., hippuric acid, cystine, etc., without success. The excretion of ammonia by the skin was sometimes evident by the white fumes formed on bringing hydro-chloric acid to a patient's body. If Frerichs is correct, that the phenomena of uræmic intoxication as in ecclampsia, for example, are not produced by urea, or by any or all of the excreted matters of the kidney, but by the transformation of urea accumulated in the blood into carbonate of ammonia, then the citric acid and the diuretics formed of vegetable acids recommended further on in the history of the treatment, have a scientific reason for their exhibition, as they neutralize the carbonate of ammonia. The difficulty of coagulation of the blood in Yellow Fever may be connected with ammonia in excess in that fluid. The almost constant presence of bile, the frequent existence of albumen in the worse cases, the absence of uric acid crystals, deposited spontaneously, though uric acid can be manufactured out of almost any specimen of urine by the addition of hydrochloric acid, characterize this secretion, as far my present experience permits me to speak.

With reference to the assumed absence of urea (as shown by the addition of nitric acid and the application of heat) I shall be excused for a few additional remarks. I believe the line of reasoning respecting the pathology and treatment of the disease suggested by the difficulty, if not impossibility I met with in obtaining urea, as well as by my previous experience in its treatment, as maintained in that paper, to be by no means invalidated. The reasoning and opinions regarding the pathology and treatment therein set forth, have been most satisfactorily and fully sustained—as I may have occa-

sion to prove by the detail of cases which my experience this Summer, in and out of the Hospital, as far as it goes, abundantly furnishes.

Urea, as an ordinary constituent of urine, is never entirely absent from any specimen of this fluid, though the relative amount, the only important point to the pathologist, varies materially in health and disease, and also in different diseases, and I believe, in which I am sustained by others, that in certain stages of *Yellow Fever*, and most assuredly in certain cases accompanied with congestion of the kidneys and arrest of the urinary excretion, urea is thrown back upon the blood, reaching the brain, and causing the symptoms of coma, or uremic poisoning, so often met with in this disease, as well as described by authors as occurring injuriously in other morbid states. A certain proportion of our cases of Yellow Fever die in this way, for I have noticed at least three distinct modes of death, though examinations are yet wanting to *prove* the coëxistence or not of the absence of urea in the fluids excreted from the kidneys in the particular variety above referred to. I have also, during the epidemic of the last summer, clearly traced the death of certain patients with Yellow Fever to albuminuria—the presence of an enormous quantity of albumen being detected in fatal cases, characterized by no other symptom sufficient to produce a fatal result, the patient apparently convalescing, and the urine in these cases only giving the slightest indication of the presence of immense quantity of albumen upon boiling, and the addition of nitric acid.

From observations made by myself in a number of cases of intermittent and remittent fever, treated by sulphate of cinchona (see article Charleston Med. Journal, March, 1858), I certainly am of the opinion that the proportionate amounts of urea discharged after the use of quinine and sulphate of cinchona, in these two diseases, is very much greater than in certain varieties, if not in every case, of Yellow Fever. In other words, that deficient action of the kidneys (owing to congestion, and to its relations with the skin, and with the diseased capillaries, principally of the skin and cellular tissue)

is accompanied by want of normal discharge of this principle; and that one important indication in the treatment is to prevent or arrest such a tendency.

The presence or absence of certain principles is plainly recognized by recent writers, and will demand examination at the hands of the pathologist. I will cite one of the latest authorities: "Any disease, however," says Thudichum, in his treatise on "the Pathology of the Urine," just published, (London, 1858) "which interferes with the secretory power of the kidneys, by changing its structure, such as Bright's Disease, is certain to cause retention of uric acid, in the blood, in proportion to the retention of the other constituents of the urine. Scarlatina seems to make an exception. * * * In inflammatory diseases, fever, and certain other zymotic diseases, an increased amount of uric acid is met with." (P. 85.) A remark not borne out by my examinations of the amount spontaneously crystallizing in the urine in Yellow Fever. He refers to accumulations of this substance in the system, "an excess of uric acid may be due to excessive production in the body, particularly when the increased excretion lasts for a certain time. It may, however, be owing to the discharge of accumulated uric acid, *after retention in the blood.*" I moreover maintained, in both papers, and I believe for the first time, the *intimate relation between Yellow Fever and certain diseases of the skin*—accounting for the tendency to congestion of the kidney in each, by the complementary action of the two organs, and by the high degree of capillary combustion going on in the former.

And now to palliate any apparent haste in the announcement of my observation respecting urea, a word or two may be said, to show that I earned some title to commit an error, if the large amount of time I devoted to the subject could make one. After some forty or fifty examinations of the urine in Yellow Fever, during the summer of '56, (the specimens generally obtained from patients in the Roper Hospital being sent to my office) the epidemic over, I carefully searched for the results as detailed in my Case-book, which contained each set of observations, with rough drawings as they

were made at the time. I then published the apparent conclusions furnished by these observations, including that respecting urea. Besides, it was the Editor, in his table of contents, on the cover of the Journal, who made the "absence of urea" the main feature in the article—never regarded so by myself, but only one step in advance, with regard to its pathology and treatment, as laid down at length in the article itself. Let any one mark the difference between the caption of the article as written by me, and that contained on the cover of the Journal, for which I am not responsible.

I may add, that my publications on the subject of urinary pathology have been sanctioned by no slight devotion to the subject, with whatever inadequate abilities,—certainly by the previous expenditure of a large amount of my leisure. After devoting several months, daily, to the study and practice of the microscope, under the direction of M. Robin, in Paris, in 1853–'4, I have examined and recorded upwards of three hundred cases of every description of disease, including tumors, morbid growths, fluids, etc., making, in every instance drawings, however rough, of the view presented upon the field of the instrument, and subjecting the matters examined to boiling, re-agents, tests, etc. Still I feel duly contrite, and humble at the propagation of an error, even if it was not absolute, but one only of degree.

I repeat, that though urea (as evidenced by use of nitric acid) is *not absent* from the urine in all cases of Yellow Fever at every stage, when the specimen is examined early, yet the reasoning respecting the failure of the kidneys to excrete it freely from the blood in certain stages of the disease, the general treatment enunciated in my article, as well as the particular application of this idea, (together with the assumed absence of uric acid) to the treatment, with respect to its bearing upon the use of alkaline diuretics and revulsives, as sustained by my previous experience, is by no means invalidated; and I have every reason now, after the experience of this summer, to congratulate myself upon the application of these ideas to practice, as well as fully to endorse the general plan of management of these cases, then for the first time

described. I hope further and more ample opportunity for seeing the disease will not tend to abate this confidence respecting the amenability of Yellow Fever to treatment, when it occurs in any healthy subject, seen early.

But I am bound at present, after the epidemics witnessed by me, with my comparatively limited opportunities for observation, to assert the proposition that the cases of the "prevailing fever," be they yellow fever, imitative, parasitic, breakbone, or catarrhal, in patients that came under my care, were as amenable to treatment, under the condition expressed above, as any other disease affecting a similar number of subjects, if not more so.

I am compelled to make this assertion, for, of thirty cases of fever, of various grades of intensity, treated in '54, the condition of all of which were noted twice daily, and the record preserved, the smallest fraction died. Of the three fatal cases, two were seen after forty-eight hours.

This summer I had *no* death out of thirty-two cases treated out of the Hospital, all of which were seen within ten hours, though it must be stated that I have but two cases recorded among Irish or Germans. There was no death in that institution, where the patient was seen within ten or fifteen hours, up to November 1st. Of course those, for the most part, who came into the Hospital after 24 or 36 hours, died, as any one at all conversant with the rapid maturation of dangerous symptoms in a disease like Yellow Fever, would readily predict. For one in this stage to survive, is demanding more than we have a right to expect, or any person of ordinary foresight would require. A *sine qua non* is that the patient should be of ordinary health, not addicted to drink, and that the treatment be commenced early. If a physician lost 99 out of 100 of those who applied for relief in the latter stages of the disease, it would not imply that he was at fault with respect to his method of treatment, by which almost all recovered who sought early assistance. And now in this view of the fatality of the disease when treated early, in patients not habituated to the use of ardent spirits, I am not

singular in this city—there being several of our physicians who have secured an unusual proportion of recoveries to deaths.

I will again, as an important omission was made in my article in this Journal, rapidly detail the general system of management, which must not, however, for a moment be confounded with anything like mere routine.

I would beg those of our professional brethren who confess that they regard the disease at any stage, and in any subject, as often fatal, to submit it to a careful test. Those who hold that we are quite in the dark, and that not a particle of progress has been made in forty years, as regards either the rationale of, or the success in the treatment, will most assuredly welcome and embrace anything that promises what they do not aspire to, however they may desire it.

I write this with a becoming diffidence, fearing whilst it is uttered, that a future day may prove it like the baseless fabric of a vision—but the facts so far warrant no such conclusion.

Treatment.—I find powerful revulsion to the stomach and spine with mustard plasters, next the skin, at the very *commencement* of the desease, persisted in every *two or three hours* and *for several days;* with the employment of sponging or ice to the head if there is much fever, headache and heat of surface, of the first necessity. It is of prime and absolute importance, and often turns the scale in favor of the patient. I fear no injury from any amount of counter irritation with the agent; (though any other irritant might do as well); it prevents or arrests nausea, diverts powerfully to the surface and from the internal organs and brain (conjested during the usual chill ushering in the fever,) lessens the headache, and may also have a special influence in abating the capillary stagnation, seen by its effects more readily during the latter stages. I have seen a blister placed upon the surface of the abdomen, to which mustard had been applied for two days, and the great irritation caused by both disappeared before the patient had fully convalesced; I fear no injury from them.

The use of a large dose of Calomel and Quinine grs. xx. of each to an adult white person at the very commencement of the disease, followed, if the patient is seen early, in a few, say three hours, by a dose of salts or citrate of magnesia, (I prefer saline to other purgatives,) completes all active treatment, so far as laxatives are concerned. Free purgation is desirable in the first stage, *injurious* if not useless, possibly fatal after it, by inducing irritability of stomach, and intestines. It should not be forgotten that both calomel and quinine, in *large doses*, are sedative, and reduce the action of the heart; as used above, so salivation follows; they provoke, besides, excretion from the glands. The subsequent administration, after the salts has acted, of the effervescing draught of the U. S. Dispensatory, made with bicarb. potash and fresh lemon juice, or the acetate or citrate of potash, (with morphia gr. $\frac{1}{8}$, if there is any nausea,) every three hours have proved, in my hands, completely efficient, provided the patient is seen within six or even ten or fifteen hours from the beginning of his attack.

The preparations last named are diaphoretic and cooling in their influence; acting on the blood and capillaries, proving diuretic, and preventing congestion of the kidneys, which leads on, if unchecked, to arrest of the normal discharge of urine and urea, to hemorrhage, etc.

As soon as the fever abates, or disappears, whether during the first or fourth day, quinine alternating with the alkaline solution is again administered in from one to four grains every four hours; hoping by this means to prevent a return of the fever, laying aside its recognized value as a sedative and tonic, besides its value in all disease, of malarial origin, presenting intermittence or remittence. It does no injury certainly, in my experience, and one motive for again republishing this general plan of treatment is the omission of it in my article alluded to from having failed to look over my notes of cases treated in '54 and '56. Many cases, too, occurring in this city, if not often confounded with diseases of malarious origin, have the intermittent element plainly marked, and thus quinine often proves a useful adjuvant. Indeed some of

our Physicians believe yellow fever in the stranger to be only bilious remittent in the native or the partially acclimated. No more efficient agent than mustard sinapisms, *constantly* applied, to keep up powerful revulsion, exists to prevent a return of the fever, and, also, to arrest those internal congestions and alterations of the mucous membranes, liver, portal system, etc. The feet being also placed in hot mustard water, under the covering of the bed, and then rubbed dry, diaphoresis is induced—besides the diversion of the blood from the brain caused thereby.

Before each dose of the purgative medicines are administered the mustard *plasters* are ordered to be applied, and it may seem remarkable that in 32 cases of *fever* of every grade of intensity treated outside the hospital, by means of this agent, I have not had more than one to vomit a dose of medicine or, save in one instance, to vomit at all. I do not pronounce all these *yellow fever*, giving immunity from second attacks, for I find it impossible to distinguish, with precision, the grades of fever prevailing where there are those of every shade of intensity, from the simplest to the most severe. I defy any one to decide positively into which category to throw some of those occupying the undeniable middle ground—too severe to be called simple fever, and yet hardly enough so to be classed as yellow fever, and giving immunity from future attacks—and yet the physician has seen apparently milder fevers, at their onset, followed by black vomit. During the period which succeeds the subsidence of the fever, when the pulse often, even in the most favorable cases, descends to 60, 50 and even 48 and 44 to the minute, as I have noted in three cases which recovered, brandy and nourishment may be given cautiously. But I have noticed, repeatedly, that though useful, stimulants have no power to raise the pulse in this stage, however much employed. I have observed the pulse becoming elevated only about eight or ten pulsations through the twenty-four hours, and this continuing several days after convalesence, before it attained the normal standard. I have made the observation repeatedly in those whose pulse had descended to 44, 48 and 52, having made it my du-

ty to examine these patients some days after recovery, timing the radial artery, in every instance, with the watch, at least twice a day, and I have a record of this for almost every case treated during '54 and '58.

At any time during the disease resort is had to the local application of mustard, also to the foot baths, should there be a rise in the pulse symptomatic of a return of fever, for it generally indicates some stagnation in the internal organs, brain, skin, etc., quite natural in the weakened and diseased condition in which the patient is after a disease like this; if unchecked, leading on to disorganization of mucous membrane and of the blood, to jaundice, to black vomit, to hemorrhages, coma, etc. I use a blister to the abdomen at any time after the first twenty-four hours, if nausea or vomiting exist. Enemata may be given, if there is constipation, during convalesence; but never resort to a repetition of the purgatives, to the calomel or to any drastic or depleting agent, and this is *de rigueur*, for they irritate the stomach and intestinal canal, incite to further irritability of stomach, and I humbly believe often tend to *hasten* a fatal issue.

It may be true that repeated small doses of calomel, the mercurial treatment, have not proved injurious in some cases of fever, the management of which is commenced during the first stadium; I have no experience with it, but am inclined to regard the proscription of *all* active, certainly of all drastic purgatives, after the bowels have been well *moved* and the intestinal canal and its appendages well emptied, at the beginning of the disease, of the first importance—the patient being allowed to trust, afterwards, to the power of his constitution, (as in the allied skin diseases,) with the aid of the cooling diaphoretics and diuretics, and the revulsives mentioned. In the persistent use of the latter, in the avoidance of repeated mercurials and cathartics, the subsequent diuretics and quinine, I think lies the secret of the management of yellow fever—these are the principles to guide us.

I have used in the hospital a single other prescription with advantage, viz: a stimulant of turpentine, ammonia and camphor in mucilage of gum arabic when there was much dry-

ness of tongue, and a typhoid condition was threatened.—Possibly bleeding and emetics at the very beginning of the disease, and leeching or cupping the epigastrium or head at any stage in which there was a presumed congestion of the organs, might prove serviceable. Dry cupping has been advised in albuminaria, and I have used it in cases where there was albumen in excess in the urine. Creosote or muriated tinct. iron may be useful in arresting hemorrhage and black vomit. In the latter I am inclined to trust more to nourishment, brandy, carefully given, and minute doses of morphine. If the patients are seen early I hope that these dangerous symptoms will not supervene, for it is during the fever of access that the internal organs and the blood, are most fatally altered, and this being mitigated or arrested, the chances of such dangerous results as black vomit, and hemorrhage, and coma, are correspondingly lessened or abolished. In the jaundice, sometimes following an attack of yellow fever, resort may be had to rhubarb and minute doses of calomel. In a post mortem, made in a patient dying in this stage, I have found the gall duct full of bile, the liver, mucous membranes, fluids of the body, etc., tinged with the excessive secretion of this substance. Hence the indication for its removal by agents eliminatory of it, cautiously employed.

I hope my experience of the fever prevailing in '54 and '56, and up to the date of this epidemic, (Nov. 1st,) will continue to be sustained, that all cases, or certainly a *very large percentage*, seen within the first five or ten hours (though the earlier the better) are amenable to treatment and judicious nursing, and may, and do recover. Drunkards, and those with digestive organs so impaired as not to respond to medicines, are of course excepted, though I have not yet had occasion to attribute any death to this cause, as absolutely none died who was seen early.

It will be an important admission, if made, for whilst some of our brethren claim, with myself, a very satisfactory proportion of recoveries, a number regard the disease, in their hands, even under the most favorable circumstance, as often perfectly incurable. Such was the opinion entertained when

I returned here the first week in September. At that period it was the medical profession which entertained the gloomiest views concerning the inutility of any system of management in certain cases and at any stage of the disease. Important, because a main feature of the plan of management indicated above, is the avoidance, for the most part, of all weakening and irritating agents, drastric cathartics, salivation, &c.; and also, because despatch in securing the services of a physician, good nursing and attendance will almost certainly insure recovery.

Just as in cholera, the administration of medicines during the period of cholerine, tends to increase the probability or possibility of cure a thousand fold, or as in ordinary cases of scarlet fever, recovery is almost certain if the physician persits in avoiding the use of *active medication*, and trusts materially to mild alkaline diuretics, warm baths, etc., and allows the patient to survive the inevitable course of the disease.

The analogy between yellow and scarlet fever, and measels, is, in my opinion, much closer than many medical men believe ; but as I referred to the distinction between them, as well as to the points in the pathology of the two diseases, in the two articles already alluded to, and published a year since, I will not do so now. Such comparisons are fruitful in useful results.

I am, I confess, myself, quite hopeful with respect to the prospect of a continued improvement in our knowledge of the true pathology and treatment of yellow fever—in the possibility of the treatment being systematized upon general principles ; just as the treatment of pneumonia and scarlet fever, fevers of periodicity, (tertiary) syphilis, etc., have been systematized and improved within the last ten or fifteen years, thanks to investigations in pathological anatomy, therapuetics and the materia medica. I welcome investigators, close thinkers, and original reasoners—I welcome those who will study and ascertain the mode of application of veratrum viride, croton oil, or any other powerful, but untried agent, and I hope much from the growing spirit of inquiry and original research which animates a few of the younger and more hardy, but still conservative, spirits in our profession.

A word as to the fevers prevailing along with those of severer grade ; they may be entitled imitative, ephemeral, parasitic or break bone, but I have not seen one this season which was catarrhal, there was no sniffling, no fullness about the head, running at the nose or eyes. None of these characterize them. The head and intellect is clear, the eyes free from tears, but with conjunctivæ injected. There is no covyza, though the fever may be high in both diseases. Their intensity seems often to depend entirely upon the amount of climatization enjoyed by the subject. I conclude by saying that I lay special stress upon the avoidance, in this disease, after the very first stage, of active medication, purgations, etc.; the use of powerful revulsives, *began with as early as possible* and *persisted in for several days;* the employment of blisters, if required, with alkaline diuretics and quinine, the treatment generally as indicated above ; and I hope and humbly believe that my opinions, as published and announced to my friends, have been fully sustained by the results, and that they will continue to be found true and well founded. Certainly, at present, mild diuretics in the second stage have been adopted by a large proportion of our practitioners.

The method of managing cases of yellow fever, as detailed in this Journal for the past ten years (I have taken the trouble to examine) is either radically opposed to the above, or is wanting in some of the most essential features. There are no views as to the pathology of the disease, and consequently of any rational system with respect to its management. There is no reference by two writers, who have written largely, in our midst, of the use of revulsives from the begining, in the vigorous and energetic way above recommended ; nor of the diuretics or quinine after the first or hot stage of the fever (stage of chill and excitement) have passed off; or what is, and perhaps too, of more importance of the avoidance of active means after the lapse of a certain period. Ten to twenty grains of calomel with repeated doses of salts and snake root, or followed by purgatives, and then by broken doses of calomel, or the same quantity divided into four powders and given every three hours, and this persisted in, was the plan adopted.

If the patient was seen the third day of the disease they were nevertheless used in the same way. Some of our writers, too, express themselves astonished that death should ensue after the apparent quiet of the second stage—because the pulse being at 75 and there being no bad feeling complained of (as is often the characteristic feature in fatal cases,) it was not seen in what way and why death could be imminent—and still calomel and purgatives were continued! A patient whose treatment is began on or after the second or third day of this disease, or who has been allowed to remain too long with the hot burning fever upon him, (all the while altering or destroying the blood,) or whose intestinal canal is continually irritated by mercurials and purgatives, is already destroyed, and no power on earth, yet applied, can save him. His tissues are compromised, and neither blisters or stimulants will avail aught, even though a mental and physical quiet reigns throughout his system and pervades his actions. I really believe, from my own experience of its effects, that a good and efficient purgative (probably the mercurial with quinine possesses peculiar advantages,) used at the very inception of an attack of yellow fever, if it does not abort the disease, will do very much to render it mild and easy of recovery.

And I find, only recently, (Jan., 1859,) upon consulting a "Treatise on the plague and yellow fever, by James Sitter, compiler of the medical part of the Encyclopedia Britannica, 1799," the following remarks: "It was formerly a custom to use bleeding and purging when people arrived in warm localities; but this practice fell into disuse, perhaps without sufficient reason." Dr. Rush attests the efficacy of these remedies as *preventives when signs of the disease appeared.* "During the existence of the premonitory symptoms, (says he) and before patients were confined to their rooms, a gentle purge, or the loss of a few ounces of blood, in many hundred instances, prevented the formation of the fever. I did not meet with a single exception to this remark." The writer goes on to speak of the adaptedness of mercury for the purpose. The same writer says, further on, "Dr. Rush is, indeed, very much of opinion that it is easily practicable for people

to cure themselves of this disorder, dreadful as it is, provided they take it in time. But by this we must understand, that the very moment the person feels uneasiness he must apply a remedy, and not trust to *nature* in any case whatever. When the yellow fever prevails, every person who feels the slightest disorder, may be assured that the disorder partakes of its nature, and ought immediately to have recourse to a mercurial purge."

Again, *purgatives* are found extremely useful, both as preventives and medicines. There are innumerable instances where an incipient attack of the disease has been carried off by a brisk purge. Dr. Chisolm was able to remove the slighter cases of Boullam fever by purgatives. I have been glad to find that corroborative testimony, and I think that after the purgative, the diuretics and revulsives, tend further to benefit, as eliminatory agents, and divert from internal organs to the surface. I would certainly not object to either bleeding or tartar emetic also, at the commencement of the attack.

REVIEWS.

ART. VII.—*Watson's Practice of Physic.* A new American, from the last (the fourth) Revised and Enlarged English Edition. With Additions by D. F. CONDIE, M.D., &c., &c. Philadelphia: Blanchard & Lea, 1858, 8vo, pp. 1224.

It is entirely too late to offer any ex cathedra opinion as to the general merits of Dr. Watson's world-renowned Lectures on the Principles and Practice of Physic. Among the most entertaining of all technical writings, they are full of instruction. The varied learning of the distinguished lecturer, his extensive reading, as multifarious as that of the merely literary man, his obvious knowledge of the world, all find occasion for display in the cheerful, easy flow of thought and language which mark these prelections, and set them absolutely apart from every thing similar that has ever been presented to the public. Long since out of print in England, they enjoyed the widest circulation in America. Dr. Watson has at last gracefully complied with the demand for a new edition in his own country, and our enterprising publishers, Lea & Blanchard, of Philadelphia, have, without delay, issued the large and well got-up volume, which we are about to notice, under the editorship of Dr. Condie, himself a well-known and popular author.

We propose, in this brief review of the work, to speak chiefly of a few points of recent and immediate interest, in which the present edition differs from its predecessors. Dr. Watson is not one of those men for whom the world moves in vain. He notes and follows its progress, and accommodates his views to all efficient and inevitable change. He avows, for example, an entire alteration of opinion in regard to the Continued Fevers of Great Britain; "the positive distinctions

that have been insisted on by eminent pathologists between typhus and typhoid fevers, are recognized as being founded in truth." He enters into the discussion of the new points started by Prof. John Hughes Bennett, of the University of Edinburgh, in reference to Inflammation—its nature, history and treatment. On all the several topics considered successively in detail, we shall find him posted up in most of the later discoveries and suggestions, coming from whatever quarter—with one exception—that are worthy of attention. With one exception, we say. We are sorry to observe so little regard to, or apparent knowledge of American writings on Medicine, and of American authors, physiologists, pathologists, and therapeutists, as is shown from the beginning to the end of this thick volume of more than 1200 pages. It is exceedingly rare that any allusion, however distant, will be met with to our country or our countrymen, except within the brackets of the Philadelphia editor. In the respect and admiration felt for Dr. Watson, in these distant regions, "all the reciprocity," to use the words of an Irish orator, "seems to be on one side." The existence of a transatlantic community is acknowledged, inferentially and incidentally, some three or four times;—by the mention of the *celebrated* Gen. Washington; Dr. Francis, of New York; Beaumont and his St. Martin; and of Dr. Bartlett's excellent work on Fevers, in a brief note of four lines. There is no more notice taken of us than this.

Nay, while, as we have said, very generally posted up on matters of scientific importance and practical interest, as far as European authorities are concerned, he is greatly "behind the times," where the progress, in whatever direction, has been made by Americans. The omissions seem almost of necessity wilful, or designed and contemptuous. Thus, for example: In 1840, Dr. Horace Green, of New York, published a work "on Diseases of the Air Passages," in which he describes a practice, originated by himself, of introducing a sponge, saturated with a solution of nitrate of silver, into the larynx, through the glottis, and adds plates of the instruments employed, and a detail of the method of using them. The

work, and the practice recommended in it, was received with loud encomium and vehement denunciation on both sides of the Atlantic, and the whole subject extensively discussed in reviews and journals of greatest weight and highest reputation. Trousseau in France, and Ernsen in England, violently assail the statements of Dr. Green, who replies acutely, and makes many converts, and even enlists some warm partizans. In 1851 he visits Edinburgh, and in presence of Professor Bennett and his class, "passed the sponge, saturated with nitrate of silver, through the larynx into the trachea" of a patient laboring under "acute laryngitis." Dr. Bennett repeated the application several times, and the patient got well. In another patient, whose case was chronic laryngitis, Dr. Green introduced the sponge one day publicly; and Dr. Bennett continuing the same local treatment a few days, complete recovery took place. [V. Bennett's Clinical Lectures on Medicine, pp. 590, 591.]

Dr. Green, pressing forward his earnest efforts to relieve the hopeless subjects of phthisis, has twice dared to inject a solution of the same metallic salt into the lung, by an apparatus arranged for the purpose. This bold practice has been imitated many times, and, as is said, with good effect, and it still continues to be pursued.

It will hardly be believed, that in 1857, Dr. Watson deliberately reprints precisely what he had said in 1843. Compare p. 514 of the second American Edition, 1845, with the same publishers and editor, with p. 550 of the last "new" Edition, 1858, and you will find, word for word, the same obsolete twaddle about Mr. Vance, and his practice of "pressing against the superior surface of the larynx a sponge dipped in a solution of nitrate of silver, and secured with a string, or fastened to the finger of a glove," which, "having been a naval surgeon, he called in naval phrase, *swabbing* the affected organ." He repeats his having got Sir Charles Bell and Dr. Arnott to practice this swabbing; but after all, we find the exact phrase re-uttered, after an interval of fourteen years; while the world has been moving on, while physicians have been introducing sponges into the trachea through the larynx,

and injecting fluids into the lungs through the bronchi, after all this, Dr. Watson tells us in September, 1857,—as in September, 1843,—"Now, of this method of cure, I do not know much except by report." He speaks of swabbing, as he calls it; but he does not speak at all of the insertion of the sponge into the larynx;—shall we infer that he knew *nothing* of it, not even by report?

He does coldly admit, in speaking of pain, that "the glory and triumph of the discovery (of ætherization) was reserved for our brethren beyond the Atlantic." For this just acknowledgment, which had almost escaped us, we desire to record our thanks. With the single addition of the word "excellent," applied to Dr. Bartlett's work on Fevers, in a note, which work he does not seem to have read, or even seen, this is all we have to thank him for.

He has never heard of our peculiar or new method of treatment of any of the forms of disease common to both hemispheres; nor of any of the new diseases or varieties of disease supposed to be specialties with us; and of course says nothing of remittent or yellow fever, dengue, etc.

We cannot help feeling surprise, and trust that we shall be excused for expressing it, that this work, admirable as it is in itself, and desirable as a component part of every good medical library, should be made the text-book in any American Medical School, and especially in any Southern school of Medicine. Malarial remittents are not discussed; congestive fever is not alluded to, nor the word congestive applied in our familiar sense, in any single paragraph; but these, with yellow fever, and dengue, or "break-bone," as it is vulgarly called, constitute the most unfailing, prominent and important subjects of inquiry, attention and daily practice, throughout the whole South and South-West.

These defects are partially supplied, it is true, by the contributions of the Editor, Dr. Condie, who has also inserted essays besides on puerperal fever, cerebro-spinal meningitis, and cholera infantum, of undeniable value; but we are speaking of Dr. *Watson's* Lectures.

Some new and curious facts in reference to ætherization are presented at pp. 104 and 105, in the text, and in a note, which show strikingly how near we may come to a great and useful discovery, without making it. We do not agree with our author, that Sir Humphrey Davy, however "sagacious," offered any suggestion of the *anæsthetic influence* of nitrous oxyd gas. This "blessed invention," as he styles it, was most certainly reserved for our unhappy Wells, who clearly ascertained, and distinctly pointed out, this influence or property of the gas, and but for the ill-fortune which attended the early experiment in the Mass. Gen. Hospital, and which fatally disheartened him, would now have been universally recognized as having made the most valuable discovery of modern times: what do we say?—of any age indeed, and of all time! If the ancients erected more temples to Esculapius than to his father, Jupiter, because "he brought health to men," what lofty fanes, what high temples, what solemn worship would they not have dedicated to him who taught us to subdue and control Pain—the greatest of physical evils! Such worship, indeed, would scarcely have been idolatry.

Davy only knew his "laughing gas" as an exhilarant; we have already many agents of that class, and scarcely need any addition to their number. But the case which Dr. Watson relates as having occurred under the care of "the late Dr. Woolcombe, of Plymouth," is one of true and perfectly defined anæsthesia from ætherization, in which not only relief of suffering was fully obtained, but absolute and "heavenly unconsciousness." The *extent* to which the simple remedy was carried in the case of Lady Martin was the novelty—a result so gratifying, impressive and suggestive, that one cannot help wondering that nothing came of it. The use of the same volatile agent in asthmatic paroxysms is old practice, and every schoolboy soon learned to inhale it to exhilaration and intoxication. We are all, and have long been familiar enough with its palliative and less decided effects in the relief of various forms of dyspnœa. A medical gentleman of this city, who died many years ago of phthisis, used habitually by inhalation, æther, in which cicuta leaves were infused; and

the writer of this article has for a third of a century customarily prescribed similar frequent inhalations of undiluted and unmixed brandy, for the same purpose.

We cannot, we confess, make anything of his Italian soporific. Not being compounded of volatile substances, it does not appear how it could act, applied *ad nares.*

Next in order, as we look over the rich pages, full of anecdote, argument and illustrative observation, we come upon a note, which contains a reply to, and remarks upon the new doctrines taught by the celebrated Professor of the Institute of Medicine in the University of Edinburgh. The controversy which this gentleman's clinical teachings has excited, has been ably managed on all sides, and has brought into the field some of the most distinguished names of the day. A brief notice of it may not be misplaced or uninteresting here. Dr. Bennett repudiates the employment of the lancet altogether, as a remedy for inflammation, and urges this abstinence especially in Pneumonia. He maintains this position upon the general grounds of the irrelevancy of venesection; upon the principles of "a sound pathology;" the gradual disuse or decline of the resort to it in recent times; and the tabular results of statistics collated from various quarters. Both Dr. Watson and Dr. Alison, whom Dr. Bennett has selected as the opponents most entitled to a rejoinder, admit the fact that the lancet is much less used than formerly, and account for this by a supposed change in the character of inflammatory affections, or rather of the average condition of the human constitution at the present day; they do not agree with Dr. Bennett, as to the soundness of his pathology of inflammation, or the correctness of his definition of it; and they deny the validity of his statistics.

Dr. Bennett having, as it seems to us most needlessly, gone out of his way to depreciate the professional skill of Cullen and Gregory, Dr. Watson has a word to say, in passing, in defence of these truly illustrious Scotchmen. We fear that his feeble apology for them may be regarded rather as "damning them with faint praise." They deserve a loftier tribute of respect from us, their successors. The dwarf ele-

vated on the shoulders of the giant, sees farther than his supporter, and any schoolboy may now know as much or more of geography and astronomy than Christopher Colon or Isaac Newton. We are of those who believe that whatever may be the importance to the diagnostician, of the physical signs perhaps not unduly estimated at the present day, the pathognomonic symptoms, so closely studied and so well appreciated by the physicians of the last age, supplied them with all the knowledge necessary, as well for special as well as general prognosis, diagnosis, and therapeutic. We know that the profession is not infallible now, nor was it then; we are willing to wager that Cullen and Sydenham made as few mistakes as any practitioner now living. No man has a right to affirm their ignorance or liability to error with the offensive positiveness shown by Prof. Bennett. In the attempt to prove his 1st proposition—that "no reliance is to be placed on the experience of Cullen and Gregory," etc.—he falls into some strange assertions. He states that formerly inflammation was "recognized" by the presence of "groups of symptoms" which immediately again he tells us, "constituted inflammation." *Now*,—nous avons changé tout cela;—*now*, we know that "inflammation is a diseased condition of a part"—"a disease of nutrition." How do we know it? Do we not infer it from the phenomena present, just as was done of old; and if we understand better the relation of symptoms to the condition which they manifest, surely that does not alter the foundation, or even the essential character of our knowledge. We are positive that Cullen was no more likely to confound "a diseased condition" with the symptoms by which it is "recognized," than Prof. Bennett himself, in the full enjoyment of the light of the 19th century. Nay, obfuscated as the notions of the great Scotchman seem to our modern diagnostician, we are prepared to maintain that they were quite as clear as the new views of Prof. Bennett, and, in all probability much nearer the truth. On the very next page of his book, Dr. Bennett announces that he means by "the term inflammation, an *exudation* of the normal liquor sanguinis." It is not possible to accept the definition or dogma, even on Prof.

Bennett's own previously avowed principles. On p. 129, he states distinctly and truly, "that exudation results from a previous series of changes in the capillary vessels and the blood contained in them," going on to describe very fairly this series of changes. Now we can hardly conceive any thing more illogical than to set aside all the primary morbid movements, and fix exclusively upon their ultimate consequence or result, as the element, and the only element, by which the diseased condition shall be defined or characterized. These previous changes, *all together*, constitute inflammation, which often terminates, i. e. ceases, subsides or disappears, when exudation has taken place. No wonder that he should contend that v. s. is a remedy irrelevant here. Dr. Watson admits that no one would propose it or hope anything from it under the circumstances. It is as arresting, or at least retarding and diminishing the force of this series of preliminary changes, and thus *preventing exudation*, that v. s. is to be useful, if at all. And even when exudation has occurred, if inflammation be progressive, or tenacious, it is relevant,—whether expedient or not, is not now the question,—it is relevant, as a therapeutical means.

Dr. Watson, while intimating these opinions, does not, we think, press the argument with his usual force and ingenuity; but, perhaps, he was too clearly conscious of the weakness of his opponent's reasonings. It seems to us that the whole tissue of statement made by Dr. Bennett is both confused and erroneous. He contends that it is "not good practice to diminish the flow of blood to the part" inflamed; that the calor, tumor, rubor, are movements of the Vis Medicatrix. "The great work to be accomplished is an increased growth by cell-formation, to perform which an augmented flow and amount of nourishing fluid is necessary, as in the mamma when milk is first secreted," etc., etc. But what is the nature of the first central movement in a furuncle, or common boil—anthrax, or carbuncle—a scrofulous gland—a schirrous tumor? and of what is the inflammation curative? Of the exudation? Nay, but that "is" the inflammation. Of "the series of changes preceding it?" Nay, but they are the symptoms,

merely, which we moderns are too wise to regard as inflammation. Of "the diseased condition of the part?" Well—be it so! but what is that? if it be not exudation—not calor, nor rubor, nor dolor—we are not informed what it is.

But we must not take up too much space with this topic, Suffice it to say that Dr. Watson in few words expresses his entire dissent from the new views of Dr. Bennett. "I venture," he says, "to call in question the accuracy of Dr. Bennett's positions, that inflammations can never be cut short; but whether they are to end favorably or unfavorably, must and always will run through a certain course; that it is the physician's proper business to promote rather than impede this, their natural progress; that the formation of pus-corpuscles is essential to the elimination of the products of inflammation from the body. I believe, on the contrary, that inflammation may sometimes be extinguished in its very infancy, before any of its customary products have occurred; and that even after they have some of them occurred, the intensity of acute inflammation may be abated, and its extension stayed by the judicious use of the antiphlogistic regimen and remedies. I cannot think, as Dr. Bennett seems to think, that every step after the very first step in the inflammatory process, is to be regarded as nature's mode, and the only mode, of bringing that process to a satisfactory termination."

In another paragraph, he takes ground with Dr. Alison, as to the true value and uses of statistics. We have lived too long to trust them as establishing anything in Therapeutics. We do not know a single practitioner who is not prepared to establish the absolute superiority of his own method of treatment, whatever it may be, of any disease whatever, by a resort to tabular statements; and we know no one who yields to any amount of opposite figures.

Dr. Watson has "no room to discuss the question answered in the negative by Prof. Bennett, whether febrile inflammatory diseases may change their type;" but makes the distinct avowal: "For my own part, I am fairly persuaded by my own observation, and by the records of medicine, that there are waves of time, through which the sthenic and asthenic charac-

ters of disease prevail in succession; and that we are at present living amid one of its adynamic phases. (P. 174.)

Dr. Bennett's position in regard to this subject is altogether untenable. Long before the present controversy arose, it is admitted on all hands, and dwelt on by himself, that the lancet had come to be used, by common consent, less and less, even among men who still believe as we do, that it is not only a relevant and appropriate, but potent remedy for inflammation. Look back through the writings of physicians within the present century, and you will meet with the frequent remark that an endemic or epidemic, which on a previous invasion had required free bleeding, would not bear it on its return or renewal. We could quote many such. Taking up from our table, as just at hand, the treatise of Chambers "on Digestion," a work of high authority, we find on p. 316 the plain assertion of his belief that "a change has taken place in the human constitution, as well as in the minds and manners. The increased *intolerance of bleeding*, and other depletory measures, exhibited by the sick, however afflicted, is matter of daily observation." The degeneracy into a comparatively adynamic condition, which Dr. Bennett denies, is acknowledged as a fact by every physician of our own age and standing in these United States, whose opinion we have had opportunity of asking. The contrast presented between our own notes and reminiscences of forty years ago, with the therapeutical habits of to-day, is truly striking.

Whether there is, as suggested by Dr. Watson, a sort of rhythmical periodicity of revolution in the process of such change, whether there are cycles of return and renewal, is indeed a question as difficult as interesting. We participate in the fear intimated by Chambers, that the proclivity may be rather of truly progressive character, strengthening in our race as the world grows older and civilization developes more and more prominently its ultimate and characteristic tendencies.

Turning on, and "reading with our fingers," we catch an American name. It is that of "the celebrated Gen. Washington, who died of acute laryngitis, after free bleeding."

Near by we are told that "Dr. Francis, of New York, recovered after similar treatment, and still survives, enjoying a well-won reputation." In the first edition, it was "*a* Dr. Francis;"—"only this and nothing more." He seems, in the meanwhile, to have heard of the Doctor. But he seems not to have heard in that or any other connection of Gurdon, Buck, or Peaslee, or of Horace Green, as we remarked before, —or of Morton, or Pennock, or Gerhard, or Bigelow, or Gross, or Flint,—or anybody else who was so unlucky (for his fame) as to be born on this side of the Atlantic.

He still maintains (p. 633)—Dr. Hughes Bennett to the contrary, notwithstanding, and we are ready to back him in this first half of his therapeutic—that "venesection and tartar emetic are the proper remedies for the early stages of those flagrant forms of thoracic inflammation," pneumonia, for instance, "which our predecessors had to deal with," but, "that such flagrant and sthenic forms of pneumonia have become very rare among us." We have abandoned tartar emetic as thoroughly as Dr. Bennett has left off blood-letting—having become acquainted with many equally efficient and much safer substitutes. Among these, the veratrum veride—Norwood's excellent hobby—stands prominent. But Dr. Watson, of course, does not know anything of either Norwood, or his admirable "*bradycrote.*"

Cholera affords the occasion for some interesting remarks. He still adheres to the belief which he has "held from the beginning, that epidemic cholera is catching, that it results from a portable material poison," &c., &c., p. 921, That this poison must somehow multiply and increase is certain." But he seems to admit the probability of its multiplying out of the body as well as in it—a vexed question, as he truly says, and dwells with great fairness upon the views and observations of Snow and Petenkoffer.

We come next to Continued Fevers, when on p. 1089, "Dr. Bartlett's excellent book" is referred to, incidentally, in a note of four lines, which does the injustice of intimating a precedence in time as enjoyed by a European Physician, mentioned in the same note. Dr. Watson avows a total change

of opinion concerning the relations of some of the varieties of fever which Dr. Jenner—"the Dr. Jenner of our time"—has treated of "with patience and sagacity worthy of the great name he bears; and has traced out plain lines of division between them, and especially between two forms which had been chiefly confounded together, and which we *now* call, respectively, typhus and typhoid fever." It hardly need be stated that he had not seen Gerhard's paper of 1835, nor Bartlett's published in the same year, or that he has not noticed Nathan Smith, nor Hale, nor Jackson, nor Swett, nor Wood's anticipation of Dr. Budd, even in his wished for appropriation of a name—enteric, or as Dr. Budd has it, not quite so well we think, intestinal fever.

With his unfailing ingenuity and admirable facility of language, he sets forth the grounds of his new opinions; yet, after all, he leaves us in some doubt as to the actual extent of the change of mind he has undergone. Formerly he regarded all continued fevers—"typhus fever, brain fever, bilous, putrid, low, nervous, &c., as mere *varieties* of one type—running insensibly into each other, even the most dissimilar of them—and traceable often to the same contagion." Now he recognizes between those even most nearly allied, the most positive and characteristic differences. Yet he thus separates only three; typhus, typhoid, and relapsing fever. Of the last we know nothing, but we cannot help thinking that there still exists not a little confusion and uncertainty in the several histories given us of it by Jenner and others. Nothing can be more explicit than the doctrines taught by Dr. Watson as to the etiology of continued fevers; he contends pointedly, and with zeal, that they not only arise from contagion, but that they arise *exclusively* from contagion. Ochlesis, and all other suggested causes, he looks upon as merely predisposing influences; nurses, not parents of disease. To the questions "whether the same contagious quality belongs to all the three species of fever which the progress of medical science has compelled me to recognize—to the typhoid, the typhus, and the relapsing fever, and if so, whether these are all equally contagious; and again, whether they

all spring from the same poison, or whether each is caused by its proper virus only:" he answers, p. 1113—"I believe that they are all contagious; I believe that the contagious property is not equally intense in them all; and I believe that one species of fever cannot generate another, but that each is produced solely and exclusively by its like; that typhus always propagates typhus and never typhoid or relapsing fever, and so of the rest." This is all very clear, plain and simple, encumbered with no other difficulty than the *insurmountable one* of the origin or source of the first case, in each and every series. Creeds do not, however, incommode themselves with difficulties.

But we are weary of our task. If, when Aristides was living, the man who said he was annoyed at hearing him called the Just, had been able to alledge any definite instance of injustice as having been committed by him, he would not now be quoted, as himself unjust and absurd in his condemnation of his great countryman. Without making any objection to the uniform, universal and unqualified laudation of Dr. Watson's Lectures, in all our journals, as fascinating, delightful, interesting, instructive, erudite, and altogether complete, we have shown that though they possess great and varied merits, yet, that to an American they exhibit the striking defect, nay the positive and unpardonable fault, or vice of *injustice*—which involves the serious charge either of malice or ignorance. The former imputation we utterly disclaim. We do not imagine Dr. Watson capable of intentional injustice, but he is then pinioned on the other horn of the dilemma. To Jeffrey's famous question, who reads an American book? he would answer with a safe conscience, so far as appears in this large volume—NOT I. Of the manner in which the American Editor has done his office, we must speak in terms of sincere commendation—except that we have a similar accusation to bring forward against him also.

In the well written treatise on Yellow Fever, with which Dr. Condie has supplied one of the most remarkable deficiences of the English work which he has so ably re-edited, we meet with a studied avoidance of all reference to facts of

recent occurrence in the immediate vicinity of the city in which the book is published; facts of the utmost publicity, the highest interest, the most decisive weight and value. We do not suppose that he could, by any possibility, be ignorant of them; we are sure that he could not expect or intend, in passing them by, to suppress them; we are, therefore, totally at a loss to conjecture how it happens that he has made no allusion to them. The writings of Vaché and Harris are in every body's hands; and the daily newspapers, as well as the monthly and quarterly journals of medicine teem with stories, and cases, and arguments and replies concerning them.

Within one hundred miles of Philadelphia, lies the beautiful and once most healthy of all marine resorts and residences, Staten Island. At the very throat of the narrows, and at a moderate distance from the city of New York, it furnished a most convenient quarantine station. Summer after summer, this station first, and in gradual progression, the pretty villages, and neat farm houses along the shore in its immediate propinquity were invaded by the Hæmagastric pestilence. The same terrible plague also assailed the opposite coast of Long Island, separated by so straitened a channel that vessels anchored between them were not far from the land on either side. The common sense of the people living on these Islands, as well as the professional judgment of the physicians above named, whose opportunities for observation were unequalled and whose competency admitted of no question, all along, ascribed these annual invasions of pestilence to the presence of vessels from ports in the West Indies and elsewhere, infected with this fatal disease. Within a few months, indeed, public opinion on the subject embodied itself in large and excited meetings, and at last proceeded to action; the quarantine buildings were destroyed by fire, and the sick forcibly expelled.

We are not arguing the technical question of contagion, nor the other most serious question, social and political, of the right to abate violently, and under all contingencies, an alledged nuisance. But we say plainly, that the annual presence of yellow fever, on Staten Island, was a notorious fact,

involving palpable inferences, which imperatively demanded notice at the hands of Dr. Condie, who, in thus ignoring all the authorities and events appertaining to, and concerned in this portion of the medical history of the neighboring metropolis, has imitated, if not improved upon, the most prominent fault committed by his author, in the very popular work on which we have been commenting.

Dr. Condie maintains "the invariable origin of Yellow Fever from a specific poison existing in the air of the localities where it prevails, and produced from causes there existing—a local malarial cause; we mean also, 'he says,' to deny its propagation by contagion." New York is spoken of but once, and then among the towns or "places in which it (Yellow Fever) is an occasional, not annual or even frequent visitor." Staten Island, in which *it has for several years past been an annual visitor*, a spot not a half a day's journey from Dr. Condie's residence, is not even mentioned. But the people of that Island, with one voice, deny that there is, on its favored shore, any thing whatever that can be pointed out as giving local origin to such a postilence, under which they suffer so grievously, while the reeking streets and lanes of New York and Philadelphia, far less fortunately situated, and under all similar climatic conditions are exempt. Its granite hills, rising rapidly from the tide, with swelling undulations; its pure and refreshing sea breezes, and clear springs of limpid water; its surface naturally well drained, and covered with a short fine turf—no landscape on earth seems, to an eye accustomed to pestilential and insalubrious localities, less likely to give rise to the formidable and destructive malady with which it is afflicted.

Its condition must have been known to our editor, and in his Compendious Treatise on a topic of such warmly disputed interest, the omission to notice it, and to account for so palpable an anomaly—a state of things so directly in contradiction and contrast with the view he lays down so dogmatically, certainly requires explanation.

ART. VIII.—*Diseases of the Urinary Organs. A Compendium of their Diagnosis, Pathology and Treatment.* By WILLIAM WALLACE MORLAND, M.D., Fellow of the Massachusetts Medical Society; Member of the Boston Society for Medical Improvement; One of the Attending Surgeons at the Central Office of the Boston Dispensary; Corresponding Member of the Glasgow Medico-Chirurgical Society. With Illustrations. Philadelphia: Blanchard & Lea: 1858. 8vo. pp. 599.

The substance of the above volume, Dr. Morland informs his readers in the Preface, is composed of two essays, to which prizes were awarded by the Boylston Medical Committee, in the years 1855 and 1857. Furthermore, we are told that he has examined the entire subject of Urinary Pathology, and presented a digest of it, which, with certain alterations and additions, he believes, will "constitute a convenient hand-book" for those whose occupations will not permit them to study it practically and in detail.

From a perusal of the book, we can acquit the author of having sufficiently well satisfied the conditions of the first clause of his avowal. His researches appear to have been varied, laborious, and careful, but we must enter a protest against the unqualified acceptance of the latter proposition whose terms, we are of opinion, have not been fulfilled. Whether for purposes of reference or consecutive study, we have seldom encountered a book whose arrangement is so faulty and inconvenient.

We are aware that many departments of this special subject present inherent difficulties in their arrangement which are not easily overcome, nor do we feel prepared to offer a solution of them, but we speak now of a viciousness of method, voluntarily chosen, not reluctantly accepted, and for which the author is, therefore, fully and wholly responsible.

The work is divided into two parts, each of which comprises the bulk and preserves the form of the separate papers submitted to the Boylston Committee. The division was

fair, perhaps judicious under the circumstances; but when the essays are brought together, to make up a single volume, and that too intended for a "hand-book," it is imperative that the two be so combined and arranged as to present clearly and collectively under each head, all that appertains to it in diagnosis, pathology and therapeutic. On the contrary, they have merely been stitched together, part the first dealing solely with the diagnosis, and part the second with the etiology, pathology and treatment of urinary diseases. The reader, therefore, is obliged to turn from one to another portion of the work, throughout, in order to collect a definite idea of the author's opinions and suggestions in full, and make continual and annoying references to the index, in so doing. This trouble he has, we think, a right to complain of, as vexatious, *per se*, and incidentally so, as implying on the part of the writer a carelessness of the public ease, which that great autocrat is not likely to overlook or forgive.

So much for the manner; for the matter of the work, we have no hesitation in pronouncing it excellent, but when what is the property of others has been carefully sifted out, the author's proper residue will be found to constitute a very small portion of the whole. All of the modern writers upon this subject, which is at present engaging so much interest, and which has been recently illustrated by many distinguished names, have been largely drawn upon, and the author very honestly acknowledges, on nearly every page, his indebtedness to the works of Johnson, Christison, Bird, Prout, Rayer, Bright, Civiale, Rokitanski, Coulson, Gross, and a host of others.

Chapter I. treats merely of the anatomical relations of the urinary organs, and the occasional difficulty of diagnosis which arises from their anomalous position.

Chapter II. very properly disposes of the diagnosis of disease of the supra renal-capsules in less than two pages. All of the signs which are at present affirmed to be of value are extremely doubtful, both in relevancy and constancy. Indeed, if the researches and experiments of Dr. Harley are trustworthy—and they seem to have been careful and numerous—none of the conditions asserted to be consequent upon their

disease, injury or removal, are uniformly or necessarily presented when these organs are extirpated. Much obscurity, we are obliged to confess, still rests upon their relations to the organism at large, and the time of life at which their functional activity is most highly exhibited. The shortness of the chapter is therefore unnecessarily apologized for in a note, which informs us that it was written before the investigations of Mr. Addison had drawn especial attention to them, and that their pathology is presented at length further on.

In Chapter III., the enumeration of the diseases of the kidneys adopted by Dr. Johnson is followed:

NEPHRITIS.
  A. *Acute Desquamative Nephritis.*
  B. *Chronic Desquamative Nephritis.*
WAXY DEGENERATION OF THE KIDNEYS.
NON-DESQUAMATIVE DISEASE OF THE KIDNEY.
FATTY DEGENERATION OF THE KIDNEY.
SUPPURATIVE NEPHRITIS.
NEPHRITIS FROM RETENTION OF URINE.
PYELITIS.
NEPHRITIS FROM RENAL CALCULI.
TUBERCULAR OR SCROFULOUS DISEASE OF THE KIDNEY.
HÆMATURIA.

Of the fourth in the above table, Fatty Degeneration of the kidney, the author says:

"Different affections of the kidney have been described under the title "Bright's disease." At present there are *two forms* which mainly comprise it. *First*, a rapidly developed disorder, the morbid deposit in the kidneys being excessive; the tubes of the cortical portion being gorged with fat, the entire gland enlarged, and atrophy of its substance probably never occuring; the vessels of the organs compressed; in most cases, obliterated over a great portion; extreme impairment of secreting function; nearly always irreparable injury.

"Previous to the establishment of this condition, there is a stage of *congestive action*, but as Johnson asserts, there must be a certain amount of deposit in the kidneys previously to this; the congestion manifested throughout the disease may be either active or passive.

"*Secondly*, there is a form in which the kidneys are found, *post mortem*, 'granular and atrophied;' the deposit of fat is more slowly and less regularly made; some of the tubes, being filled, press like 'prominent granulations' upon the surrounding parts, and an 'atrophied' state results."

In making our diagnosis, the previous history of the patient should always command our attention. Tendencies to former renal disease must be taken into account. On examination of the urine, *albumen* is always (?) present, often excessively so. The specific gravity is lowered. The color of the urine is natural, free from sediment, contains neither epithelium nor tube-casts. As the disease progresses, oil globules appear, the fatty material becomes more and more abundant, and the degeneration is finally completed, beyond any hope of reparation.

"The 'mottled fat kidney' answers to the first of the 'forms' of the affection already referred to; the 'granular fat kidney' to the second. The last form is most commonly observed, *post mortem*. * * * *

"When once established, the disease induces hyperæmia and hypertrophy in variable degrees; the latter is gradual, but sure, in the first form; the explanation of the atrophied condition has been given. When the kidney is rendered heavy by the morbid changes mentioned, we may reasonably refer much of the sense of weight and uneasiness to this cause. Heat in the lumbar region is often annoying. The kidneys are frequently found hæmorrhagically spotted and softened; the Malpighian capillaries opaque and thickened; so complete is the disorganization, that it is wonderful how the patient could so long exist, and why the physical distress had not been greater, and the state of general *malaise* no sooner set up."

Chapters IV., V., and VI. are occupied with the semeiology and symptomatology of diseases affecting the ureters, the bladder, the urethra, and conclude the portion of the work which formed essay the first—devoted to whatever appertains to the diagnosis of these important maladies.

Part the second discusses, as we have above remarked, the pathology and treatment. "The term pathology," says Dr. Morland, in the opening chapter, "has a wide significance. It properly comprises whatever enlightens us upon the nature and phenomena of disease; its materials are to be gathered during life and after death; by familiarity with the constitutional peculiarities of patients when in ordinary health, as well as when ill. Their history, hereditary and personal; their habits; mental and bodily employments; their aspect and associations, are all pathological elements. To these, the physician adds his deductions from healthy and morbid anatomy; physiological reasoning; comparisons between perfectly performed and deranged function; micoscopical investigation, and the hints afforded by the action of remedies.

It is therefore evident that pathological knowledge is gradually acquired; our first treatment is often necessarily directed to the most urgent symptom, but all ulterior management properly rests upon a careful appreciation of morbid causes and effects. Not only is diagnosis thus established, but the question whether any, and what, treatment is required, can thus alone be answered.

"This portion of our subject may be summarily stated as follows:—

"*First.* What is the actual knowledge we possess as to the nature of the diseases of the urinary organs?

"*Secondly.* What is their best treatment?"

The first of these questions is considered in its relations to both general and special pathology, urinary disorders arising from deviations from the normal anatomy and arrangement of the organs; hereditary and accidental influences affecting their functions or tissues, departures from their normal physiological requirements; habits of life, exposure, climate, food, drink, etc., are discussed in detail, and the appropriate

treatment, whether regimenal, dietetic, mechanical, medicinal, or surgical, mapped out in extenso under the separate heads. The divisions and arrangements here correspond throughout with those designated in Part I.

An Appendix, made up of cases and opinions which have been reported in various European and American Journals, since the body of the text was prepared, concludes the work, which we can recommend sincerely, as offering a reliable and full resume of all the recent advances in urinary pathology, but which would have been far worthier of favor if more attention had been devoted to the clearness of its style and method.

BIBLIOGRAPHICAL NOTICES.

ART. IX.—*A Treatise on Fractures.* By J. F. MALGAIGNE.—Translated from the French, with notes and additions. By JOHN H. PACKARD, M.D., Philadelphia. J. B. Lippincott & Co.: 1859. 8vo. pp. 683.

We have so long been in the habit of consulting Malgaigne's work on Fractures, and are so familiar with his text, that it appears to us almost superfluous to call the attention of the Medical public to a Treatise which has been so long before it. French is now so universally read, having become a constituent of general education; that it is no longer a barrier to the scientific inquirer. Those, however, who are not so familiar with this tongue, will hail, no doubt, with much gratification, the appearance of this American translation; for, of all the works meriting this distinction, there is none upon which it has been more worthily bestowed. As a literary,

historical, or practical Treatise upon the subject which it discusses, it has no equal in this or any other language. Throughout its pages we find every where the marks of great erudition, laborious research, and a fixed determination to be practical, which constitute, indeed, the characteristics of the writer. No where can we turn for more reliable statistics, and in no treatise will we find collected so much useful information. No stone is left unturned to search out the truth wherever it could be found, and works, from the time of the earliest to the most modern writers have been culled with judicious care.

More than one-third of the work is taken up with minute inquiries into Fractures in general, with their etiology, varieties, semeiology, march, terminations, diagnosis, prognosis and treatment, and, to us, it forms, by far, the most valuable portion of the book, for the subject is most faithfully delineated. It is from this portion that most recent articles on this subject are compiled, for we continually detect, in works on Surgery, as they are offered to the public, very liberal extracts, usually acknowledged.

It was in studying the etiology of Fractures in the original volume, that we were struck with the relative predisposing influences of age, sex, seasons, peculiarities of bones, &c., and how such diseases as scrofula, scurvy, syphilis, gout and the like, which are supposed by many to impair the cohesion of bone, are rather coincident with the very common, we might say natural, deterioration in the bony system resulting from age, and classified as the rachitis of senility. Among the predisposing causes, Malgaigne mentions that local circumscribed imflammation of bone has been too much overlooked. That pain is often found existing, perhaps for weeks, previous to the application of force, the bone yielding at the point thus previously defined.

In regard to symptoms, it was by Malgaigne that our attention was drawn to the great confidence which should be placed in *mobility* as, perhaps, the most important and least equivocal sign of fractures. Also, as regards pain, although used as only a presumptive sign, when a point is found where

the uneasiness upon pressure is suddenly and markedly increased, he has ventured to diagnosticate a fracture, and fix its seat by this sign alone.

One subject which he treats in greater detail than any other writer on fractures, is the stiffness of the limb, following such injuries, which often continues for months and years after perfect consolidation has been effected, being one of the most pertinacious sequelæ. He treats of it as an anchylosis, which is sometimes produced from adhesive inflammation, having as a cause, compression of the structures of the joint, from a continued extended position of the limb. A more common stiffening of the neighboring joints is found in the actual shortening of the ligaments and surrounding tendons, so that after a long interval of rest, when motions are imparted to a limb, some of these ligaments are stretched, and some may be actually torn, producing sprains of various grades, with their inflammatory accompaniments; and all of these injurious results are caused by keeping the limb immovable for a longer time than is absolutely necessary. On this account Malgaigne deprecates all immovable apparatus.

Irrigation, a mode of treatment in which we place so much confidence, he considers fraught with danger, and warns his readers against its use. He asserts that irrigation and the too early use and too long continued application of bandages, will retard, and can even prevent union by impairing nutrition.

We make the following extract relative to the extreme timidity of patients about the use of limbs after fractures.

"There often remains in the patient's mind a mistrust, purely moral, an instinctive fear that the limb has not sufficient strength to sustain him. This fear lessens and passes off in time, it is true; but sometimes convalescence is singularly retarded by it. Here the Surgeon must interfere, must inspire security, must, if needs be, force it; for reasoning alone hardly ever avails against instinct. I have often seen, in my wards, patients, furnished with crutches, hold the foot up from the ground for eight or fifteen days. I tell them to rest upon it;—Monsieur, it is impossible. I take away one

crutch, they grasp the foot of the bed, wavering, fearing to fall. Walk! It is impossible. I bring them into the middle of the room; I tell them there is no danger; I push them and urge them; they walk as if stupified. I take the other crutch from the armpit, make them grasp it like a cane; they again resist, saying it is impossible. However, they walk; then I take away the stick, and reach out one finger for their support; they walk again. I withdraw the finger, and they still walk. I tell them to run, and they run. To remove all pretext for any new fears, I make them jump, first on the sound foot, and then on the fractured limb; in the space of a few minutes, these men who could not walk upon two crutches, thus run and jump without any crutches at all; their courage is entirely restored."

Malgaigne wages war, as every rational man must, upon the very absurd custom of starving a person because he is unfortunate enough to have a limb broken. It is an item of no little importance to continue the accustomed habits and mode of living of the patient, as discord in the system, generating trouble, is sure to arise from any sudden change.

For the study of Fractures, we know no better work than the one now before us. The treatment laid down differs, somewhat, from that pursued at the present day, as we place most confidence in what he considered the most dangerous applications. Irrigation and removable apparatuses are now of universal adoption, and greater triumphs in practical Surgery can no where be found than among us. The work is one which no physician should be without. It has lost nothing in the translation, for which Dr. Packark deserves much credit. The one hundred and six illustrations appended to the text are faithful delineations of the plates accompanying the French work, making, on the whole, a book which we can, with every confidence, recommend to the profession.

ART. X.—*Etudes sur La Morchidie et La Cryptorchidie chez l' Homme.* Par M. ERNEST GODARD, Interne des Hopitaux de Paris, Member de la Sociètè de Biologiè et de la Sociètè Anatomine. Extraits des Memoires de la Sociètè de Biologie Annèe, 1856. Paris. 1857. p. 163.

Studies on Monorchis and Cryptorchis in Man. By ERNEST GODARD. House Physician in the Hospital of Paris. Member of the Biological Society, and the Anatomical Society. Extracted from the Memoirs of the Biological Society for 1856.

The absence of one, or both, testicles from the scrotum of the human subject, has been an interesting study to physiologists, of all ages, who have ever been assiduous in recording cases of such malformations, with remarks upon the virility or sterility of those so afflicted. In 1851, M. Follin published, in the Archives de la Medicine, an excellent paper upon the Anatomical and Pathological study of the anomalous positions and atrophy of the testicle, which was followed up, in 1856, by a valuable essay, indicative of laborious research, the conjoined work of M. M. Joubaux & Follin, which appeared in the Memoirs of the Biological Society. The above work of M. Godard is an historical review of the labors of all his predecessors ; appended to which, is the result of his individual observations upon eleven cases, with minute descriptions of their anatomical, physiological and pathological peculiarities. The result of his inquiries are at variance with those of previous writers, especially as regards the pathological changes to which testicles, retarded in their decent to the scrotum are liable.

M. M. Joubaux et Follin's microscopic study of the testicle, either retained within the abdomen or in the inguinal canal, or at the ring in the genito crural fold, revealed not only constant atrophy, the diminished volume of the retained organ being marked, but the intimate structure was profoundly altered, taking on either a fibrous or a fatty degeneration.

In eight cases examined by M. Godard, no such transformation was observed, the parenchyma of the incarcerated gland, being in every case identical with that of the healthy scrotal testicle. The diminution in size of both testicle and vesiculæ seminales was confirmed; but a most marked difference was found in the secretion from the retained organ, which contained no spermatozoa, the total absence of the animalculæ existing in the testicle, epididimis, vas deferens and vesicula seminalis of the deformed side. He calls attention to the fact that the testicle is always firmly fixed in its anomalous position, whether this be in the inguinal canal, in the groin, on the thigh, or in the vicinity of the anus, whilst the testicle, in the scrotum, is subject to constant motion from the cremaster muscle. In the one case the testicle can elude injury, in the other its fixed position renders it constantly exposed to accident, with its train of pathological conditions of inflammation, of tubercle or of cancerous degeneration. We think that he shows too much desire to establish a cause, when he accepts the motions, by the cremaster muscle, as indispensable for the secretory power of the testicle; as if spermatozoa were manufactured by this churning motion, "nous pensons que la testicle non completement descendre, ne secrete pas de spermatozoaires, parce, qu' il n' a pas la mobilitè qui lui est propre et dont il jouit dans le scrotum, où a chaque instant il est soumis aux contractions du cremaster."

He lays it down as a rule, that the non descent of the testicle into the scrotum, is equivalent to the suspension of its anmalcular generating power, and is a permanent cause of sterility, not only in man, but in all animals, having their testicles pendant in a scrotum. That although such men or animals, may exhibit every virile power, with copious emission, no spermatozoa will be found in the seminal secretion from such testicle or testicles. *In other words, a testicle not in the scrotum, is no testicle at all, as it does not possess the attributes of the organ.* His microscopic researches thus refuting the many cases found in old authors, of propagation without scrotal testicles. When only one testicle has descended, should any accident, as orchitis or epididymitis befall it, occluding the

vas defferens or excretory duct, sterility must necessarily follow. He mentions singular cases where chronic orchitis, or epididymitis has, in this way, affecting both testicles, destroyed all powers of propagation. Where both testicles are retained, even at the external inguinal ring or in the canal, no scrotum exists, the victims of this arrest of development, both in appearance, manner and voice, similating eunuchs.

Besides the valuable accumulation of all reported cases, a most laborious undertaking, M. Godard has engrossed a very minute description and explanation of the formation and descent of the testicle from the lumbar region, its primitive seat, to the scrotum, and, also, very interesting researches into the causes of sterility in the male, rendering the work a valuable contribution to medical literature.

ART. XI.—*Sur un Fonction peu connue du Pancréas, La Digestion des Aliments Azotés. Expériences parallèles sur la Digestion Gastrique et Intestinale; Inductions Cliniques.* Par LUCIEN CORVISART. Paris, Librairie Victor Masson, Place de L'école-de-Médecine, 1857–1858.

On a comparatively unknown Function of the Pancreas, the Digestion of Azotized Food. Parallel Experiments on Gastric and Pancreatic Digestion, and Clinical Inductions. By LUCIEN CORVISART. Paris: 1857 and 1858. 8vo. pp. 123.

This brochure, which consists of a series of papers published during the last two years in the *Gazette Hebdomadaire de Médecine et de Chirurgie*, is one of the freshest and most entertaining works we have lately encountered. Its title sufficiently explains the purpose of its argument, which, if not confuted, confirms and extends the views of Purkinjie and Pappenheim, who, as early as 1836, asserted that the

function of the Pancreas was not merely to emulsionize fats—as shown by Eberle—but also to assist in the digestion of nitrogenized food.

We have not space to offer, in detail, the numerous, careful and ingenious experiments and observations of M. Corvisart, which appear to us both accurate and reliable, and can only give a general resumé of the conclusions which he considers he has established. If hereafter proved correct, this essay alone must give him a distinguished position among modern physiologists, as, in any event, it earns him the distinction of an earnest and laborious worker in a great field.

1. Azotized food is digested by the stomach, and also by the pancreas.

2. The pancreas is, so to speak, a supplementary organ, whose action, after copious repasts, assists that of the stomach.

3. The nature of the two digestions is similar, in this sense, that all food submitted to one or the other of them, is transformed to an exactly similar nutritive product (albuminose or peptone).

4. The pancreatic juice exhibits, under the influence of heat, and of certain agents, particular reactions, which *the gastric juice does not exhibit.* This difference in the juices shows itself when, by digestion, they are charged with peptones, and might be wrongly attributed to a difference in the juices themselves; but the possible error being shown, is easily avoided.

5. When food, or a portion of azotized food, is completely subjected to gastric digestion, the pancreatic juice does not act upon it, and does not transform it into another peptone.

6. It is upon that part of albuminoid substances which have escaped from the stomach before having been transformed into albuminose, that the pancreas is called upon to act.

7. In certain cases, the full action of the pancreas can equal that of the stomach.

8. If one has only observed the quantity of the digestive fluids secreted, he might readily believe that of the stomach to be the most powerful, for the gastric juice is ten times

more abundant than the pancreatic juice, but by compensation, the pancreatic juice is ten times more rich in ferment (pancreatine).

9. If the action of the gastric juice is aided by remaining and becoming thoroughly mixed with the food, the pancreatic juice enjoys the great privilege of acting equally well in the alkaline, neutral or acid state, and three times more quickly upon azotized food.

10. The duodenum is so constructed that the pancreatic juice acts as soon as it encounters the food; the stomach is constructed not only that a great portion of the food may be transformed into peptones, but also that the other portion may be at least prepared to submit very rapidly to the pancreatic digestion.

11. This preparation, which varies with the quantity and the quality, both of the food and the gastric juice, etc., etc., consists as much in a simple imbibition, as in a separation or an extreme division, or a dissolution. The pancreatic digestion, considering its forced rapidity, finds in this preparation a useful aid, the stomach playing in this case in connection with the pancreas, the same rôle that the teeth do in regard to the gastric digestion.

12. In the same way that the gastric juice alone can digest, the pancreatic juice is capable of accomplishing singly the digestion of food which has not been submitted to this preparation or gastric division. Thus the albuminoid matter immediately separated into fragments, in the intestines, *in a state of crudity*, i. e., without any preparation, are there perfectly and completely digested, the action being more slow, only.

The pancreatic juice accomplishes by itself the digestion of azotized food, without needing the assistance of the intestinal juice or the bile, to acquire the digestive property.

13. When the gastric and pancreatic juices are separated, and act successively, each exercises its function fully, and the quantity of albuminose product can thus be doubled.

14. It is remarkable that if these two digestive ferments come together in a pure state, the two digestions do not go

on with the same ease; instead of the digested product being doubled by this union, it is, on the contrary, reduced to nothing, for in this unphysiological condition, the pepsin and the pancreatine destroy each other.

15. In the normal state, nature prevents this conflict by three means: 1st, the pylorus which separates the two ferments; 2nd, the gastric digestion itself, by which the pepsin in forming the peptone is exhausted; 3d, the bile, as has been demonstrated by Pappenheim, destroys the activity of the gastric ferment.

16. The bile does not precipitate the peptone products of the stomach, in such a manner that the digestion shall be destroyed and re-produced; on the contrary, it is the bile itself which is precipitated by the gastric juice, or the chyme.

17. The nature of the azotized food has much influence upon the quantity of peptone that, for the sake of economy, the two digestions can successively produce. Thus, in our experience, while the musculin and the casein furnished almost 30 grammes of perfect peptone, the albumen, or the gelatinous tissue, though given in equal quantity, produce from it scarcely 15 grammes.

18. The digestion, whether gastric or pancreatic, effaces immediately in the different albumenoid substances, their most characteristic properties; it liquefies the insolubles, takes away from the albumen its coaguability—from the casein the property of curdling on pressure, from the gelatine that of gelatinizing, from the musculin that of being precipitated by chloride of sodium, etc.; finally, it transforms all into albumenose or peptone.

The *albumenoses* having individual reactions much less pronounced than the albumenoid matters which proceed from them, do not the less preserve their differential characteristics.

19. The nature of peptone varies according to the azotised substances from which it proceeds, this variety subserving the different necessities (plastic?) of the economy.

20. The albuminose or peptones which have most analogy, and are most difficult to distinguish from each other, are the albumen peptone, the musculin peptone, and, strange to say, the gelatine peptone, as if the food from which these proceed was less different than it seemed. The fibrin peptone and the casein peptone are more easily distinguished from each other, and the preceding ones.

From the slight differences between the azotised nutriment or peptones, there results among them a kind of unstable equilibrium, favorable to the work of assimilation, accomplished by the tissues.

21. The peptones possess this generic character, that they remain always soluble in water, whether acid, neutral, or alkaline, which ensures them an easy circulation in the economy. Heat does not coagulate them; the acetate of lead does not precipitate the greater part of them. The peptones, moreover, resist generally better than azotised alimentary matter, insoluble metallic combinations.

22. The peptones form a class as well characterized as the albuminoides; it is evident, however, that that the progress of science will determine their nature much more precisely than we can do at present.

23. Some physiologists persist in the singular error of believing that the stomach only swells up, or divides the food, without dissolving it. Which denial adds considerable weight to their side of the balance who show, that all albuminoid food, taken into the stomach, is not divided, but dissolved, passes across a filter, and is absorbed through the membranes.

24. Others have maintained that the gastric juice, acting upon azotised food, only produces gelatine, without reflecting that the qualities which make the gelatine a peculiar species of albuminoid, can never be recognized in chyme, even neutralized, after a digestion of fibrin, casein, musculin, albumen, and that the gelatine itself loses definitively its specific character, that is to say, is digested in the gastric juice.

25. Finally, others, following the old hypothesis that the albumen of blood is only the matter itself digested, require that in ceasing to be acid, that is to say, being neutralized, the

peptones are resolved into albumen; the error is only possible, if, to the exclusion of other food, we only regard the albumen and fibrin, in which an incomplete digestion assists the error. In fact, the albumen, always increasing, escapes in part from the gastric digestion; the fibrin, badly digested, is only transformed into albumen (caseiform); except these two cases, if we examine the product, regularly digested by the stomach, of the concrete albumen (and washed), of the casein, of musculin, and of gelatine, there is no longer any doubt. These gastric peptones contain albumen.

26. The peptones received or produced by the pancreatic juice, do not form any new albumen, and though primitively or consecutively acid, alkaline, or neutral, do not augment by an appreciable weight the coagulable albumen, which the pancreatic juice, pure, and without peptone, normally contains.

27. During the three hours which follow a repast, the time in which the dissolution, transformation, and digestive absorption are very little advanced, the blood of the portal vein (compared with the blood of the veins generally) is not enriched by a sensible quantity of azotized material, from digestive absorption; on the other hand, in the intestines, or in a vapor-bath, under the influence of the alkaline pancreatic juice, the elements of the blood, globules, fibrine, are transformed into albumen (caseiform) at the commencement of digestion.

28. Now if we consider that during the three first hours of digestion: 1st, the pancreatic juice poured into the duodenum remains there in a pure and active state; 2nd, that it can pass into the portal vein, for absorption by the mesenteric veins is not suspended; 3d, that the pancreatic juice can exercise its digestive action in an alkaline medium like the blood;—if we consider, on the other hand, that precisely during these hours a great part of the globules and of the fibrine of the blood of the portal vein is transformed by equal weights into albumen (commencement of the transformation similar to that which they would be subjected to in the intestine under the influence of the same pancreatic juice) it is difficult

to avoid the hypothesis, which I formulate distinctly, of a veritable intra-venous digestion.

29. We cannot distinguish between the azotized matter called extractive, and the albuminose products of gastric or pancreatic digestion, any really differential character. Yet the chyliferous vessels, the portal vein, and its continuation, the hepatic veins, that is to say, the vessels which receive directly the products of digestion, are richer in extractive matter (albuminose) than the rest of the blood; we remark that they contain more glucose also.

30. The richness of the vessels of the liver in nutriment (albuminose, glucose) is explained by the gastro-intestinal absorption, to which is joined in an active manner the prolonged intra-venous digestion, without the liver, in itself, effecting anything.

ART. XII.—*A Treatise on Diseases of the Air Passages; comprising an inquiry into the History, Pathology, Causes and Treatment of those affections of the Throat called Bronchitis, Chronic Laryngitis, Clergyman's Sore Throat, etc.* By HORACE GREEN, M.D., L.L.D., President of the Faculty and Emeritus Professor of the Theory and Practice of Medicine in the New York Medical College, etc., etc. Fourth Edition, revised and enlarged, with an Appendix. New York: Wiley & Halsted. 1858. 8vo. pp. 348.

The above work is dedicated to Dr. Valentine Motte, who does not seem to have been overwhelmed by the compliment, or else has no more favors to expect from this quarter, since he has recently arraigned the author before his peers, on a charge of having put to death an unfortunate patient by passing a probang through his trachea into the lungs. We believe that Dr. Green has been acquitted, after a hearing, but expect that the dedicatory page of the next edition—

should it reach a fifth—will bear another name than that of the distinguished surgical veteran of our great metropolis.

The public, we believe, are, by this time, well acquainted with Dr. Green and his specialty, through the latter of which, indeed, he has achieved his fame, and the exhaustion of four editions of his Treatise on Bronchitis indicates, we may assume an equally extensive knowledge, on the part of the profession generally, of his particular views on the pathology and treatment of diseases of the air-passages. When, in 1840, he brought before the New York Medical and Surgical Society, the subject of the treatment of diseases of the larynx, by means of the direct application of therapeutical agents to the lining membrane of that cavity, it was the subject of much animadversion from the greater part of the members, and no little skepticism was expressed, both in this country and over the water, in regard to the truth of his statements. Our English and European comperes, indeed, have until recently, stoutly denied—notwithstanding the affirmations of many distinguished American practitioners, of the practicability of making topical applications to the surface of the larynx and trachea—the possibility of manipulating in the manner asserted by Dr. Green to be practised by himself, but recently they seem to have yielded to conviction, and exhibit a somewhat fairer animus towards American art. both in this and other particulars. Trousseau, long a bitter and denunciatory opponent, J. Hughes Bennett, in England, and Prof. Griesinger in Germany, have admitted the practicability of topical medication after the method first proposed by our author, and we suppose, ere long, its value will be generally recognized and tested by practitioners universally.

If Dr. Green has not kept his skirts altogether clean, if he has occasionally indulged in practices which his brethren cannot commend, and said and done, as most of us may at some unlucky moment, things neither wise nor prudent, he has yet achieved a noble and useful triumph, and our profession and humanity owe him a large debt of gratitude for the ingenuity, perseverence, resolution and skill which he has exhibited in originating and perfecting an instrument of un-

doubted value, and a method of appliance largely available in the successful management of a class of diseases, distressing, pertinacious, sometimes fatal, and, until his day, comparatively incurable.

The four first chapters of the work treat of the Anatomy, Physiology, and Pathology of the Larynx, Trachea, and Bronchi. Chapters four, five, six, seven and eight, discuss the Etiology and Pathology of follicular affections of the air-passages, and the remaining two are occupied with the special and general treatment which he advocates. The latter portion of the work we sincerely recommend, as containing theraputical directions and suggestions eminently wise and practical.

An appendix which constitutes the bulk of the enlargement made to this edition, contains an abstract of the views of those European writers whose testimony and experience is corroborative of the value of the opinions and practice of our author.

ART. XIII.—*A System of Human Anatomy, General and Special.* By ERASMUS WILSON, F.R.S. A new and improved American, from an enlarged London edition. Edited by William H. Gobrecht, M.D., Professor of Anatomy in the Philadelphia College of Medicine. With three hundred and ninety-seven illustrations on wood. Philadelphia: Blanchard & Lea. 1858. 8vo. 614.

We hail with pleasure another edition of Wilson's famous and familiar Vade Mecum. In some respects Quain & Sharpe's great work is a more desirable book for purposes of reference, but as a companion in the dissecting room, convenient, accurate, full, we know nothing equal to this well known and well worn volume. Indeed, we doubt if, for its intended purpose, that of indoctrinating beginners, "Medical Student's,"

technically so called, into the mysteries of Anatomy, any more admirable Treatise will ever be prepared.

In the preface to this edition, Dr. Wilson acknowledges his obligations to the voluntary assistance of Prof. Retzius, whose supervision and emendations cannot fail to have secured additional accuracy and completeness to its pages. He has also placed on record, with distinguished propriety, an act of liberality on the part of our great medical publishing house, which it not only gives us pleasure to mention and approve, but which, we trust, may be accepted by their brother publishers every where, as an example worthy of all praise and imitation.

The following extract from a letter addressed to Dr. Wilson, by Messrs. Blanchard & Lea, explains the incident referred to:

"You may, perhaps, be acquainted with the name of our house, as having published in this country your works on Anatomy and Diseases of the Skin. When English works which we issue are successful, it always affords us pleasure to make the author an acknowledgment more substantial, if not more gratifying, than the transatlantic reputation thus obtained; and we, accordingly, take pleasure in handing you the annexed draft, which you will do us the favor to accept."

ABSTRACTS AND TRANSLATIONS

FROM FOREIGN AND DOMESTIC JOURNALS.

PATHOLOGY AND PRACTICAL MEDICINE.

Typhoid Fever in Children. From the Second Edition of the Traité des Maladies des Enfants, by MM. F. RILLIET and E. BARTHEZ. Translated for the Charleston Medical Journal and Review. By DAVID E. EWART, M.D., Newberry, S. C.

[Continued from Vol. XIV., No. 1, p. 109.]

ART. III.—SYMPTOMS.

SEC. 3. *Organs of Sense.*—The lesions of the functions of the organs of sense are either rare or difficult to verify. We have nothing to say concerning taste and smell. The hearing, on the contrary, offers many symptoms: *tinitus aurium*, which can very rarely be ascertained with certainty; *deafness*, which is somewhat more frequent: this generally comes on from the ninth to the twenty-sixth day, and is, most frequently, coincident with an otitis, though sometimes independent of it. Diminution of cutaneous sensibility is an extremely rare phenomenon, and one which belongs to the gravest cases. Disturbances of vision, like vertigo, are very difficult to recognize. We have but rarely seen dilatation of the pupils; and still more rarely, and only in grave cases, contraction: this symptom presents itself either at a period just preceding death, or, when the termination is favorable, at a very advanced stage of the disease.

Cutaneous Eruptions.—Papulæ.—Sudamina.—Typhoid fever bears some resemblance to eruptive diseases, from the tendency of the skin to become partially covered with certain eruptions, and from the sanguineous congestions which take place in some of the mucous membranes. Every one knows the diagnostic value of the *papulæ* and *sudamina*. The symptom of rose-colored lenticular spots in children, present characters analagous to that occurring in the adult; they are

round, flat papulæ, which disappear on pressure. They are found principally on the abdomen, chest, and upper part of the thighs. In exceptional cases, to which we will again refer, we have observed them on other parts of the body.

In a very few instances, we have seen the eruption make its appearance on the fourth day; but usually it manifests itself between the sixth and the tenth or twelfth day; in two of our cases, it re-appeared during a relapse, between the forty-fifth and fifty-ninth day from the commencement of the first attack. When there has been no relapse, the latest period at which we have noticed these spots to appear is the twenty-ninth day. The duration of the eruption, though sometimes very short, (two, three, or four days) is generally seven or eight days, rarely longer. In a large majority of cases, the number of spots is small, being from one to six at a time. This conclusion, to which we had already come in 1840, is the reverse of that obtained by M. Taupin, but it is in conformity with the researches of MM. Rufz, and Stœber, whose works are more recent than ours. In a few cases, there are from two to twenty spots; and in some very rare and exceptional ones, the eruption is so abundant as to cover the entire surface of the body, and resemble an eruptive fever. We have reported one case of this description in a thesis of which we have already spoken. In this case, there was noticed on the whole anterior portion of the trunk, an eruption of pale-rose colored, and but slightly elevated spots, confluent at some points, but mostly separated; we have published a similar example in the *Journal des connaisances medico-chirurgicales.* M. Taupin has seen three such cases. These rose-colored spots are not always observed in typhoid fever. This we have before stated, and recent facts collected by us, though slightly changing our former statistics as to the proportion of its relative frequency, (three-fourths instead of two-thirds) have nevertheless proved to us that the eruption is frequently wanting. Besides, these spots occur in other diseases than typhoid fever; thus, we have noticed them in two patients who died of enteritis, and in others attacked with slight gastritis; in all these cases, however, the eruption was sparse, and of short duration. We have endeavored to ascertain whether the abundance of the eruption is in any way proportionate to the intensity of the disease, and we have arrived at the conclusion that, generally, in very grave cases, the eruption is quite moderate, and frequently even wanting, whilst in light cases, or those of moderate intensity, it is both

more constantly met with and more abundant. But though the results are very generally true, they offer many exceptions.

The *sudamina* ordinarily appear at a somewhat later period than the rose-colored spots; twice only have we noticed them on the fifth day, and once on the sixth and seventh; in all other cases, it was from the eighth to the twentieth day, rarely beyond this period, that they appeared. Contrary to what is the case with the lenticular spots, sudamina are very numerous, and generally as much more so as the period of of their appearance is delayed. Their duration is not long; though in some cases we have seen successive and abundant eruptions appear for twenty consecutive days. Sudamina are ordinarily seated on the sides of the neck; when abundant and confluent, they are found on the abdomen. We will see in the article *Treatment* that the remedies used may have something to do in their production.

Epistaxis, which indicates a sanguineous congestion of the pituitary membrane, is by no means a rare phenomenon (in a fifth of the cases); but, what is remarkable, it scarcely ever takes place the first day. In two entirely exceptional cases, we have seen it last, in one, from the first to the sixth day, and in the other, from the first to the ninth. It generally takes place between the tenth and twentieth day; sometimes, there is but one hemorrhage; at other times, there are two, three or four. It is not usually very intense; we have never seen it occur to such an extent as to constitute an accident of any gravity. Once only have we observed it to be abundant; it was repeated on the tenth, twelfth, fourteenth and twenty-second days of the disease.

Sec. 4. *Circulatory System.*—We have been better able in our private than in our hospital practice, to study the characters of the fever at a period near its commencement, and we have ascertained that nearly always during the first three or five days, and often during the whole of the first septenary period, the fever is manifestly remittent. There is not a moment of the day in which the pulse is not found more frequent than in health, the heat of skin, especially in the palms of the hands and folds of the neck, more elevated; but, at certain times of the day, all these phenomena assume a greater degree of activity; the face becomes flushed, the pulse more frequent, the skin hotter; then follows another remission, which is again succeeded by a fresh febrile exacerbation. The febrile recrudescence is especially marked in the middle of the day and the evening. We have, however,

noticed it to occur in the morning also. These exacerbations are sometimes of only an hour's duration, but generally they persist for several hours. During the remissions, the child may yet be sufficiently well to be up and about, and though he may no longer play with his usual liveliness, yet he is still pleased and attracted by surrounding objects. After the third or seventh day, the fever becomes continued, but still preserves a tendency to exacerbations. The skin is generally very hot and dry, especially in grave cases, the frequency of the pulse varies according to the age of the patient and the gravity of the disease. We have counted from 100 to 140 pulsations in the oldest children, and from 160 to 180 in the youngest. The more continued and intense the fever is at a period near its commencement, the more we have to dread a grave disease. Thus far, we have said nothing of chills: it is because we have seldom ever observed them; our results in this respect, in our private practice, exactly accord with those obtained at the hospital, and are contrary to the opinion of M. Taupin, who has noticed violent chills in the commencement of the disease, and for several days following, in more than half his cases. The fever persists during the entire course of the disease.

We have observed nothing absolutely regular as regards the exact period at which the febrile movement ceases. Yet in our private practice the figures fifteen, twenty, and twenty-one are those most frequently met with in our notes in this respect. Our confrère, Doctor Dufrène, has observed during one epidemic, that out of twenty-two children, the febrile movement ceased in eight on the fourteenth day, and in eight between the twentieth and twenty-second day.

At a more or less advanced period, and especially in the gravest cases, the pulse becomes small, thready, difficult to count, and irregular, or, soft and compressible. It is rare to find the pulse simultaneously slow and irregular, which is sometimes, as in meningitis, remarkably wiry; but in certain ataxic fevers, it may occur. We ourselves have sometimes noticed it in our private practice; and if other indications, (diarrhœa, dry lips and tongue, and burning heat of skin) had not caused us to persist in our diagnosis, this symptom was sufficiently characterized to have made us distrust our opinion as to the true nature of the disease.

Dryness of the skin is very perceptible during the two first septenary periods, and sometimes during the whole course of the disease. But after the twelfth or fifteenth day, or later, perspirations may be noticed. In cases in which these have

been very abundant, they have appeared to us to be the result of the medicines (sulphate of quinine). At other times, on the contrary, owing either to the form of the disease, or the prevailing epidemics, we have been able to observe a moisture of the skin or sweats, during the first week.

Respiratory System.—1ST. *Respiration.*—The respiration is more or less accelerated, according to the intensity of the bronchial inflammation. But it is necessary to bear in mind two facts. The first is, that dyspnœa is very considerable in the beginning of the malady, in grave cases, even when the respiratory organs offer no signs of disease. This phenomenon is of the same order as that which we pointed out as occurring in ataxic rubeola. The second is, that the respiration is sometimes irregular and sighing,* and that, too, in exactly those cases in which are observed other delusive symptoms, such as slowness of pulse, moaning, yawning, changes in the color of the face. and sharp and causeless cries. This, we repeat, is an exceptional symptom, but it is well to be apprised of it, for we have seen mistakes in diagnosis committed, and we have committed them ourselves from an ignorance of it.

2ND. *Cough.—Auscultation.*—At the outset of the disease, there is no cough, and the respiratory murmur is good; in rare cases† it is thus during the entire course of the malady; but it is oftener the case that after the fifth day, seldom sooner, sometimes later, from the tenth to the fifteenth day (Taupin) the cough appears, followed, at the expiration of a few days, by a sibilant bronchitis. This bronchitis, an almost necessary accompaniment of typhoid fever, is a valuable diagnostic symptom; when it goes beyond certain limits, it becomes a very dangerous complication. We will again speak of it.

ART. III.—DESCRIPTION OF THE DISEASE—VARIETIES, PROGRESS, DURATION, ETC.

For the convenience of description, and especially in view of the treatment, we will distinguish three varieties of typhoid fever: *light*, *grave*, and *very grave*. These divisions are partly arbitrary, yet they possess the advantage of grouping together cases which may present some similarity.

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* M. Taupin has noticed this in eleven of his cases.

† At the Hopital des Enfants, the proportion of these cases was about a seventh; in private practice, a tracheo-bronchitis was still more rare.

*Mode of access.**—Whatever may be the progress of a light, grave, or very grave typhoid fever, we may nearly always recognize two phases in its access: the one called the premonitory stage, the other the commencement proper.

It is very rare in typhoid fever that the passage from health to disease is made suddenly, and even in cases in which apparently a dothinenteritis seems to come on abruptly, an attentive investigation of all the circumstances which have preceded the invasion will nearly always disclose a more or less marked derangement of the general health. Thus, the child is less lively than ordinarily. He sometimes has an unusual disposition to sleep during the day; his eyes are surrounded by a dark circle; he looks fatigued; his parents find him changed in appearance and character, and enfeebled; his appetite is diminished, his bowels rather costive; his nights are not passed as quietly as formerly, he is restless; the heat of skin, especially in the palms of the hands, is at times greater than it should be. The child asks for water frequently. By the close of this period, he has already lost flesh, though he may not have yet been regarded as positively sick.

Light form.—After a variable duration of these symptoms, (generally from three to eight days, but rarely more) there arises an intermittent form of fever, which marks the commencement proper; it is accompanied by a somewhat intense headache, anorexia, thirst, slight abdominal pains, with diarrhœa or constipation; there is no vomiting. Most frequently, during the first few days, the child is not bed-ridden. The tongue is moist, and covered with a thin, white coating; the abdomen is yielding. The expression is dejected, the forces depressed, the eyes surrounded by a bluish circle, the nostrils dry; the nights are tolerably well passed, or there may be a slight restlessness. For five or six days, these symptoms persist without any change, though sometimes the restlessness becomes more decided, and is accompanied by a transient delirium; the abdomen is larger, the coating on the tongue thicker, the bowels looser, and the pulse more accelerated. Then, after the sixth or eighth day, the headache ceases, and a few rose-colored lenticular spots may be perceived on the abdomen and inner part of the thighs, and sudamina on the sides of the neck, or on the abdomen. The

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* It is in our private practice that we have been best enabled to study this question.

tongue is not now so moist as it was; the gums are slightly swollen, and covered with small white patches; the abdomen is distended and tympanitic, and there is a gurgling sound on pressure in the iliac fossa. The cough now appears, or, if it already exist, it increases, and very frequently a pretty abundant sibilant râle is heard at the posterior part of the chest. A little later in the disease, the spleen is sometimes felt jutting beyond the ribs, or rather the left hypochondrium is not so sonorous as in health. These symptoms remain stationary for several days, and then successively disappear; the tongue again becomes moist, and is cleaned of its coating; the abdomen becomes yielding and soft, the diarrhœa is checked; the fever diminishes, the restlessness disappears; the cough ceases; the lenticular spots are no longer to be seen, but the abdomen is yet covered with sudamina; the countenance no longer expresses dejectedness, the eye resumes its vivacity, the strength returns, and the patient enters upon convalescence. On an average, the disease scarcely lasts more or less than from fifteen to twenty-one days.

In this form the disease terminates, in a vast majority of cases, in a return to health.

Grave Typhoid Fever sometimes commences as a light dothinenteritis, and it is only after the expiration of six or seven days, that the gravity of the symptoms is made apparent; though sometimes, from their greater violence, this is very evident from the commencement of the malady.

The febrile movement and the headache are intense, and constipation, as well as vomiting, of frequent occurrence. A loose condition of the bowels soon follows the constipation, especially in the youngest children, in whom the stools are involuntary. The thirst is great, and there is complete anorexia; the tongue is covered with a thick coating, and is often red and dry at its tip. The abdomen is painful at the umbilicus or iliac fossa; there is a gurgling sensation on pressure; the countenance expresses prostration and languor, the vital forces are very much depressed, the delirium is now very decided. The child is restless, especially at night; during the day it is in a state of somnolency. One can rarely verify the existence of tinitus aurium, or vertigo. The lenticular eruption is pretty abundant, the sudamina are numerous, the cough is intense, and the sibilant and sub-crepitant râles are heard over the greater part of the chest; sometimes an epistaxis supervenes. The size of the abdomen is increased; it is swollen, sonorous, distended with gas; sometimes its parietes become so thin as to be almost transparent.

Sometimes the febrile movement, with severe evening exasperations, may be the most prominent feature of the disease. At other times, the abdominal form predominates; in these cases, the tongue is dry, the diarrhœa abundant, the abdomen much distended; vomiting appears from time to time, accompanied with great prostration. At other times again, the thoracic symptoms are the most apparent; the cough is increased in frequency, the respiration is accelerated, the bronchi are obstructed with mucous, the countenance has at times a purple hue, the skin is often covered with sweat, depending either upon the catarrhal condition or upon the embarrassed respiration. Then, after a variable duration, these morbid phenomena progressively diminish, and the patient becomes convalescent; or, the symptoms may maintain almost the same degree of severity, and a complication may arise, sometimes benign—as an otitis,—at other times very grave—as a pneumonia, a pleurisy, an intestinal perforation, etc.—which destroys the patient. This form of typhoid fever rarely lasts less than twenty-five days, and sometimes is prolonged as far as the thirty-fifth day.

Very grave Typhoid Fever, seldom ever commences with the appearance of a benign typhoid fever* Sometimes it commences as the preceding variety; but again in other cases, the intensity of the symptoms is so great from the outset that we may readily anticipate the gravity of the disease. Thus, the fever is very high, accompanied with a burning skin, intense headache, great feebleness, and pretty constant vomiting; there is frequently constipation; the features express prostration. At the expiration of a few days, there supervenes a delirium of the most intense sort, an extreme agitation, soon followed by a profound prostration, which is again succeeded by fresh exacerbation of delirium and agitation. Now, the head is thrown back; the eyes are sometimes closed, sometimes half-opened, sometimes spasmodically drawn up under the upper eye-lid; the pupils sometimes dilated, sometimes contracted; the lips are convulsively moved about and trembling; the countenance changes expression at every moment: sometimes it expresses great wildness, sometimes the child appears to sleep, but in a few moments after, he starts from this lethargy and gives vent to sharp, prolonged and plaintive cries; he constantly changes his position,

* Yet in our private practice, we have seen a boy, aged ten years, pass from a condition apparently but little alarming, to an ataxic fever of the highest gravity, to which he succumbed on the seventeenth day.

which is sometimes dorsal, sometimes lateral; the cries become more and more intense, the respiration irregular and sighing, the pulse sometimes irregular. The constipation is succeeded by diarrhœa, abundant evacuations fill the young sufferer's bed; the urine is passed involuntarily. Prostration at last predominates, there is complete insensibility, the body becomes as stiff as a bar of iron, the intelligence is destroyed, the face is covered with a cold sweat, and death closes the scene, at the expiration of seven or nine days.

But the disease may last a much longer time; in such cases the delirium is very high and persistent, alternating with drowsiness, the pupils dilated; subsultus or carphologia is sometimes observed. The tongue becomes covered with a thick, fuligenous coating; the teeth are encrusted, the lips dark, excoriated and red, the nostrils pulverulent, the face pinched, the pulse small and thready, the abdomen immensely distended, or sometimes retracted. Sloughs are formed on the sacrum and greater trochanters; and death comes on between the fifteenth and twentieth day.

At other times, the disease terminates favorably; the symptoms progressively and successively diminish; the tongue is cleaned of its coating, the abdomen regains its suppleness, the evacuations become natural, the sloughs afford a healthy suppuration, and after many alternate changes for the better, and for the worse, the patient enters upon convalescence. At the hospital we have seen the disease last for eighty days, but generally, it terminates between the thirty-fifth and fiftieth day. In our private practice we have seen children out of danger by the fifteenth or eighteenth day, and convalescent by the twentieth or twenty-fifth.

The three forms, which we have just described, are not equally frequent. In our hospital, as well as in our private practice, but, especially, in the latter, the light forms are the most frequent, and the grave varieties the most rare.

In comparing the lesions with the symptoms, we are satisfied that in almost all the cases in which we found no intestinal ulcerations, the form was very grave, and it was also the same case when death took place, between the fifteenth and thirty-sixth day.

The division which we have adopted for the establishment of the different varieties, appears to us to be the most practical. If we now wished to consider the facts under a different point of view, and to group together our observations according to the predominence of certain symptoms, we might establish the following comparison:

To the light form generally correspond the varieties called	Mucous, bilious.
To the grave form,	Inflammatory, bilious, pectoral, abdominal.
To the very grave form,	Cerebral, nervous, malignant, ataxic, adynamic, ataxo-adynamic.

Convalescence in light typhoid fever is generally short; in the grave form it is longer! The patients remain weak, become very thin, and their skin is pale, dry and scaly. In the very grave form, when the disease is of considerable duration, the convalescence is still longer, and the emaciation greater.

Mr. Taupin has observed that children nearly always lose their hair at this stage of the disease. It is often, during convalescence that many of the complications appears, to which we will devote a special article. We have never observed critical phenomena to follow typhoid fever. M. Taupin places in the number of crises, furuncle, anthrax, phlegmonous abcess, and ottorrhœa; he gives to this complication the name of crisis *par excellence.*

Relapses.—Dothinenteritis is essentially a continued disease, and except some alternate ameliorations and aggravations of symptoms, it follows its usual stages. We have however observed a relapse to supervene in three children after a complete cure. Of these three patients, two were eleven years of age, and one thirteen. In two of them the disease appeared under the benign form; the third was very seriously affected. No cause could be offered for the reappearance of the typhoid symptoms; the convalescence had been thorough, and we had kept the patients in their beds for several days after their cure. The symptoms, after the relapse, were very similar, in their march and intensity, to those of the first attack. The typhoid eruption, in particular, was very abundant, and in one of them it was even more so than it had been in the first invasion. In the same child, the spleen was so much enlarged as to protrude below the ribs, which had not been the case when first seized. We took great care to assure ourselves that no complication could account for the symptoms observed. The question might arise, what had been the condition of the intestinal canal at the time of the relapse: had there been a fresh inflammation of Peyer's plates and the mesenteric ganglions? This is probable, since we were enabled to discover, in one of the patients, the lesion

the spleen which usually accompanies the two others. But fortunately the anatomical proof was wanting.*

Anomalous and Secondary Typhoid Fever.—All the cases which we have analyzed, for the composition of this chapter, were examples of primary typhoid fever. It then appears that dothinenteritis, exclusive of all other diseases of childhood, always comes on in perfectly healthy subjects. It has been a question with us whether there does not exist bastard, irregular or anomalous typhoid fevers, representing similar forms of eruptive fevers, and resembling the well characterized typhoid affections, sometimes by their symptoms, at other times by their anatomical characters. These ideas were suggested to us, on the one hand, by the study of certain forms of enteritis, the symptoms of which offer considerable resemblance to those of dothinenteritis, and by the possibility of confounding the two affections in young subjects; and on the other hand, by the frequency of the inflammation of Peyer's glands, and the existence of veritable ulcerations of the agminated follicles in children in whom the disease under which they suffered, had offered no resemblance in its march to the typhoid affection. Might not one also compare to anomalous typhoid fevers, those cases, so numerous, in which we see children suffer from the epidemic influence, without, however, offering all the symptoms of dothinenteritis, and remain thus for eight or ten days in an irregular condition, between health and disease?

It is impossible for us, at present, to give a definite solution of these questions; we must be contented with directing the attention of pathologists to them. In closing this part of our subject, we will give an abridged report of a case which might be considered as an example of secondary typhoid fever. This case, about which we had some doubts, did not enter into our analysis.

A boy, aged five years was attacked successively and in the space of a month, with scarlatina and then rubeola. These two diseases were followed by considerable emaciation, and an extreme debility. About fifteen days after the disappearance of the rubeola, the diarrhœa and cough, which had existed during the exanthema, reappeared; the thirst was great, and the appetite lost; the child was irritable; there were no cerebral symptoms. Five days before death, a gangrene of the mouth was developed. Two days later he came under

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See the cases which we have published in the *Journal des connaissances medico-chirurgicales*, May, 1811, p. 198—199.

our observation. We then discovered the existence of the gangrene; besides, the fever was high, the pulse 144; the countenance expressed suffering; the strength was very much depressed, and the emaciation extreme; numerous sudamina covered the neck; there were no lenticular spots to be seen. The abdomen was pretty large, tense, and generally sonorous, but only slightly painful on pressure; the appetite was entirely lost, the thirst great, and there was an abundant and greenish diarrhœa. There was no cerebral symptom, save the flagging intellect. Upon auscultating the chest, a sub-crepitant râle was heard on both sides posteriorly.

On the two following days, no modifications occurred in the abdominal symptoms; the general condition was lower, the gangrene more extensive.

By an autopsy we discovered: 1st, a partial lobular broncho-pneumonia; 2d, two turbercles in the bronchial ganglions; 3d, a slight nephritis; 4th, besides the intestines, the mesenteric ganglions and the spleen offered the following alterations:

The *small intestines* contained a slightly abundant, yellow, liquid matter; the mucous membrane was consistent. There were fifteen or sixteen plates that were very soft; they were red, elevated about half a millimetre and had small areolœ. The sub-mucous tissue was healthy. The two lowest plates were of the size of a two franc piece; they were mamillated, formed of unequal monticules, some of which were raised two or three millimetres above the surrounding surface; their colour was a reddish gray. There were, besides, four ulcerations discovered, two of which were upon the last mentioned plates, and two others at ten or fifteen centimetres from the ileo-cœcal valve; they were small, round, with thin borders, not decollated, with sub-mucous bottoms, and but very slightly injected; their form was regular, their diameter from two to five millimetres.

The *large intestines* were of a rosy-gray color; in no part of them was the consistence of the mucous membrane affected, though it was slightly increased in thickness. The follicles were pretty numerous and were surrounded by half a score of small, round, but somewhat uneven ulcerations.

The *mesenteric ganglions* were from the size of a lentil to that of a large bean; there were five or six of the latter size, principally near the valve; they were red, pretty soft, and infiltrated.

The *spleen*, tolerably large, soft, and of a bright red color, ten centimetres long, seven wide, and two thick.

We find in this autopsy, the anatomical characters of dothinenteritis, as the triple lesion of the follicular apparatus of the intestines, the mesenteric ganglions and the spleen, coexisted in the same child. If we wished to establish the age of the lesions from their first appearance, we would say that they had existed for twenty or twenty-five days. Now, the rougeola had made its appearance on the 26th of May; it was after the 11th of June that the diarrhœa and other symptoms enumerated above came on, and death took place on the 1st of July. Thus in comparing the time at which the abdominal symptoms were developed with the presumed age of the lesions, we discover a coincidence which allows us to establish the relation of cause and effect between the lesions and the symptoms. This case might really have been a secondary typhoid fever, for the child's health had not been entirely re-established; he had remained greatly debilitated and emaciated in consequence of the two eruptive fevers. If this case leaves any doubt in the mind of the reader as to the existence of a secondary typhoid fever, it will not be so with the following observation published by M. Marèchal in the *Journal hebdomadaire* (tom 11, page 116, 1829.) We will give a succinct resumè of it.

It occurred in a girl, aged ten years, who was suddenly seized with vomiting and diarrhœa. On the second day a rougeolous eruption came out, and passed through its different stages in eight days. The gastro-intestinal symptoms, which had ceased during the course of the exanthema, re-appeared a few days after it had left the patient, accompanied with stupor, headache, and vague pains in the abdomen. When brought to the hospital it was discovered that there existed well marked symptoms of typhoid fever. Five weeks after the commencement of the illness, the child died, and the autopsy revealed the anatomical lesions of typhoid fever; the ulcerations in the small intestines were even more numerous than are generally observed in children.

(TO BE CONTINUED.)

PHYSIOLOGY.

ART. XV.—*Dynamoscopy.* From the American Journal of Medical Sciences, with corrections by the author. By J. Mc. F. GASTON, M.D., Columbia, S. C.

In an article translated from the *Journ. de Med. et de Chirurg. Prat.* in the *Charleston Med. Journal* for May last, on the subject of dynamoscopy, this language occurs: "We have to announce another application of the aural sense to the diagnosis of disease; but in this service the stethoscope does not only inform the practitioner of the general state of his patient; it becomes, in addition, an important means of investigation in a host of special cases, and opens the door to a kind of exploration hitherto entirely unknown."

The process of investigation is subsequently spoken of as a discovery of M. Collongues, and the reader is left to infer that the data upon which he proceeds were not previously known to the profession. While I admire the zeal which is manifested in undertaking to analyze and systematize the various indications which may be presented in health and disease, it is due to science and to our country to have it understood that the basis of this discovery may be found in an addition of *Horners Anatomy* as long ago as 1843, and doubtless earlier, as this is the sixth edition of the work. On page 363 of vol. i., near the bottom, we read the following: "It is owing to the vibratory motion in the fibres of muscle during their contraction that a rustling is heard in the application of the stethoscope to them. The hollow, distant rumbling when the meatus externus is closed by the finger is owing to the same vibration in the muscles of the finger employed. This is readily proved by the following experiment: Close the meatus with the end of the handle of an awl or a fork pressed against it by the finger, and it will be found that the muscular vibrations are continued along the instrument; plant afterwards the point of the instrument upon a soft, inelastic substance, so as to make, in that way, the closure of the meatus, and the rumbling will instantly cease."

Those who have read the article on the dynamoscope will at once perceive that all the points, as to the data of the investigation, are clearly and definitely presented above in the extract from Dr. Horner's work, and to him belongs the originality or priority in detecting these phenomena, and even suggesting, by the awl-handle, the mode of investigating them which is now proposed.

We are none the less indebted to M. Collongues for the practical use which he proposes to make of the signs of organic action as revealed by this process, and it is much to be desired that his investigations may be continued.

It is be regretted that nothing more specific appears as to the intensity or feebleness of these sounds in an inflammatory or excited state of the organization contradistinguished from an anæmic or depressed condition of the system. The most important features of these sounds must have reference to the sthenic and asthenic phases of life, and while the extremes of these opposite states are very clearly defined and well understood, it is very desirable to have some clearer insight into the distinction between their near approaches to each other, and such, we trust, may be presented in these signs. The inquiries should be directed especially to this end until they lead to some useful diagnostic indications.

In the translation before us, no intimation is given of the views of the author as to the source of the sounds, and we are left with Dr. Horner's explanation based on muscular vibrations, which, to my mind, is not satisfactory. First, because the sounds are greatest where there is least muscular development, viz ; at the extremities of the fingers and toes; and second, that the parts may be kept in a state of complete immobility for any length of time without lessening the sounds, and hence I infer they do not result from muscular activity in any shape whatever.

There is an explanation of these sounds, which I beg to present to the consideration of the medical philosopher, and to the careful investigation of the physiologist.

In an article entitled "A Criterion for Stimulants," which was published with the *Transactions of the South Carolina Medical Association* for 1855, I took the position that the capillaries serve an important part in facilitating the passage of the vital fluid, and I considered the relative promptness with which this portion of the circulation performed its office in different conditions of the system, maintaining that the promptness or delay of return of colour after pressure upon a part, was an index to the tone of the capillaries, and thus to the general organization. This view has been corroborated by farther observation, and leads me now to submit to the public my impression that the sounds detected by placing the ends of the fingers in the ear, and also noticed in other parts of the frame, ensue from the passage of the blood through the capillary vessels, which are, of course, distributed chiefly at the extremities, where the arteries cease and the

veins are developed to return the blood. To test the matter, I enveloped the extremity of my index finger with collodion, thus constricting it so as to produce painful tightness, and upon placing it in my ear the sound, which had previously been very perceptible, was almost entirely absent. Any one who tries this simple experiment will perceive at once the great difference produced by the arrest of the circulation in the extremity of the finger from this uniformly tight bandage, and will be prepared to appreciate my reasoning on this point, in tracing the absence of sound to the absence of capillary circulation.

In illustration it may be mentioned that in varicose aneurism a sound results from the passage of the arterial blood into the venous circulation, and indeed any fluid which is forced from a vessel through a channel or channels of smaller calibre, moves for the time being with so much greater speed and friction as to favour the idea of an audible movement amongst its particles. The passage, also, from one medium to another by force always is attended with sound, and we must advert specially to the sudden transition in this respect which occurs through the tubular network of the capillaries. It is known to all that the composition of arterial and venous blood differs in so much as to be readily detected even by the eye, and must, therefore, present as it were different media, between which a connection is established and maintained by the capillaries. Before entering the heart again, the venous trunks collect into two main channels, and the blood is conveyed through the right ventricle to the lungs to be renovated; and here again we discover sounds which have been referred to the entrance of the air into the bronchial tubules. That some portion of these sounds may result from the passage of the blood through the capillary network of the pulmonary tissue is not improbable, and demands a thorough investigation. The cause of the sounds which result from the heart's action is involved in so much controversy as to throw no light upon our subject, and indeed there are not such points of comparison as to afford much assistance even were these sounds accurately understood. But it is known that in the passage of the blood through this organ, sounds do emanate from it; and in so far a presumption is raised in favour of the explanation I have proposed, of the sounds at the ends of the fingers and on other parts of the body.

It is not my province to enter upon any analogical course of reasoning, but there are many things in connection with the ordinary processes in the arts illustrating the fact that

when the space through which a fluid passes is suddenly contracted and again enlarged, it gives rise to a sonorous movement, and I would simply refer the reader to a consideration of these facts as throwing some light on the subject before us.

This whole matter may be viewed by some as an impracticable undertaking, but when it is remembered that the vital phenomena, which go to make up the indications of the various states and conditions of the organizations, are not by any means fully understood, and that every feature which can be presented tends to elucidate the laws of health and disease, it does not become the scientific explorer to shut his eyes to such peculiar facts as those about which dynamoscopy treats. In the paper to which allusion is above made, I undertook to show the importance of the capillary system as an index to the nervous system and the general circulatory system, and if it should appear upon farther investigation that we have, through the sounds given out at the extremities, a guide in deciding upon the state of the capillaries, it must very materially aid our explanations of the diseases involving the vital powers generally. We very much need some additional data in this class of cases, and the congestive and nervous elements of disease will be shorn of their obscurity if we come to understand properly the modifications of the capillary circulation. If the slowness in the return of colour after pressure upon a part is found to correspond with a very feeble sound at the extremities, they will afford a very strong corroboration of the indications of the pulse and the sense of prostration, which will enable the practitioner to decide most indubitably upon the use of stimulants. On the other hand, if there is more than ordinary sound at the extremities, accompanied with speedy return of colour upon pressure, and we find a strong pulse with general excitement, depletion will be indicated. Without such concurrent evidence of the true condition, it is well known to every practitioner how liable the symptoms of excitement or depression are to be misconstrued, and tentative bloodletting or a tentative resort to stimulants are consequently in common use in lieu of the indications which we may now receive through the capillary circulation.

As my former article, suggesting a criterion for stimulants, was noticed in the review department of this journal, I would present this additional element in the same train of phenomena, and trust it may lead to farther investigation of the vital forces.

In illustration of the view, that the sounds which fall with-

in the province of dynamoscopy result from the passage of the blood through the capillaries, I have examined three cases of paralysis of the arm, of dissimilar origin, and very different in their character. One was the result of an apoplectic seizure, in which hemiplegia ensued, leaving the circulation in an almost normal condition, while motion and sensibility were extinct in the left superior extremity. In this case there was no muscular contraction of any kind whatever, and yet the sound was very distinct at the end of the finger of that hand. This would lead to the inference that the circulation was the source of the sound, and as it is known that the vitality of a limb is not materially lessened by paralysis of this kind, it is plain that a nervous function must be extended to the circulation even when motion and sensation are lost, and this belongs to the organic system of nerves which is more immediately related to the power of the organization respecting which this new inquiry is concerned.

The second case which has been under observation, is one of long standing paralysis connected with some rheumatic features in the subject. Partial hemiplegia in the left side, involving both the leg and arm, has been present for a number of years, and the left arm hangs useless at the side, though there is sensibility in the limb. In this case, there does not seem to be any deviation from the standard of healthy action in the circulation, and yet I suppose the long continuance of this inert condition, with contraction of the tendons of the fingers, would manifest less action of capillary action. My anticipation was to some extent realized, and yet the sound was entirely perceptible in the fingers of the affected limb, indicating that the flow of blood was sufficient to give this sign of vital action in the part. Had the sound depended on muscular action, we could not have expected it to be heard in such a case as this, when no evidence of the muscular contractility had been present for so long a period.

The third instance to which I wish to draw attention, is of so much interest within itself, that I will give a more detailed account of the circumstances of its origin and progress. It was a local paralysis of the left arm, without any general impairment of the physical organization, in which all power and sensibility were extinct, and in which at the outset there was no evidence of pulsation in the arterial trunk or branches which supply the circulation to the limb.

On the 20th of April, 1857, I was called to this subject, who was a negro man of robust frame, engaged on the railroad near Columbia. He had been crushed by a bank of earth,

which lay upon him, with his arm beneath his body, for a considerable time. He was excavated in an almost suffocated state, and brought to this city, when I discovered that no motion, sensibility, or pulsation was present in the limb; and indeed it was necessary during that night and the next day to resort to artificial means to promote the flow of blood from the veins, and thus invite the supply by the arteries as there appeared to be a degree of stagnation in the circulation incompatible with the life of the part. The cause of this suspension of pulsation must be involved in some degree of obscurity, but the solution of it may throw some light on the important subject of the arterial action, in the causation of the pulse, and hence it is proper to investigate every point connected with it. To make the case understood, it may be stated here that the absence of pulsation was a characteristic feature of the limb for several weeks. Why was no pulse to be detected while the blood evidently did find its way slowly through the arteries into the veins? Had the artery been compressed so as to agglutinate its sides, the blood could not have passed; or if a clot had formed in the trunk from the protracted pressure at the axilla, no flow of blood through the vessel would have followed. Or if we suppose that the channel of the vessel was reduced very much at the point of pressure, can it follow that the small column which is transmitted should not have the pulsation imparted to it? This view seems the most available for explanation of any, looking to the direct injury to the arterial trunk; and yet it is not favored by analogy of suspension of pulsation under partial occlusion of the trunk of an artery, nor was the diminution of flow so great as to induce the conclusion that any considerable obstruction existed.

The explanation which appears to me most plausible and satisfactory, refers the difficulty to the local injury of the nervous supply of the limb, from the long-continued impaction of the member under the weight of the mass of earth which covered the patient. The head of the humerus was turned forwards and inwards so as perhaps to be pressed out of the glenoid cavity of the scapula for the time, owing to the pressure, of which there was ample evidence, in the posterior aspect of the shoulder. The nerves were compressed between the head of the humerus and the clavicle or upper ribs, to an extreme degree, and hence undoubtedly resulted the paralysis of motion and sensation. Is it then unphilosophical to infer that the organic nerves which go to keep up the pulsation of the arterial circulation were affected in like manner, and

that the absence of the pulse followed such an impairment of nerve-power? I foresee the consequence of such a solution, in the recognition of an arterial action independent of that of the heart, and when facts lead to a correct induction, there is no more reliable conclusion. In this point of view, the case under consideration is perhaps without a precedent, and the particular features of it are calculated to elucidate this subject of arterial pulsation, as no other physiological or pathogical data are on record.

After one month of continued absence of pulse, the radial artery began to manifest some pulsation; and at the end of two months he was attacked with measles, in which excitement the arterial development of the affected arm seemed to participate, and the pulsation was more distinct than previously, and the eruption over the surface seemed to be as full as elsewhere. The sensation was somewhat restored along the inner portion of the arm as low as the elbow, but there was no sensibility to pinching or pricking or any other irritation along the forearm or hand, and there was no power of motion in any part of the limb at any time subsequent to the injury. The muscles were in a state of flaccidity, and the member had become diminished in size from want of use and deficient nutrition. Twelve months after the compression of the part, the circulation was very deficient in pulsation, and it was thus examined for dynamoscopic indications, when I found very little of that peculiar sound at the end of the fingers, which was distinct and full on the sound hand. Here, then, was a case in which the weak circulation with scarcely a well-defined pulsation, was characterized by a deficiency of that peculiar sound at the ends of the fingers, which is found to be well-marked in the healthy state of the organism, and I take it as very strikingly corroborating my view, that these sounds are caused by the propulsion of the blood through the capillary vessels in the extremities of the part examined. This limb was subsequently amputated as useless, and the arteries were found thinner and less firm than natural.

If we undertake a brief analysis of the three cases, it will be noted that all were equally deprived of the power of motion; and hence, if muscular contractility was a *sine qua non* of these sounds, they should not have been observed in any of the cases. Yet they were perceptible in each, but most defective in the instance of impaired circulation, and seem there to bear a direct relation to the tone of the circulation.

This renders the subject of dynamoscopy of considerable

importance, as affording a clue to the vital forces of the organism. And my own conviction is, that a close investigation of the bearings of this organic indication will conduct the medical inquirer to the most satisfactory information as to the power and capacities of the different subjects who require medical treatment.

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Physiology of the Liver. By Dr. MOSLER. (Journal de Physiologie and Virginia Medical Journal.)

The author has lately made experiments on dogs with artificial biliary fistulæ, to determine the influence of certain principles on the liver, when introduced into the blood. These substances were either mixed with the food, or injected into the veins of the animal's limbs. Those substances were chosen for experiment which are commonly used in medical practice, such as iodide of potassium, sulphate of quinine, nitrate of potass, etc. Moreover, he endeavored to ascertain if certain substances, such as calomel and turpentine, really possess the power over the liver which have been attributed to them. M. Mosler first ascertained that in the dog, the bile in its normal condition contains no albumen; and that the albumen which has been found in it is derived from the exudations given off by the edges of the biliary fistula. Then also he found, that the presence of albumen in the bile may be artificially induced by the injection of water into the veins. He found that the albumen appeared in the bile two hours after injecting water into the crural vein. That the quantity of albumen was greatest in the bile five hours after the injection, and in the urine three hours. That all trace of albumen disappeared from the bile eight hours after the injection; but that the urine still contained an appreciable quantity ten hours after, and that the urine always contained more than the bile. M. Mosler found this fact to agree with clinical observations made by him, viz: that in cases of Bright's disease, the bile (analysed after death) contained no albumen.

Sugar.—The author ascertained that in the dog the bile normally contains no sugar, even when the animal has been restricted to an exclusively vegetable diet, or has absorbed a considerable quantity of grape sugar. It was not until from seventy to eighty grammes of the sugar had been injected into the blood, that the sugar could be found in the bile.

Cane Sugar.—M. Bernard had already remarked that cane

sugar passes much more readily than grape sugar into the urine, and M. Mosler has found the same fact true as regards the passage of cane sugar into the bile. After the injection of five or ten grammes of cane sugar, he found it present in the urine, but not in the bile; but after further injections of twenty-five to forty grammes of the cane sugar, he found it present both in the bile and in the urine.

Iodide of Potassium.—A gramme of this given to a dog with his food, gave to the bile no trace of iodine. Two grammes, however, gave a clear but passing manifestation of its presence. Thus the law presiding over the passage of iodine into the bile, appears to be the same as in the case of the mammary secretion.

Nitrate of Potass.—Six grammes were given to a dog in solution; the urine contained a notable quantity, the bile none; ten grammes given solid in raw meat again yielded no trace in the bile. This negative result confirms the idea recently given by Buchheim, respecting the mode of action of nitrate of potass.

Sulphate of Copper.—During three consecutive days sixty centig. of this salt in pills were given to dogs. The urine and bile examined the second day contained no trace of it; on the third day, the copper was found in the urine and in the bile, and most abundantly in the bile. Here again Buchheim's views are confirmed. He found that the elimination of copper and other analogous metals (zinc, cadmium, etc.) took place rather through the bile than the urine.

Calomel was given to dogs in increasing doses; but the author was never able to ascertain the presence of mercury in the bile, nor even a notable increase of the biliary secretion. This fact seems to authorize us to refuse to calomel the special influence over the biliary secretion generally attributed to it; and weakens then the ideas which many medical men have in prescribing it.

Sulphate of Quinine.—One gramme was given to dogs in nineteen hours; one gramme 25 centig. in eighteen hours, and three grammes twenty-five centig. in eight hours. In each case the urine contained quinine; but in no case was it found in the bile.

Benzoic acid was given in doses of four, six, eight grammes; each dose in two days. Hippuric acid only was found in the urine; but nothing abnormal in the bile. From these interesting experiments it would follow:

1. That water injected into the blood causes albumen to be present in the urine and in the bile; but more quickly and in greater quantity in the urine than in the bile.

2. That certain substances, such as sugar of milk, and cane sugar, pass more readily from the blood by the urine than the bile.

3 That other substances—sulphate of copper, for instance—pass more readily by the bile than by the urine.

4. That nitrate of potass, sulphate quinine, and benzoic acid pass off both by the bile and the urine.

5. And that calomel does not pass off by the bile, nor increase its quantity.

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Regeneration of the Spinal Cord.—(Lancet.)—SIR: Numerous as are the recorded cases of nerves rapidly healing after division, and regenerating after a small excision of their substance, I am not acquainted with any case of regeneration of the spinal cord after complete division. The following may therefore be of interest:

In the course of numerous experiments on the batrachia, some of which consisted in dividing the spinal cord in various regions, to establish different independent centres of nerve-action, I found that a frog with the cord divided in the dorsal region, after exhibiting for some weeks the most complete independence in its anterior and posterior extremities, began at last to exhibit a certain *consensus*. Originally, when the posterior extremities were pinched or touched with acid, they were agitated, but the anterior remained motionless; and *vice versa*. Sometimes the anterior segment crawled, dragging the posterior segment after it like a log; at other times the posterior segment moved, and the anterior was quiet. No irritation of the one segment had any perceptible effect on the other segment; the two were as completely separated as if they had been two different animals.

About ten days ago, I was showing a friend this interesting example of two independent centres of nerve action in one animal, when, to my surprise, instead of the anterior extremities remaining perfectly quiescent, when the posterior was pinched, a decided agitation was visible in them, though of a slight character. This was so contrary to all that I had observed in the case of other animals, that at first I set it down to the wrong cause; but on reflection, it occurred to me that there had perhaps been a partial regeneration of the spinal cord. The next day I tested this, and found unequivocal symptoms of sensibility. Whenever the posterior extremities were pinched, the anterior were also

agitated. It was evident that the two centres had become one, as respects sensibility; but the slight traces of a consensus of voluntary motions were too equivocal to admit of a decisive affirmation.

To-day, I find all doubt disappear. The consensus of voluntary motions is so far re-established that the frog (which, by the way, has only one hind foot) in crawling, uses the posterior extremities in aid of the anterior, awkwardly and inefficiently, it is true, yet in a manner unmistakeably different from his former mode. It will even make one hop, when much irritated! This shows that to some considerable extent, there has been a re-union of the sensory and motor centres, formerly separated. There are no longer two utterly independent centres; whatever affects the one segment affects the other. A consensus has been re-established; and this would probably have been more perfect had the animal been left in perfect repose from the period of the original section of its cord; but having since then been subjected to a variety of experiments, a perfect healing of the two ends has perhaps been prevented. I am, Sir, yours truly,

G. H. Lewes.

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Nervous System, Report on.—Kussmaul and Tenner produced by compression and ligature of the arteries of the neck in healthy rabbits, the same epileptic convulsions which are seen in animals bleeding rapidly to death. In very debilitated or etherized animals neither loss of blood nor compression of the arteries caused the convulsions. The closure of both carotid and subclavian arteries was necessary in order to effect the convulsions; if circulation was allowed to continue in any one of the four vessels, convulsions never ensued, but only weakness and paralysis. The general convulsions usually commenced about eight to eighteen seconds after the interruption of the arterial circulation. For the description of the convulsions we must refer to the essay itself. Epileptiform they are, according to the authors, because the animals before the commencement of the general convulsions fall down, deprived of voluntary motion, with the appearance of loss of consciousness, never screaming before or during the attack; further, because the pupils are, during the attacks, dilated and motionless, and the eyeballs likewise fixed; that the attacks begin with contraction of the cervical muscles, that the respiration is suspended, although the heart's action is uninterrupted;

and that the atonic convulsions of the limbs end in tonic convulsions, are additional causes for the authors views regarding the nature of the attacks. As soon as the interruption of the circulation ceases, the convulsions leave off. The sudden return of the blood to the head never causes convulsions. By means of holes made in the cranium, the authors observed that the stoppage of the circulation was followed by capillary anæmia and venous oligæmia of the brain and its membranes; that the venous oligæmia became diminished simultaneously with the commencement of the convulsions, but that the paleness of the brain continued. As the essential cause of the convulsions, the disturbed nutrition of the brain is considered. The diminished mechanical pressure is regarded as of only secondary importance. The attacks took place even when a great part of the cerebrum, cerebellum, and medulla oblongata was laid open. Anæmia of the spinal marrow caused only paralysis of the limbs, the muscles of the trunk and respiration, never real convulsions; when the privation of blood was sudden and complete, slight trembling motions of the limbs preceded the paralysis. A series of experiments, made by taking off parts of the cerebrum, proved that the epileptiform convulsions did not depend on the cerebrum, but on the suddenly interrupted nutrition of the motor centres situated between the optic thalami. From these results, and the authors' experience on man, they infer that epileptic convulsions in man occur only when, besides the cerebrum, some of the parts, or all parts of the brain situated behind the optic thalami, become suddenly deprived of the sufficient amount of blood; that, however, the fall preceding the convulsions, the loss of consciousness and sensibility, depend on the affection of the cerebrum. Congestion of the brain, either active or passive, appears to them a very improbable cause of epileptiform convulsions. The sphagiasmus and trachelismus of M. Hall, the authors say, is not to be considered as a source of epileptic paroxysms, but the laryngismus is such a source. It seems further probable, from some experiments, that some forms of epilepsy are connected with a spasm of the contractile elements of the cerebral arteries. The medulla oblongata, therefore, as the origin of the contraction of the glottis and of the vaso-motor nerves, may be regarded as a frequent cause of epileptic paroxysms.

Stich observed that in cases of paralysis of the facial nerve, the corresponding margin of the tongue exhibits a diminution of taste, which phenomenon does, however, not extend to the root of the tongue. The diminution of taste does not take

place if the cause of paralysis lies on the base of the cranium; but if the cause lies at the origin of the chorda tympani, or has a more peripheric situation, the disturbance of taste occurs. Schiff remarked this phenomenon in a man whose facial nerve had been cut through close to its emergence from the stylo-mastoid foramen. It appears, therefore, that this disturbance is attributable not to the facial nerves, but to the fibres derived from the fifth pair.

Spiegelberg concludes, from numerous experiments on rabbits, cats, and guinea-pigs, regarding the movements of the uterus. 1. That the stoppage of the circulation is the cause of the peristaltic movements of the uterus. The uterus resembles in this respect the intestinal tract. Compression of the aorta immediately below the diaphragm causes always movements of the tubes of the uterus and intestinal tract, which continue as long as the compression lasts, and cease as soon as the interruption of the circulation is suspended. 2. The pneumogastric nerves exercise no immediate influence on the uterus: but if they stop the action of the heart, this circumstance acts like the compression of the aorta. 3. Irritation of the medulla oblongata excites contractions of the uterus, which phenomenon is not influenced by section of the pneumogastric nerves. 4. The cerebellum is the principal centre for the movements in question. Irritation of any part of it excited the action of the uterus, that of the posterior half of the corpora quadrigemina was likewise often successful, that of the cerebrum never. 5. The movements may be excited from every portion of the spinal marrow, but especially from the lumbar and sacral portion. The movement of the pregnant uterus may be stopped by means of irritation of the spinal marrow; this, however, the author attributes to over-excitation. 6. The sympathetic and sacral nerves form the means of communication between the nervous centres and the uterus. 7. The impulse to the parturient action of the uterus is not given by the nervous system, but probably by local changes of the circulation in the uterus, occurring at the termination of pregnancy.

The same author confirms the dependence of the intestinal movements on the spinal marrow and brain. From all places which excite the movement of the uterus, that of the intestines may be likewise excited. Interruption of the circulation effects likewise very active movement of the intestinal canal; compression of the aorta does it to a much greater degree than compression of the vena cava or vena portæ. This would be opposed to Brown-Séquard's view, that the car-

bonic acid in the blood is the cause of the peristaltic movements. It would also appear incorrect to ascribe the peristaltic motion of the intestines exposed to the air immediately after the death of the animal merely to the excitation by the air.

Pflüger's assertion, that irritation of the splanchnic nerves stops the peristaltic movement of the intestinal canal, has been repeatedly contradicted. Kupffer and Ludwig found that irritation of the nerves in question may excite the action of the intestine, or stop it, according to the condition of the latter. We may add to this, that there are now various observations extant that the same nerve may cause movement and may arrest movement. Schiff's view, that the arresting of movement is not a peculiar action of the nerve, but the product of over-irritation—i. e., exhaustion, appears to deserve the attention of experimenters. We need only mention that Erkhard observed acceleration of the heart's contractions by *moderate* irritation of the pneumogastric nerve.

Budge found, in his experiments on various animals, contrary to the view expressed by Romberg,* that the ganglion semilunare and the ganglion mesaraicum are in a considerable degree sensitive; at all events as sensitive as the pneumogastric nerve, the splanchnic nerve, the ramus communicans, and the cervical ganglion of the great sympathetic; but much less so than the fifth pair or the posterior roots of the spinal nerves.

Czermak confirms Ludwig's observation, that irritation of the sympathetic branch of the submaxillary gland, or of the cervical portion of the sympathetic nerve, may excite the secretion of saliva just as much as irritation of the glandular branch of the lingualis. The author remarks, however, in addition, that electrical irritation of the cervical part of the sympathetic nerve, may diminish, and even suspend the secretion of saliva. Sometimes the irritation of the glandular branch of the linguails does not influence the secretion of saliva, when the sympathetic nerve had been previously irritated. If both nerves are stimulated simultaneously, the secretion rapidly reaches its maximum, but after fifteen or twenty minutes it becomes diminished, and frequently ceases altogether. We have therefore another proof of the arrest-

* Lehrbuch der Nervenkrankheiten, third edition, vol. 1, p. 151.

ing action of the nervous system, if we do not prefer to explain the phenomenon just described as the product of over-irritation.

The experiments of Harless, made on the nerves of frogs, prove that the sheaths of the nerves are of great importance in regulating the irritability of the nerves. The author considers himself entitled to infer that the irritability and the conducting power of the nerve are in close connexion with the proportion of water contained in the nerve—viz., that they become much increased by any loss of water sustained by the nerve. He further maintains, that the degree of irritability and conducting power varies with the degree of pressure exercised by the sheath; that this pressure becomes increased by the process of exudation of the sheath; that, therefore, the irritability and conducting power of the nerve depend, to a great degree, on the percentage of water contained in the sheath.—*Brit. & For. Med.-Chirur. Review.*

MATERIA MEDICA AND THERAPEUTICS.

On the Therapeutical Action of Per-Chloride of Iron in the Treatment of Acute and Chronic Urethretis. By M. BARUDEL. (Bulletin Général de Thérapeutique, May 15, 1858.

Dr. Barudel, in his investigations upon the therapeutical employment of the per-chloride of iron, found that this agent exercised a special influence on the genito-urinary mucous membranes, for, from the period when it was administered internally, the inflamed membranes were rapidly affected, and no longer secreted the abundant puriform fluids which accompany the inflammation of the genito-urinary tract. The per-chloride appears to owe its efficacy to a double action, for it possesses a hæmostatic power, which retains the blood in the capillary vessels, and opposes its escape, and also a sedative power, which rapidly restores the functions of the capillary circulation to a regular mode of action. When the per-chloride of iron was administered internally, Dr. Barudel found that it lowered the pulse from seventy to sixty, and even to fifty, in the minute, and he therefore was induced to employ it in many cases where it was essential to reduce vascular excitement. The duration of urethritis was certainly shortened by this medicine, and cases which ordinarily required a period of thirty or forty days for their cure were successfully

chronic blennorrhagia, and was also employed in the form of a styptic injection to the affected membrane. The employment of this treatment has never been attended, in the hands of Dr. Barudel, with any unfavorable results, so as to induce him to discontinue it, although, as is well known, a multitude of serious complications often spring up in the course of blennorrhagia.

In all the cases treated by Dr. Barudel, the treatment was identical, except in some rare instances. In acute urethritis, an injection of iodide of lead was employed, this salt being almost insoluble and exciting no pain in the inflamed mucous membrane, the solution of perchloride of iron in injections was reserved for chronic blennorrhagia, the constriction which it exercises upon the urethral canal rendering it more applicable to the chronic than the acute affections. The internal treatment of all the cases consisted exclusively in the administration of a mixture containing twenty drops of tincture of perchloride of iron, taken every two hours. In general, at the end of three days a certain improvement was effected; but no aggravation of the inflammatory condition of the parts was ever produced by this internal and external treatment, either in the acute or chronic cases. The cure was almost always completed in fifteen days. In chronic urethritis, the only modification consisted in the use of an injection made of tincture of perchloride of iron and distilled water; this injection was repeated three times a day, taking care that the fluid should remain at least ten minutes in the urethra. The general treatment was of a strengthening nature, wine being allowed to the patients. Dr. Barudel quotes two cases, successfully treated in the military hospital at Lyons by the means just described, and he draws the following conclusions from his researches: 1. That perchloride of iron may be used successfully, both externally and internally, in the treatment of acute and chronic blennorrhagia; 2. That this agent is endowed with well-ascertained hæmostatic properties, and possesses also a sedative action on the general circulation; 3. That several venereal affections, which resist ordinary remedies, may be safely treated by the perchloride of iron; 4. That the perchloride, given both in acute and chronic cases, appears to act, in the great majority, as a specific; 5. That the perchloride of iron is sufficient in the treatment of simple urethritis; but that when the syphilitic element is associated with this disease, mercurials must likewise be employed; and, 6. That the most important auxiliary to terminated in eight or twelve under the use of the perchloride, which was administered internally in cases of acute and

the internal use of iron is the employment of an injection, the iodide of lead being applicable to acute, and the perchloride of iron to chronic, urethritis.—*Brit. and Foreign.*

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On the Uses of Sesquichloride of Iron in Medicine. By Dr. PLEISCHL. (Zeitschrift der Gesellschaft der Aertze zu Wien, July 10, 1858.)

Dr. Pleischl points out the use of the sesquichloride of iron in some cases which it has not hitherto been generally prescribed. There are two preparations of this salt in the Austrian Pharmacopœia—namely, the crystalline sesquichloride, and the solution called oleum martis. The therapeutical operation of these preparations is astringent and styptic, and they are therefore applicable in cases of hæmorrhage and profuse secretions in the different passages, especially in the intestinal canal. Dr. Pleischl relates several cases in support of the efficacy of sesquichloride of iron in these affections.

In a case of Hæmoptysis in a young man, the attacks had been treated by alum, acetate of lead, and ice, without effect; but the hæmorrhage was speedily arrested by the administration of a mixture containing sesquichloride of iron. A second case was one of Hæmatemesis, in which the patient had lost from ten to twelve pounds of blood in three days. In this instance acetate of lead, laudanum, alum, and the internal and external use of ice, had been tried in vain, but on employing the mixture of sesquichloride of iron the bleeding ceased, and the patient recovered. The cause of the bleeding in this case was subsequently ascertained to be a granular liver. A third case was one of chronic diarrhœa in a young girl, to whom calumba, opium, and acetate of lead, had been administered without success; but after the employment of sesquichloride of iron the evacuations soon became more consistent and in a few days the patient was completely restored to health. Another case was one of hæmorrhage from the intestinal canal, occurring after an attack of fever. There were fæcal accumulations in the lower bowel, and when these were removed with difficulty, a profuse hæmorrhage followed, and threatened to carry off the patient. The sesquichloride of iron was employed both internally and in the form of injection into the rectum, and under this treatment the bleeding ceased, and a cure resulted. As an injection, the sesquichloride has also been employed with much success in cases

of uterine hæmorrhage from cancer and fibrous tumours of the uterus, and in one case of leucorrhœa, after solutions of alum and sulphate of zinc had been injected without effect, the employment of sesquichloride of iron as an injection was attended with a notable diminution of the secretion.

The sesquichloride of iron has the property of being easily absorbed and assimilated in the system, and its use is not attended, as often happens in the case of lead, with symptoms of poisoning; and moreover, in the sequelæ of hæmorrhages, the use of iron is attended with the best results in the removal of anæmia.—*Brit. and Foreign.*

On the Employment of Oil of Turpentine and Opium in Large Doses in the Treatment of Severe Puerperal Diseases. By Dr. E. Bonfils. (Bulletin Général de Thérapeutique, May 30, 1858.)

M. Trousseau has lately employed, with considerable success, a method of treatment, proposed originally by Dr. Graves, in puerperal diseases. This treatment consists in giving opium and oil of turpentine in large doses to women in child-bed who are attacked with metro-ovaritis, peritonitis, uterine phlebitis, &c. Among other cases, M. Trousseau has treated in this manner, and with success, a woman attacked with peritonitis and double pleuropneumonia. He also employed this plan in another case of a woman attacked with general and very severe peritonis, which was very rapidly checked and afterwards cured; but although the cure appeared to be permanent, the patient was unfortunately seized with hectic symptoms of an insidious character, and sunk under what appeared to be a putrid infection. In the first case the opium was prescribed in pills and the turpentine in injections. At first five centigrammes (about one grain) of opium were given in five pills, to be taken daily; then the dose was gradually augmented till it reached about two grains a-day. The opium was continued for thirteen days. The turpentine was administered at first in the dose of ten grammes (about two drachms and a half) in two glysters, one in the morning and the other in the evening; then the quantity was progressively augmented to thirty grammes, (about seven drachms and a half.) In the second case the opium was also given in pills, in the dose of five centigrammes (about one grain) for three days. The oil of turpentine was administered

by the mouth in capsules, each containing one gramme (about the fourth of a drachm) of turpentine; six of these capsules were taken every day, and they were continued for six days. —*Brit. and Foreign.*

Practical Medicine. (Extracted from Braithwaite's Retrospect.)

Therapeutic Uses of Arsenic in Cholera.—Dr. Black believes arsenic to have a specific action in cholera, from an experience of nearly two hundred cases, in none of which has it ever failed to produce a speedy and permanent cure. It may in very severe cases, where the cramps come on every few minutes, and the vomiting and purging are all but incessant, be given in doses of five drops of the liquor arsenicalis every 15 minutes until the symptoms abate, and then be given every hour. (p. 248).

Dr. Begbie says that the lameness and deformities of chronic rheumatism frequently disappear under a prolonged use of arsenic. It is generally given up too soon. Two cases of this nature are related, in which the most marked benefit was derived from this mode of treatment. About five drops of the liquor arsenicalis should be given after each meal, and continued till the characteristic effects of this drug are produced, when it may be intermitted for a time. Many and obstinate forms of neuralgia are in a similar manner found to yield to arsenic. Arsenic holds the "foremost place" amongst the remedies employed for the cure of chorea, and the author has never yet seen it fail. Besides the various forms of skin disease, in which its use is so well known, it has been used very extensively and successfully by Dr. Simpson in amenorrhœa and other disorders of the uterus, where iron appeared to be contra-indicated, as well as in that peculiar affection of the bowels which he has described as prevalent among females, and characterized by copious discharge of membranous shreds, and accompanied by great emaciation and a long train of neuralgic and other nervous symptoms. Dr. Simpson places most reliance upon small and very long-continued doses of arsenic, as two drops of Fowler's solution, or a pill containing the sixtieth of a grain of arsenite potass, taken three or four times a day.

Treatment of Boils and Carbuncles—The manner of operating by incision is neither requisite nor safe, and is much sur-

passed in efficacy by the use of potassa fusa for this purpose. It should be fresh and unaltered by exposure to the air, and may be inserted into a goose-quill by way of handle. The best time for opening, is when the skin has become dusky, and is perforated by pin-hole orifices, whence issues an ichorous oozing. To prevent the caustic running and destroying the healthy skin, a dossil of dry, flocky lint should be applied upon and around the part to which the caustic has been applied. The carrot poultice is the best that can be applied afterwards, with a view of cleaning the sore, and a solution of chlorinated soda is very useful at particular stages of the sore. A weak caustic, like nitrate of silver, is of no use, but even prejudicial in these cases. (Mr. B. Travers, jun.)

Chronic Eczema in Children—First get the scabs or crusts separated, by means of poultices, if the spots are limited, and are not settled on the head, face and neck, where these applications are not suitable. Before applying them, it is a good plan to moisten the surface with a solution of carb. of soda (ʒ ii. to ℥ viii). When the surface is large, a water dressing is to be preferred; a little sub-carbonate of potash being added to the water. If situated on the head. face or neck, paint the scabs over with a mixture of carb. of soda and cod-liver oil, removing the crusts carefully next morning, and moistening the surface with the alkaline lotion. This must be repeated as often as necessary, till crusts cease to be formed, and a red, inflamed, but painless surface is left. Next, we must remove this condition of the skin. One of the best means to this end, is the application of a solution of ʒ i. of acetate of zinc, and the same acetate of lead, in ℥ viii. of distilled water, adding to this, at the time of using it, an equal quantity of strong chamomile infusion. The last indication is to restore the activity and healthy tone of the skin, by such hygienic measures as fresh country air, free exercise, a well-regulated and wholesome diet, and such local applications as weak solutions of alum and sulphate of zinc. (Dr. Behrend).

Treatment of Purulent Ophthalmia.—During the acute stage, you may: 1. Deplete locally by leeches. 2. Scarify the inner surface of the lids. 3. Divide the external canthus, or scarify the ocular conjunctiva in radii, if chemosis is high. 4. Apply nitrate of silver drops, the strength varying according to the severity of the symptoms. 5. Foment constantly with decoction of poppies, containing a little alum. 6. After the bowels have been well opened, for which purpose croton oil is the best, as being the most speedy purgative, exhibit mercury, which must be used guardedly if ulceration threaten

the cornea. 7. Give quinine when debility prevails. 8. Moderately nutritious diet should be given, and progressively improved. In the convalescent or chronic stage, tonics with varied and mild local astringents and slight counter-irritation must be used to complete the cure. The above is the treatment adopted at Guy's Hospital. (Mr. J. F. France.)

Treatment of Cynanche Maligna.—Give powdered guaiacum in combination with chlorate of potash. Support the strength with beef-tea, mutton broth, etc. Cause the patient to frequently gargle the throat with a solution of chlorinated soda; or, if he be too young, or unable to use the gargle well, cause the throat to be mopped or sponged with it. The secretions of course must be attended to; and in the latter stages of the disease, tonics, such as quina, or bark, and nitric acid, will be beneficial. (Dr. W. A. Bryden).

Treatment of Dysentery.—In sub-acute and chronic dysentery, confine the patient to bed, and in the recumbent position, that the bowels may be supported and kept quiet, and unirritated by the action of the abdominal muscles. Milk and farinaceous food are most applicable, as least stimulating peristaltic action, and being most likely to be assimilated. In the subacute and severer dysentery of this country, no remedy is equal to Dover's powder in ten grain doses, with an occasional dose of castor oil, guarded by a few drops of laudanum, for the removal of fæcal matter. If there be a deficiency of bile, give a little hydr. c. creta; if there be much pain or tenderness, apply a few leeches to the anus. In acute cases confined to the rectum, medicines administered by the mouth do little or no good, here give soothing injections with opium.

In the purely chronic form of the disease, the treatment consists in the administration of mineral and vegetable astringents combined with opium, occasionally using aperients for the removal of fæces and foul secretions. When the ulceration is confined to the rectum or lower extremity of the colon, astringent injections of acetate of lead, nitrate of silver, sulphate of zinc, and gallic acid, will be most serviceable. But when the ulceration extends more or less throughout the entire extent of the colon, remedies administered by the mouth, especially the mineral astringents, act most effectually. In these cases, slips of blistering plaster should be frequently applied. In the more advanced stages of the disease, and in the milder cases where we have a healing but lax state of membrane, the stools being bilious and containing but little blood or mucus, the vegetable astringents will answer best. A strong decoc-

tion of the Ægle Marmelos, or Bael fruit of Bengal, is very useful. A few drops of laudanum may be added to each dose of this. (Dr. S. H. Ward).

Relative value of the different Anthelmintics in the Treatment of Tœnia.—Dr. Peacock states, that as a general result of his experience, both in public and in private practice, he prefers the oil of male fern to all other remedies, and that he holds the kousso in very light estimation indeed. It appears that of the hospital cases respecting which notes have been preserved, the fern oil was given in thirty-five. Of these, in sixteen no other remedy had been previously tried, and in this group the result was always satisfactory, the animal being expelled in a dead or dying state. In seven cases the oil was given after the partially successful use of kousso, and in all these more of the worm was brought away. In three, after partial success by pomegranate bark, the oil brought away other portions of the parasite, and in one a like result was obtained after the use of the turpentine draught. In six cases in which the oil was used, either the result was not satisfactory, or the patient did not attend again. The dose of the oil given was from half a drachm to a drachm and a half to children, and from a drachm to three drachms to adults.*

The cases in which kameela was given are seven. In five of these no other remedy had been previously tried, and in all these, portions of worm (generally quite alive) were expelled. In one, the expulsion of worm was caused after kousso had been tried without effect, and in the fifth, which was under similar circumstances, a like negative result followed its use also. In two cases, after the successful employment of the kameela, the oil of fern was employed without procuring the expulsion of any more of the worm. The dose of kameela prescribed was from half a drachm to a drachm for children, and from one to three drachms to adults.

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* We are informed that great care is necessary on the part of the dispenser, in order to avoid disappointment in the use of the oil of fern. Its etherial solution, which is by far its best preparation, on standing, deposits its resinous principle. A prolonged shaking is necessary to secure readmixture. Unless the dispenser pay more than usual attention to this matter, the patient is very likely to get a dose which is but little more than ether.

It would from the above facts appear, that kameela is more efficient than kousso, but that it must rank as a vermifuge rather than a true vermicide. After the fern oil the animal is usually voided dead. An important statement with regard to the comparative value of kameela, is made by Mr. Henry Callaway, formerly of Finsbury-circus, but now a medical missionary among the Zulus. The kameela is the native remedy among the Aborigines; but in a letter to the *Pharmaceutical Journal*, Mr. Callaway states, that from experience they have learned already to put much more confidence in "the white man's dose." The latter consisted in turpentine and castor oil, the time honored remedy among ourselves. We are not able from Dr. Peacock's cases, to institute any comparison between turpentine and the fern oil, and can only state that we believe he is supported by several other hospital physicians who have given much attention to this matter, in maintaining that the latter ought to stand *facile princeps* among our anthelmintic drug.

As regards the economics of the question, which are important in hospital and Union practice, it will, of course, be easily granted that all things considered, the most efficient remedy will probably in the end prove the cheapest. A dose of castor oil and turpentine, undoubtedly, costs far less than any of the others. Next to it comes the kousso, which has as rapidly fallen in price as it has in general estimation. The kameela is, as yet, rather expensive, though not nearly so much so as the fern oil. A full dose of the last costs eightpence, of the kameela about four-pence, of the kousso threepence, and of the turpentine and castor oil not more than three half-pence.

Kuchenmeister, in his *Manual on Parasites* (Sydenham Society's edition), writes of the oil of turpentine, as follows: "As has already been remarked, the touchstone of a remedy for tapeworm is not whether it expels *bothriocephalus latus* or *tœnia solium*, but whether it is also capable of effecting this with *t. medio-canellata*. That oil of turpentine is efficacious in the latter case, I can prove at any time; for the finest specimen of *tœnia med.* that I ever saw was expelled by it. In general, also, it acts pretty rapidly. Lastly, it has also the advantage, that it expels the worm entire." Of the kousso he writes, "For my part, I have always been more or less unlucky with this remedy. . . I have generally seen the worm expelled in innumerable fragments. . . . . I have never found the head. In one case I detected fragments in the evacuations for three months." Professor Mar-

tius, of Erlangen, who has also used kousso largely, never saw the head brought away. Of the male fern, Kuckenmeister states, "This remedy, which will always maintain its renown against the *bothriocephali*, appears hardly to maintain its reputation with regard to tænia." The kameela he had of course not tried.

Of the desirability of having the intestinal canal as empty as may be before giving anthelmintics, most practitioners are aware. To administer them fasting in the morning is usually thought sufficient, but in cases where difficulty has been encountered in destroying the animal it may be well, as an introductory measure, to give it a sharp purgative.—*Med. Times and Gaz.*, Nov. 6th, 1858.

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On the Value of Iodide of Iron.—The iodide of iron is comparatively a new preparation, as it was only in 1834 that Dupasquier, of Lyons, distinguished no less as a chemist than as a physician, made some very interesting trials of this preparation in the treatment of pulmonary phthisis, and proposed a new method of preparing it. He then showed that he had performed several cures upon patients affected with crude pulmonary tubercles, and had considerably relieved others whose tubercles had begun to soften. In the hands of others, however, the use of iodide of iron has not been attended with uniformly good results, probably in consequnce of the uncertain nature of the preparation; sometimes too much of the iron was received into the stomach, and besides this, there was occasionally an excess of iodine present, which was converted in the system into hydriodic acid. In recent times, M. Gille has proposed to administer the iodine in the form of sugar-plums, and in the formula recommended, it is said that the proportions of the iron and iodine are preserved unaltered. M. Boinet records two cases of abscess of a very aggravated character cured by the internal use of iodide of iron, together with ioduretted injections into the sac of the abscess. The first case was that of a child, nine years old, in whom there were caries of the fourth, fifth, and sixth dorsal vetebræ; and there were two abscesses, one in the neck, on a level with the diseased vetebræ, and the other, also on the right side, in the iliac region. The disease had continued for several months without any amelioration, when, on the 8th of January, 1857, the abscess in the iliac fossa was punctured and injected with tincture of iodine; the patient was put upon good diet, and

cod-liver oil was given internally, together with sugar-plums (*dragèes*) of the iodide of iron. On the 17th of January both the abcesses were punctured and injected at the same time. This operation was subsequently repeated on several occasions, and under this treatment the fistulous openings caused by the punctures gradually dried up, and at last became completely cicatrized. The child improved in all respects, and under the use of the iodide of iron in sugar-plums, and the influences of strengthening food, became strong and well. The second case was that of a man aged sixty-three, under the care of M. Malgaigne, at the Hopital St. Louis, who had a large abscess, which discharged more than three pints of a grumous pus. The opening was then injected with tincture of iodine; the abscess remained fistulous for some time, and allowed a small quantity of pus to flow out every day, but it soon closed. The iodide of iron was administered internally, and the patient was put upon a strengthening diet; and six weeks after the operation and the internal use of the iodide, the patient left the hospital quite well.

In a lecture on chlorosis, by M. Gendrin, the lecturer recommends iron and magnese as the best remedies in this disease, and passes in review the different preparations of iron which are employed in medicine. One of the most useful preparations in cases of chlorosis complicated with scrofula, or only with a lymphatic temperament, is the iodide of iron. But, unfortunately, this salt is very easily decomposed, and in order to employ the syrup with advantage, it should be prepared at the very time when it is to be taken. The invention of M. Gille fulfils the object of practitioners in recommending this medicine, for he envelopes the iodide with a layer of sugar, which altogether prevents the access of air. These sugar-plums have been preserved more than two years without any alteration of the iodide. M. Gendrin speaks strongly in favor of this preparation, the value of which consists not only in the indefinite preservation of the ferruginous salt, but also because it renders its administration easy and agreeable. The iodide of iron has the advantage over other preparations, of being well borne by the patient, an advantage which it undoubtedly owes to its great solubility.

M. Rostan, in a lecture on chlorosis, says that the iodide of iron possesses, in the promptitude of its curative action, a very manifest advantage over the other ferruginous compounds; the syrup of the iodide has not been used in medicine so much as it would have been if it had more stability;

but the preparation of M. Gille renders the administration of this salt as easy as that of the oxide or the carbonate.

In phthisis, the iodide of iron has been found to act very beneficially, and its importance in this disease will be increased when it is preserved chemically pure, and given in suitable doses. Louis, Andral, and Bricheteau, have all used with success, the syrup of the iodide of iron in pulmonary tubercle; and more lately Dr. Belouino has published some cures of this disease effected by the administration of the sugar-plums of M. Gille. "Recent observations," says Dr. Belouino, "have assigned to iodide of iron an important place among therapeutic agents, and it may be boldly placed among the best medicines which we possess. Formerly it was unworthy of confidence, because it was badly preserved and was easily decomposed, and consequently did not give always identical results. Physicians, in consequence, decline to make use of it. At present the iodide of iron—thanks to the laborious researches of *M.* Gille—is preserved in a state of perfect purity. I have had occasion to employ very often the preparations of this gentleman, and I have attained experimentally the conviction that the iodide of iron is an excellent medicine in cases of anæmia, scrofula, rachitis, chlorosis, and often in certain cases of pulmonary phthisis in which the organism requires to be strongly fortified." Dr. Belouino records two cases of phthisis which were cured by the administration of iodide of iron; one was the case of a lady, aged twenty-four; the other that of a child, aged five; in both, the existence of tubercular disease was ascertained, but it disappeared under the use of the idide.—*Journal des Cliniques des Hôpitaux de Paris*, and *British and Foreign Med. Chir. Review.*

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Tartar Emetic in Large Doses in the Treatment of Chorea By M. GILLETTE. In the session of March 6th, 1858, of the "Société Médicale d'Emulation," M. Gallard mentioned a thesis of M. Bonfils on the use of tartar emetic in large doses in the treatment of several cases of chorea. As the facts reported in the essay of M. Bonfils had been observed in the service of M. Gillette, the latter was requested to explain to the Society his mode of treatment. It is the following:

For the first day he prescribes ten centigrammes of tartar emetic, which the patient takes from hour to hour. In the course of the day vomiting generally supervenes; if it be-

comes too frequent, the medicine is taken less frequently, or is entirely suspended. On the second day he prescribes twenty-five centigrammes; this produces, perhaps, some vomiting still. On the third day, thirty centigrammes; this is generally *not* attended by vomiting or purging. After this period, three or four days rest. There is already an improvement, and a perceptible amelioration of the disease. In exceptional cases, the cure is obtained thus early. The patient is subjected to the same treatment for another period of three days, during which M. Gillette prescribes progressively, twenty-five, fifty, and seventy-five centigrammes of tartar emetic. This period is also followed by three or four days of repose, after which the dose of the medicine is pushed to thirty, sixty, and one hundred grains. The improvewent is such, that no disorderly movements are any more perceived. The case may then be confirmed by ordinary means, particularly gymnastics and sulphur-baths; but this is a precaution dictated more by custom than necessity.

Since the thesis of M. Bonfils, several new facts of cure have been observed. The use of tartar emetic in the treatment of chorea has afforded to M. Gillette actually thirty-seven cures and only one failure in thirty-eight patients submitted to his observation. The author's attention in the administration of the medicine is particularly directed toward establishing, as soon as possible, a tolerance of the remedy; perturbation of the system, by producing the violent physiological effects of the emetic, should be avoided. The chorea disappeared progressively, and the sooner, the more intense the affection had been.

M. Brienne de Boismont reports a case cured in five days by the method of M. Gillette.

There was nothing said of the local action of tartar emetic on the mucous membrane, and it is well known that the prolonged influence of the medicine has some inconveniences.—*Union Médicale*, June 12th, 1856, and *North Amer. Med. Chirur. Review.*

EDITORIAL AND MISCELLANEOUS.

OBITUARY NOTICE.

DEATH OF PROF. P. C. GAILLARD.

DIED, in the City of Charleston, S. C., on the 14th day of January, 1859, after a lingering illness, from consumption, Dr. PETER CORDES GAILLARD, in the 44th year of his age.

The above simple notice records the premature removal by a mysterious providence, of one of the most estimable and valued members of our professional brotherhood; it will recall, we feel assured, to all who were acquainted with its subject, the memory of a man whose lofty rectitude of purpose and action, whose pure and blameless, and well-spent life, won golden opinions from all who came within the sphere of his influence and example, and whose death affords another melancholy proof that honor and worth are but blossoms of the shortest lived plants which earth bears.

Dr. Gaillard was a native of this State, to whose soil, indeed, he was attached by strong hereditary ties; here his youth and ripening manhood were passed, and to her interests and advancement he dedicated the matured fruit of the culture received in her noble collegiate institution, where he laid the foundation of those scholarly tastes and habits which afterwards so strongly marked his intellectual life. Devoting himself, upon graduation, with his characteristic energy and concentration, to the study of medicine, he received his diploma from the same college with whose professorial corps he was afterwards to become united, and subsequently passed some time in Europe, with the view of enlarging his experience and perfecting his knowledge, before assuming the re-

sponsibilities of active professional life. Soon after his return from Paris, he became joint editor—with Dr. DeSaussure—of this Journal, then entering upon the second year of a very precarious existence. To his labor and talents, its progressive influence, popularity and value were largely indebted, and its review and editorial departments are universally acknowledged to have been marked by a scholarship and ability, never surpassed under the regime of any one of his many successors.

Laborious, earnest, indefatigable in the search for truth, his success was from the first assured and predicted; every year added to his justly-earned reputation as a medical scholar of fine accuracy, extensive learning and sound judgment, and when, in the Spring of 1858, the resignation of Prof. Dickson left vacant the chair of Practice of Medicine in the Medical College of South Carolina, he was at once selected by the Faculty and Trustees of that institution as one well qualified not only to fill, but adorn a position, which his predecessor's fame as a lecturer had made scarcely an enviable one, certainly no easy sinecure.

Alas! even in the moment of triumph, just as the hard-won laurel pressed his brow, the relentless hand—destined soon to pluck it—was stretched forth. His health, never strong, had been gradually sapped by the ceaseless toil and care of his daily occupations, but though he felt that the severe labors of this new office would materially shorten the brief term of life which remained, he accepted the forfeit with a noble manliness, fulfilled with a scrupulous exactitude, and under the most painful struggles, every requirement, the most arduous or trifling, and perished in the faithful discharge of his sacrificial duties.

We do his character and memory the justice of subjoining to this imperfect elegiac by one whose association with Dr. Gaillard was comparatively short, and never intimate, the truthful and feeling tribute of his former editorial colleague and life-long friend, but we cannot forego the melancholy gratification of casting a stone upon the cairn which the love and admiration and reverence of friendship has already

heaped upon his newly-made grave. Never more worthily can such a tribute be offered, for the assemblage of virtues which made up his character are but rarely fused into so perfect unity. Confident without being dogmatic; firm, yet never obstinate; patient, though not submissive, and as brave as gentle, he tempered the hard necessities and smoothed the rude contacts of jostling life, by an amenity of manner, an inborn grace and delicacy of feeling, and a nice sensitiveness of honor which charmed and softened the roughest of his associates, and disarmed even the most hostile of his singularly few opponents. Upright and inflexibly honest, yet never bigoted or prudish; sternly virtuous yet always charitable; he added to the dignity and surpassing excellence of an unblemished and sterling manhood, all the gentleness and kindly graces which belong to woman, and though he moves no longer among us as friend, preceptor, counsellor or guide, he has left the profession—a princely legacy—the rare example of one who, with noble aims, by honest means, achieved high honors, won large applause, and was elevated by his peers to offices of trust and power and profit, and amid all the petty jealousies and mean juggling, and baser craft of rivalry, "kept the whiteness of his soul," dared to be upright in scorn of consequence, and lived and died honored, beloved, respected by all who knew him;

> "And thus he bore without abuse
> The grand old name of gentleman,
> Defamed by every charlatan,
> And soiled by all ignoble use."

At a meeting of the Medical Society of South Carolina, held on the 1st inst., the following preamble and resolutions were unanimously adopted and ordered for publication:

Since our last meeting we have been called to lament the loss of our friend and brother, Dr. PETER C. GAILLARD. Death must conquer all, for the end of all things is death; but how solemn is the warning when his stroke is laid upon one in the prime of manhood and the vigor of intellect. Dr. Gaillard had just attained the age of man's greatest physical and in-

tellectual development, when removed from the sphere of his labors and his usefulness. Prepared for the study of medicine by a good education and a disciplined mind, he devoted the earlier years of his professional life to an extensive and minute investigation of the phenomena of disease, and careful deduction of his medical principles, thus acquiring that clear perception and logical accuracy which characterized his subsequent career. He was earnest in the pursuit of knowledge, active and zealous in promoting all that was useful or noble in the profession. In his intercourse with his professional brethren, he was courteous and gentle, with a strong sense of justice; he never violated the rights of others, nor permitted his own to be infringed. With a high standard of right, and the moral courage to sustain his opinions under all circumstances, he was yet enabled by his rectitude of purpose and his courtesy of demeanor, to conciliate even those whom he offended. A life of labor was about to be crowned with its well-merited rewards, when the hand of disease was laid heavily upon him; he struggled with all the earnest energy which had characterized his life, against the enfeebling effects of long-continued disease, and died at last almost in the very performance of his professional duties, exhibiting a noble example of the control exercised by firm, determined purpose, over a feeble and suffering body. With a character so constituted, a mind so trained, his death is a loss to ourselves, our Society, our profession. Therefore,

Resolved, That in the death of Dr. Gaillard, this Society has lost an esteemed and useful member, our profession an earnest and zealous associate, who would, had his life been spared, have contributed by his knowledge to advance the usefulness of his profession, and by his high moral character, to elevate the standard of professional dignity.

Resolved, That a copy of these Resolutions be sent the family of the deceased, and be published in the daily papers of the city.

MEDICAL ASSOCIATION OF SOUTH CAROLINA.

This body commenced its eleventh annual session in this city, at the Roper Hospital, Wednesday morning, the 2d inst. The following members were present:

R. W. GIBBES, President.
W. C. RAVENEL, Recording Secretary.
H. W. DESAUSSURE, Jr., Treasurer.
F. M. ROBERTSON, Corresponding Secretary.
Sumter.—J. A. Mayes.
Colleton.—J. May.
St. James.—J. C. McKewn.
Richland.—G. Trezevant, J. McF. Gaston.
Statesburg.—M. Reynolds.
St. John's Berkley.—Rene Ravenel.
Charleston.—R. S. Bailey, R. F. Michel, G. Trescott, A. Raoul, H. Winthrop, J. P. Jervey, T. E. Hertz, E. Horlbeck, H. R. Frost, H. W. DeSassure, J. P. Chazal, M. Greenland, R. A. Kinloch, W. Pettigrew, W. M. Fitch, J. F. Prioleau, J. J. Chisolm, D. J. Cain, F. Peyre Porcher, J. F. M. Geddings, W. T. Wragg, W. Hume, G. W. Trescott, J. Moultrie, B. A. Rodrigues, J. L. Dawson, W. C. Horlbeck.

The minutes of the last meeting were read and approved.

On motion, it was

Resolved, That the reporters for the daily papers be allowed seats in the Hall during the meeting of the Association.

The President read the annual address. On motion of R. S. Bailey, seconded by E. Horlbeck, it was

Resolved, That the thanks of the Association be tendered to the President for his able and eloquent address, and that the same be referred to the Committe of Publication.

A letter of resignation from Joseph Winthrop was received and accepted.

H. D. Fraser of Charlestou, was nominated and elected a member.

The following resolution, offered by Prof. James Moultrie, and seconded by J. P. Jervey, was unanimously adopted:

Resolved, That this Association has learned with pleasure that Miss Dix, of N. York, is at present on a visit to Charleston, and that it appreciates most highly her philanthropic efforts and eminent efficiency in the humane cause of improving the comforts and advancing the interests of the insane; and that the President be requested to communicate to her this resolution.

J. A. Mayes, of Sumter, who had been appointed at the last meeting of the Association to prepare a paper on any subject he might think proper to select, presented an Essay on "Concentrated Organic Medicines." The writer mentioned that his strength was unequal to the task of reading the whole treatise, and presented a full and interesting abstract of it.

On motion of F. M. Robertson, it was

Resolved, That the paper of Dr. Mayes on "Concentrated Organic Medicines" be referred to the Committee on Publications.

The President announced the appointment of Drs. Moultrie, Reynolds, McKewn, Mayes, and F. P. Porcher, a Committee to nominate Officers of the Association and Delegates to the American Medical Association, to meet at Louisville, Kentucky, the 3d of May next.

Before the Committee lett the room, W. C. Ravenel and F. M. Robertson declined nomination to their respective offices.

The following resolution was unanimously passed.

Resolved, That the thanks of the association be tendered to the following Rail Road Companies for their liberality in allowing the members of the Association to pass over their respective roads by the payment of one fare, viz: The Northeastern Rail Road, Greenville & Columbia Rail Road, Charlotte Rail Road, and the Darlington & Cheraw Rail Road.

On motion of G. W. Trescott, it was

Resolved, That thanks of this Association be tendered to the Trustees of the Roper Hospital for the use of their commodious and comfortable Hall, for the meetings of the Association.

The following preamble and resolutions, offered by J. McF. Gaston, of Columbia, were unanimously adopted:

Dr. P. C. Gaillard, a Vice-President of this Association and a most worthy and efficient promoter of Medical Science, having died since our last annual meeting, it is hereby

1. *Resolved*, That in his death we have sustained a great loss, and that we sincerely lament that he has been called from a situation of honor and responsibility, in which he sustained the dignity of the profession, and in which he signally contributed to the advancement of the public mind in medical knowledge.

2. *Resolved*, That a blank page of our Record be devoted to his memory.

3. *Resolved*, That we sympathize with his family in their irreparable bereavement; and that a copy of these resolutions be furnished to them by the Secretary of the Association, and published with the proceedings of this body.

Dr. D. J. Cain, of Charleston, presented the following resolutions which were unanimously adopted:

Resolved, That in the death of Drs. W. C. Miller and J. M. Brailsford, the Association has sustained the loss of valued members, whose regular attendance at its meetings evinced the interest which they felt in the objects for which it was organized.

Resolved, That blank pages in the Minute Book be inscribed with their names.

The report of the Committee on Nominations was read and adopted. The following are their nominations:

For President—Dr. J. A. Mayes, of Sumter.

For Vice-Presidents.—Dr. H. R. Frost, of Charleston, Dr. John Douglas, of Chester.

For Recording Secretary.—Dr. H. W. DeSaussure, Jr., of Charleston.

For Corresponding Secretary.—Dr. J. P. Chazal, of Charleston.

For Treasurer.—Dr. H. W. DeSaussure, of Charleston.

Committee on Accounts.—Drs. J. L. Dawson, W. C. Ravenel and F. J. Porcher.

Committee on Publication.—Drs. W. T. Wragg, R. Lebby, R. A. Kinloch.

Executive Committee.—Officers of the Association, Drs. J. Moultrie, E. Horlbeck, R. F. Michel, J. P. Jervey.

DELEGATES TO NATIONAL CONVENTION.

From Charleston—Drs. E. Geddings, R. S. Bailey, R. Lebby, H. W. DeSaussure, Jr., J. J. Chisolm.

From Columbia—R. W. Gibbes, Jr., J. McF. Gaston.

From Statesburg—Dr. M. Reynolds, M. S. Moore.

From Lexington—Dr. G. Muller.

From Orangeburg—Dr. Alexander Salley.

From Abbeville—Dr. J. J. Wardlaw.

From Darlington—Dr. T. Dargan.

From Chester—Dr. John Douglas.

For Orator—Dr. J. Dickson Bruns.

A vote of thanks was given to ex-President Gibbes.

The newly elected President returned his thanks for the honor the Association had done him. He reminded them that he had accepted the high office, after warm persuasion; that they had insisted upon his serving, and that if he proved inefficient they must blame themselves.

The Convention then adjourned to meet in the same place at 11 o'clock, Thursday.

On the following day the Association continued its session at the Roper Hospital.

Dr. J. P. Chazal presented a paper from Dr. T. J. Robertson, submitted for publication, which was referred to the Committee on Publication.

Miss Dix sent a letter of thanks, in reply to the resolutions of the Association, passed on the first day of their session, which was received and read.

After the transaction of a variety of miscellaneous matters, the business of the session being completed, the Association adjourned.

CHARLESTON AND COLUMBIA PREPARATORY MEDICAL SCHOOLS.

By reference to the advertisments respecting the above institutions, to be found at the back of this number, those interested personally, or on account of others, in the subject of medical instruction, will be informed in regard to the means and appliances offered to Students wishing to prepare themselves more thoroughly, to appreciate and profit by the advantages attending a course of oral clinical lectures in our colleges and hospitals, during the winter months.

The Charleston Preparatory School commences, in April next, its eighth session. Every effort has been made by its faculty—and with signal success—to furnish each department with such specimens, preparations, engravings and models, as will give their students the best opportunity for acquiring a practical knowledge of medicine and its cognate branches. Clinical instruction will also be given in the Marine and Roper Hospitals by lectures, autopsies, etc., and the Professors will endeavor as often as practicable to exhibit obstetrical and surgical as well as medical cases to the class.

Drs. Talley and Gaston offer their first course of preparatory instruction, in Columbia, at the same date, and from the reputation which these gentlemen justly enjoy, we feel safe in predicting, as we heartily wish them, the highest success.

Let preceptors everywhere encourage their private students to avail themselves of these and kindred opportunities of becoming thoroughly acquainted with the principles and details of their profession, and the standard of medical information and ability will, we doubt not, undergo an elevation as gratifying as it is imperatively called for. Indeed the deficient preliminary education of most of our medical classes is so marked that it is impossible to attempt any measures of re-organization and improvement in the curriculum and requirements of our colleges, until a change is effected in this particular. As things now are, in this respect, we see no more available method, held out in this direction, than that preparatory training for students in such private schools as

the above, where time and opportunity both unite to assure the zealous and diligent student a competent knowledge of the ground-work of the several departments of medicine whose technicalities alone would hinder him, without such previous information, from acquiring the measure of knowledge expected or inferred of him at graduation.

ELECTION OF PROF. E. GEDDINGS TO THE CHAIR OF PRACTICE OF MEDICINE IN THE MEDICAL COLLEGE OF SOUTH CAROLINA.

It is with the sincerest pleasure that we make the above announcement, which cannot fail to redound to the increased reputation of our college, whose success and worth is we know warmly cherished by all her Alumni. The appointment, we are happy to add, has been made under circumstances honorable alike to the electors and the subject of their choice. When the late Professor Gaillard's health became too feeble to permit him to continue his duties in the institution as Professor of Practice, Dr. Geddings generously consented to occupy his chair for him until his restored strength should justify the former in resuming it. Within a very short period from this date, however, Dr. Gaillard's death rather unexpectedly occurred, and his substitute relieved the college at once from an unpleasant and serious embarrassment, by declaring his willingness to continue the lectures until the close of the term.

It is unnecessary to add, that he has fulfilled the requisitions of the chair to the highest satisfaction of the students, whose encomiums have been loud and warm, and it would be superfluous to assert that his re-identification with the interests of the college he has aided so long and ably to sustain, gives double assurance that its reputation and influence will suffer no diminution, at least until material and unforeseen changes shall have very much weakened its supports. We congratulate his colleagues and himself most heartily upon this auspicious re-union.

ANÆSTHESIA AND ITS DISCOVERY.

We extract the subjoined exposition of Horace Wells' claims as the discoverer of the Anæsthetic powers of Sulphuric Ether, from an interesting article published in the February No., 1859, of the Knickerbocker Magazine. After a careful examination of the claims of the contesting parties, we have no hesitation in denouncing the charlatanry of Dr. Morton, and endorsing the statements made below.

In 1799 Humphry Davy, (not then Sir) who was an assistant in an institution in England for the treatment of disease by the inhalation of various substances, commenced a series of experiments with nitrous oxyd, or, as it is often called, laughing-gas. These he published, and although he does not seem to have rendered himself by its use, at any time, wholly insensible, he must have caused some decided effect, for he has written: 'As it appears capable of destroying physical pains, it may probably be used with advantage during surgical operations.'

But his was only a suggestion—a proposition which was never put by him to the test of experimentation: its death was coincident with its birth as far as any real benefit accrued from it to mankind. Nearly fifty years afterward, in the winter of 1844, a public lecture was given in the city of Hartford to illustrate the effects of this very agent, the laughing-gas. Among the audience was a person by the name of Horace Wells, who, struck by its effects upon one of the persons who had inhaled it, made the casual remark: 'That he believed that a person (under its influence) could undergo a severe surgical operation without feeling any pain.' He offered to inhale the gas himself and allow one of his teeth to be extracted. He did inhale it that very evening: a tooth was extracted, and, as he asserted, without the slightest sensation of pain.

A number of experiments were subsequently made with the gas, both by himself and others, and as is shown by the affidavits of many good and reliable men, with an eminent degree of success; for many dangerous and ordinarily painful

operations were performed upon persons who took oath they had experienced no pain whatever. But two objections were found with it: its preparation was somewhat troublesome, and it was rather too bulky for transportation. He accordingly searched for some other agent which should more fully and perfectly fill the needful indications. There was then sold in every druggist's shop an article of common use in medicine, the effects of and method of managing which had been perfectly well known for over five hundred years. This medicine was called an ether, the name of the chemical acid used in its manufacture being prefixed to it by way of designation. As there are many kinds of acids, there are consequently many kinds of ethers; of these the most common is sulphuric ether, sulphuric acid being used in its fabrication. It is a clear liquid, like water, highly volatile, intoxicating in effect, but in a much more rapid and excessive degree than alcohol. For over fifty years it had been recommended by medical men for inhalation in certain diseases of the lungs; and from individual cases where it had been used in this way, it was known that a certain amount of intoxicating effect would be produced. Reasoning on the fact which had been before experimented upon, that alcohol would produce a suspension of personal suffering, and the similarity of effect known to be caused by the inhalation of ether, Mr. Wells determined to try if it would supply the deficiency. This was the step which has left us at our present state of knowledge.

But although much was anticipated by him from the ether he did not consider it wholly satisfactory, for he ultimately returned to the use of his first agent, the nitrous oxyd gas. During a visit made by him to Boston that same winter, he communicated his discovery to an old friend and partner, named Morton. On the sixteenth day of October, 1846, this same Morton made his appearance at the Massachusetts General Hospital in Boston, and there, in the presence of a large number of surgeons and spectators, administered this ether to a man from whom was then removed a large tumor, without his having experienced the slightest pain. As from that

day the surgeon has been able to gauge the amount of suffering he will inflict in any operation, as accurately as the corner-grocer can weigh out a pound of sugar; as narcotics, for three thousand years the sole champions against inflicted pain unconditionally vacated the arena on the approach of the newcomer, it would seem the easiest thing in the world to tell the time of the discovery, and the name of the man who really conferred it.

Three men have stood before the world as claimants for the honor. Horace Wells for his acknowledged use of nitrous oxyd gas in 1844; William T. G. Morton upon the undisputed ground of his public exhibition in 1846; and lastly, Dr. Chas. T. Jackson, who makes the positive personal assertion that he, in 1842, by the accident of inhaling an excessive amount of ether, made the discovery that it would produce a perfect insensibility, and that it was from his information, and at his instigation, that Morton performed the conclusive experiment at the hospital.

By priority of date, it is obvious that the credit should be awarded Dr. Jackson, provided it were perfectly proved, first, that he did discover, in 1842, what he asserts, and, second, that he reduced what was at first but a theory to a certainty, by the test of actual experiment. But here is the dilemma. Nothing was heard of his discovery and claim, until subsequent to its verification by another, while out of his own mouth, it is proved that he never experimented upon what he considered so invaluable a discovery. Whether he induced another to experiment for him, is a simple question of veracity, in which the public have little interest; but as he kept, his secret so well for four years, it is allowable to suppose that humanity might have been none the wiser at the end of forty.

Morton, who evidently considers the pen mightier than the sword, and makes up by multiplicity of documents for weakness of proof, makes a direct denial that he ever received his information from Wells. Yet it is allowed that two years before his public appearance, he knew that Wells was experimenting with nitrous oxyd, and that he conversed with him concerning it. Some corroborative testimony is evidently

needed to show when he formed and experimented upon the theory. The claim of Horace Wells rests upon testimony showing, that from 1844 to 1846, he used both ether and nitrous oxyd gas, to produce anæsthesia ; upon testimony showing that he communicated his knowledge directly to Morton, and probably indirectly to Jackson. Could more be required to establish any demand? Should not this grain of truth, picked from the bushel of chaff with which the antagonism of others has enveloped it, be sufficient under the benign influences of honest investigation, to produce a harvest of honor to the memory of that man, who died unnoticed and unrewarded, after bestowing one of the greatest blessings ever conferred on suffering man?

EXPERIMENTS UPON THE ACTION OF THE HEART—CASE OF M. GROUX.

We lately enjoyed the opportunity of witnessing, in common with our medical brethren of this city, some experiments performed by M. Groux, for the purpose of illustrating the character and extent of the heart's motions under the varying conditions of the respiratory movements. While nothing new has been added to our knowledge upon this subject, an examination of his case offers the strongest confirmation of the views generally entertained among the best physiologists in regard to this subject.

We extract the following succinct and comprehensive description of his case :

" M. Groux is twenty-eight years of age, rather below the the ordinary stature, of a pale countenance, but in good health, and well-formed, excepting the sternal fissure. On the anterior median line of the thorax is a longitudinal V-shaped depression, occasioned by a want of union of the halves of the sternum. The width of this depression during respiration, is about one inch and a quarter at its upper part, gradually diminishing in width as it approaches the ensiform cartilage,

where it reaches its apex. The depth of this depression is ordinarily about half an inch, being somewhat greater during natural inspiration, and very much increased by a forced inspiration. By the action of the pectoral muscles, the width of the fissure can be very much increased, being dilated to about two inches and a half at its upper part; while the action of the deltoids and trapezius diminish and even wholly obliterate it. It can also be increased by forced expiration, and diminished by forced inspiration, at pleasure.

"In this triangular space, two beating tumors can be seen; one, the most prominent, being on a line with, or a little above, the fourth rib; the other below, and toward the bottom of the fissure. A third is also perceptible to the touch at the upper margin of the space.

"M. Groux can produce, by forced and prolonged expiration, a protrusion of the lungs, the depression now becoming a tumor of a semi-cylindrical form, giving a clear sound on percussion. Coughing also exhibits this phenomenon in a remarkable manner, the lung suddenly springing up and falling back, with the greatest rapidity.

"M. Groux also has the power to stop the pulse at will, by taking a full and powerful inspiration, and retaining the breath. The pulsating tumor can also be enlarged by entirely exhausting the lungs of air, and closing the channels to its ingress for a few moments. In the triangular space above mentioned, the motions of the heart, or of certain portions of it, are visible, and its sounds distinctly heard. What composes the principal pulsating tumor in the fissure, is a question upon which physiologists are divided in opinion." Bouillaud of Paris, Sipson of London, and Carlyle of Belfast regard it as the aorta; Stokes and Corrigan of Dublin, and Piorry of Paris think it the right auricle; while Peaslee, Metcalf and Dalton of New York, the Committee appointed by the Pathological Society of that place to examine into the case and report upon it, came to the conclusion that it was the right ventricle.

RETURN OF DEATHS IN THE CITY OF CHARLESTON

FOR DECEMBER, 1858.

WHITES.		BLACKS AND COLORED.	
Abscess of Lungs,	1	Aneurism of Aorta,	1
Apoplexy,	6	Apoplexy,	1
Bladder, Inflammation of	1	Brain, Congestion of	1
Brain, Congestion of	1	Cancer of Uterus,	1
Cancer of Breast,	1	Convulsions,	1
Convulsions.	1	Consumption,	5
Convulsions, Puerperal	1	Congestion, Pulmonary,	1
Consumption,	5	Croup,	1
Delirium Tremens,	1	Debility,	1
Dropsy,	1	Dropsy,	3
Drowned,	2	Enteritis,	1
Epilepsy,	1	Gastritis,	1
Erysipelas,	1	Heart, Disease of	1
Fever, Typhus	1	Kidney, Disease of	1
Fever, Congestive	1	Lungs, Congestion of	2
Fever, Scarlet	2	Meningitis,	1
Fever, Yellow	1	Old Age,	3
Gastritis,	1	Paralysis,	2
Hæmatemesis,	1	Pneumonia,	3
Insanity,	1	Tetanus,	1
Laryngitis,	1	Teething,	3
Lungs, Hemorrhage of	1	Ulcer of Leg,	1
Paralysis,	1	Want of Vitality,	2
Pneumonia,	2		
Stricture,	1	Males, 15; Females, 23. Total,	38
Tetanus,	1		
Trismus Nascentium,	1		
Wounds,	1		
Males 23; Females, 17. Total,	40		
Ages.—Under 1 year,	3	Ages.—Under 1 year,	7
1 to 5,	3	1 to 5,	4
5 to 10,	2	5 to 10,	2
10 to 20,	2	10 to 20,	3
20 to 30,	5	20 to 30,	3
30 to 40,	6	30 to 40,	1
40 to 50,	6	40 to 50,	6
50 to 60,	2	50 to 60,	4
60 to 70,	5	60 to 70,	3
70 to 80,	6	70 to 80,	2
		80 to 90,	2
	40	90 to100,	1
		Total,	38
Natives of Charleston,	24		
Natives of United States,	10		
Foreigners,	6	Natives of Charleston,	26
		Natives of the United States,	11
Total,	40	Foreigners,	1
		Total,	38

J. L. DAWSON, M.D., *City Registrar.*

RETURN OF DEATHS IN THE CITY OF CHARLESTON

FOR JANUARY, 1859.

WHITES.		BLACKS AND COLORED.	
Apoplexy,	1	Apoplexy,	4
Brain, Inflammation of	1	Burn,	1
Consumption,	3	Cholera Infantum,	2
Fever, Typhoid	3	Convulsions,	5
Fever, Scarlet	1	Consumption,	8
Heart, Disease of	1	Croup,	1
Laryngitis,	1	Croup, Membranous	2
Lungs, Congestion of	1	Dropsy,	5
Œdema of Glottis,	1	Fever, Typhoid	1
Œdema of Lungs,	1	Fever, Scarlet	3
Paralysis,	1	Hydrothorax,	1
Pneumonia,	1	Hydrocephalus,	1
Prostate Gland, Disease of	1	Influenza,	1
Rheumatism,	1	Laryngitis,	1
Tetanus,	1	Lungs, Congestion of	2
Unknown,	1	Marasmus,	1
		Meningitis,	1
Males, 15; Females, 6; Total,	20	Old Age,	7
		Paralysis,	2
		Pneumonia,	9
		Teething,	1
		Trismus Nascentium,	6
		Unknown,	3
		Want of Vitality,	2
		Males, 32; Females, 38; Total,	70
AGES.—Under 1 year,	1	AGES.—Under 1 year,	20
1 to 5,	3	1 to 5,	8
5 to 10,	1	5 to 10,	2
10 to 20,	3	10 to 20,	4
20 to 30,	3	20 to 30,	4
30 to 40,	2	30 to 40,	3
40 to 50,	2	40 to 50,	8
50 to 60,	1	50 to 60,	7
60 to 70,	4	60 to 70,	3
		70 to 80,	6
Total,	20	80 to 90,	3
		90 to100,	2
Natives of Charleston,	10		
Natives of United States,	5	Total,	70
Foreigners,	5		
		Natives of Charleston,	61
Total,	20	Natives of the United States,	9
		Total,	70

J. L. DAWSON, M.D., *City Registrar.*

METEOROLOGICAL ABSTRACT FOR DECEMBER, 1858.

BAROMETER.

Maximum, 30.670, - - Minimum, 29.812, - - - Range, 0.858

THERMOMETER ATTACHED.

Mean.

7 A. M., 56.00, - - - 2 P. M., 59.58, - - - 9 P. M., 58.90

THERMOMETER DETACHED.

Mean.

7 A. M., 54.64 - - - 2 P. M., 60 77, - - - 9 P. M., 57.90

Highest Degree.

7 P. M., 68, - - - 2 P. M., 71. - - - - 9 P. M., 68

Lowest Degree.

7 A. M., 35, - - - 2 P. M., 45, - - - - 9 P. M., 43

REGISTER THERMOMETER.

Mean.

Night, 50.87, - - - - - - - - - - - Day, 61.83

DEW POINT.

Mean, - - - - 52.25

WINDS.

Sun Rise—N.E. 10 days; N.W. 2; S.E. 7; S.W. 10; E. 2. Prevailing N.E.
4 *P. M.*—N.E. 10 days; N.W. 1; S. 1; S.E. 6; S.W. 8; E. 3; W. 2. Prevailing N.E.

WEATHER.

Fair, 11 days—Cloudy, 8 days—Rain, 12 days—Prevailing, Unfair.

QUANTITY OF RAIN.

Inches, - - - 2.66.

JOHN L. DAWSON, M.D.

METEOROLOGICAL ABSTRACT FOR JAN., 1859.

BAROMETER.

Maximum, 30.650, - Minimum, 29.910, - - - Range, 0.740

THERMOMETER ATTACHED.

Mean.

7 A. M., 49.74, - - 2 P. M., 54.39, - - - 9 P. M., 52.61

THERMOMETER DETACHED.

Mean.

7 A. M., 46.29, - - 2 P. M., 55.29, - - - 9 P. M,, 50.74

Highest Degree.

7 A. M., 64, - - - 2 P. M., 70, - - - - 9 P. M., 66

Lowest Degree.

7 A. M., 25, - - - - 2 P. M., 34, - - - 9 P. M., 29

REGISTER THERMOMETER.

Mean.

Night, 43.70, - - - - - - - - - - - Day, 56.87

DEW POINT.

Mean, - - - 41.70.

WINDS.

Sun Rise—N. 2 days; N.E. 11; N.W. 4; S. 1; S W. 7; E. 3; W. 3. Prevailing N.E.
4 *P.M.*—N. 1 day; N.E. 9; N.W. 8, S.E. 1; S.W. 7, E. 4, W. 1. Prevailing N.E.

WEATHER.

Fair, 16 days—Cloudy, 6 days—Rain 9 days—Prevailing—Fair.

QUANTITY OF RAIN.

Inches, - - - - 2.57.

JOHN L. DAWSON, M.D., *City Registrar.*

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CHARLESTON
MEDICAL JOURNAL AND REVIEW.

VOL. XIV.] CHARLESTON, S. C., MAY, 1859. [No. 3

ART. I.—*Report of the Meteorological and Sanitary condition of Charleston, for the year* 1857–8, read before the Medical Society of South Carolina. By H. W. DESAUSSURE, M.D., President.

Gentlemen:—In a second time presenting for your consideration, a report of the weather and diseases of the two years, during which I have had the honor again to occupy the position of your presiding officer, I shall endeavor to continue the comparison made in my last report, of the diseases of each year and season, and to extend it, by including references to such of the prevailing diseases of 1855–6, as may seem to have been produced or modified by Meteorological phenomena. If in making this comparison, no positive results are attained, it must be borne in mind, that it is only by observations extended through long series of years that any absolute data can be established. "To reduce into classes," says Sydenham, "all the species of epidemics, to arrange them according to their phenomena, to work out their several idiopathic characteristics, to determine in detail the proper treatment of each of them, is a work so difficult, an arrangement requiring so much leisure and opportunity, a cycle so little coincident with any recognized sequence of years, that the life time of no single physician would suffice for the collection of a sufficiency of data. Great, however, as such an undertaking may be, it must be got through, before we can

venture to state, that in so complex a series of diseases, we have done any good work worthy of mention."

Our constitution in requiring from the presiding officer, a biennial report of the weather and diseases, is accumulating for our successors, a store of those observations, necessary for forming the only sure basis of a true history of the diseases incident to our climate and locality.

Of the two years comprised in the ensuing report, the first presents about the ordinary average of disease and death; marked by no serious or fatal epidemics, it offers only the average rate of mortality. The second on the contrary, will remain for years a sad reminiscence in the hearts of the many who have been afflicted by its devastating pestilence. Frequent as have been the epidemics of yellow fever with which we have been visited, few can compare with the afflictive and fatal visitation of '58, and years must probably elapse ere its fatal results will cease to be remembered to the detriments of our commercial and social interests.

In commenting on the medical history of the past two years, they will as on a former occasion, be divided into quarters, and the phenomena presented, by each, considered separately.

The first quarter of the year '57, comprising the months of December, '56, January and February, '57, was marked by a degree of cold, unusual to our climate, both in degree and persistence. With the exception of a few days in the latter part of December, there was steady persistent cold weather; during all the two months of December and January. The average of the thermometer for the latter month, during the day being only 49°; falling at night as low as 32°. The month of February was much milder, but still without marked alternations of temperature. The barometrical range of the quarter was moderate, as was also the atmospheric pressure, 30 in. .20 being the average barometric height. The thermometer ranged during the quarter from 14°—the lowest recorded degree since 1835—to 77°. The day average was 59°. That of the night 40°. The quantity of atmospheric moisture was

considerable, 41° being the average dew point at sunrise, making .913 as the quantity watery vapour. The quarter was dry and fair, rain having fallen on only 22 days; the quantity amounted to 3in. .97.

The diseases prevailing during the quarter, were, of the zymotic class, typhoid fever, scarlatina, measles, hooping cough, membranous inflammations of the throat, dysentery, variola and varioloid: of acute inflammatory affections, bronchitis, pneumonia and rheumatism, were most prevalent. The mortality during the quarter was 223, being rather less than for the corresponding period of the two preceding years. From zymotic diseases, the deaths were only 13, or 5 per cent of the whole mortality: the greater portion of the deaths from zymotic causes, occurred among the white population, being 9 out 13; typhoid fever caused 4 of these 9 deaths. It may be remarked here that although scarlet fever, measles and hooping cough, were prevailing epidemically during this quarter, no deaths from these diseases are reported during this period. It is also worthy of remark, that during the month of February, no death from zymotic disease is recorded among either the black or colored population.

Variola and varioloid, are mentioned, as among the prevailing diseases of this quarter; the facts connected with its origin are worthy of a passing notice. In January, a man arrived from Florida, by steamboat, went to a boarding house, and from thence to the Roper Hospital; he had the small pox eruption developed at this time; he was transferred from the hospital to the Pest house. He stated, that he must have contracted his disease from an individual on board the steamboat, convalescent of small pox. No individual either in the boarding house where he lodged, or in the hospital was infected by him. But about the same time an old colored woman, living in a distant part of the city, was attacked with variola in a severe form. She also was removed to the Pest house, and the disease did not spread in the locality in which she lived; no communication could be traced between her and the individual above mentioned. About three weeks later, the disease broke out in the Church

Home, in a part of the city distant from either of the above cases, and spread through the institution, almost every individual in it being attacked; no origin for the disease could be traced. The person first attacked was a child, an inmate of the house, who could not have had any communication with either of the above cases. The disease did not spread in the vicinity of the Church Home, but about three months afterwards, in June, after the disease had run through the inmates of this house, a colored man living in a still more distant part of the city, was attacked with variola, and died. In him also, no communication with with any infected person or place could be ascertained. The apparently spontaneous origin of a disease, whose source is almost invariably attributed to direct contagion, very nearly about the time of its introduction from abroad, is one of those curious facts and coincidences in the history of contagious diseases, of which our knowledge as yet furnishes no solution.

Of the sporadic diseases of this quarter, affections of the nervous and respiratory systems, constituted the largest proportion; 25 per cent of the deaths occurring from diseases of the nervous system; and 27 per cent from affections of the respiratory organs. The largest proportion of this mortality falling upon the colored population. Of diseases of the respiratory organs, consumption caused the largest mortality; 50 per cent of the deaths from diseases of the respiratory organs among the white, and 60 per cent among the colored population, being from this source alone. Of acute affections of this class, 21 per cent of the deaths among whites, are recorded as occurring from pneumonia, and 34 per cent among the colored population.

The most marked feature of the months of March, April and May,'57, was the very moderate temperature, and the occurrence of frequent cold days. The average temperatue of the month of March, was two degrees lower than that of Feb., and that of April one degree less. Frosts occurred frequently during the latter month. May was also very cool, its average temperature being lower than the corresponding period of the two preceding years. The range of the Barometer was low,

but little exceeding a half inch, and the atmospheric pressure small, the mercury averaging only 30.06 for the quarter. The range of the thermometer was 60°, the average of the day temperature 69°, of the night 53°. The quantity of atmospheric moisture was small, the average dew point being 5° below the thermometric average at sunrise; in figures, the atmospheric moisture would be represented by .857. The prevailing character of the quarter was dry and fair, with a prevalence of W. NW. and N. winds; rain fell on 19 days only, amounting to 5in. .81.

Of the zymotic diseases mentioned as prevalent during the preceding quarter, scarlet fever was still occurring during the month of March, but disappeared almost entirely during the succeeding months. Measles, hooping-cough and typhoid fever, however, continued to prevail during the entire quarter, and of a more aggravated type than previously, 4 deaths occurring from Hooping cough and 3 from Measles, during the quarter. The cases of Typhoid fever also assumed a more serious aspect, and became more unmanageable, 7 deaths occurring from this disease. Membranous inflammations of the throat mentioned, as characterizing the preceding quarter, were rare during the present. During the middle and last of the quarter, dysentery, diarrhœa and cholera infantum, made their appearance; 16 deaths of these diseases are registered for the last two months of the quarter. Influenza, also appeared as a moderate but mild epidemic, during the quarter; the cases were numerous, but only one death is recorded from it. Many cases of erysipelas, especially of the head, face and neck, were also met with, one death from this cause also occurred.

The mortality for the quarter was large, being 274, an increase of 27 over the corresponding period of '55; and 38 over that of '56. This increase occurs entirely among diseases of the nervous, respiratory and digestive systems. The deaths from zymotic causes, being remarkably uniform for the three years, viz: for '55, 38 deaths, for '56, 34 deaths, and for '57, 33 deaths.

From diseases of the nervous system, the deaths were 71, almost 30 per cent of all the deaths during the quarter. Of this large mortality, the greater portion fell upon the colored population, 60 per cent occurring among that class. Trismus Nascentium, and convulsions are the chief causes of this disparity between the white and colored population. The deaths among the latter, from each disease being exactly double that of the whites. Diseases of the respiratory system, make up 49 or 19 per cent of the quarter's mortality. The colored population are again the greatest sufferers, 60 per cent of the deaths from these causes are registered as among the coloured population. Consumption is the cause of this disparity, double the number of deaths from this disease occurring among the colored classes. Of the deaths from pneumonia during the quarter, 66 per cent were among whites; which is adverse to the generally received opinion that the constitution of the negro is less able to bear, the extension of cold late into spring than that of the white man. During corresponding periods of the two preceding years, when the spring temperature was milder, the deaths from pneumonia were nearly uniform among black and white.

During the present quarter also, the deaths from diseases of the digestive are the largest of the 4 years from '55 to '58, being nearly double those of the three years. This increase results from a larger proportionate mortality, among both classes of the population from enteritis, hepatitis, jaundice and teething. The causes in operation, whatever they may have been producing the increase of these diseases, evidently affecting both classes equally.

The summer months of June, July and August, were remarkable for their low temperature, the thermometric average of the day being only 85°, much lower than during the corresponding period of '55 and '56, and somewhat lower than during that of '58. The average night temperature was 74°. The month of June was dry. July and August on the contrary, were marked by frequent and heavy rains, accompanied by much and vivid lightening. Rain fell on 33 days, amounting to 13in. .16. Almost half of the whole quantity

of rain falling during the whole year, occurred in this quarter. The winds prevailed during this quarter, chiefly from SE. S., and SW.

During the summer months, measles and hooping cough continued to prevail epidemically, in a more severe form than in the preceding quarters, 28 deaths from these causes are registered during the quarter. This however, was a small proportionate mortality, as the number of cases that occurred during the quarter was very great. Dysentery, diarrhœa and cholera infantum were also epidemic, and caused a much larger proportionate mortality; 60 deaths occurring from these three causes. The largest zymotic mortality of the summer, 81 per cent of the whole, resulted from the five diseases above mentioned. A few cases of scarlet fever occurred, but they were mild, only a single death being caused by it. Typhus fever also prevailed in a mild form, 4 deaths occurring from it, these being among the colored population entirely. Malarious fevers were rare during the quarter, the hospital records registered but few cases, and these were mild and manageable; no deaths among the whites from this cause is registered during the quarter. The great mortality from diarrhœa, dysentery and cholera infantum, and the infrequency of malarious forms of disease, perhaps bear some relation to the cool summer, and the low thermometric average of these months. Cases of urticaria, erythema and allied eruptive diseases, usually attributable to intense summer heats, made their appearance during the quarter, in considerable numbers. From the low temperature observed during the summer, it is evident that their prevalence was due to some other cause than atmospheric heat.

Two cases of yellow fever were carried to the Marine Hospital in August, which require some record. On the 26th of July, the galliot Johannes arrived from Emden, Hanover, after a passage of 62 days; having had some of her passengers and crew sick with ship fever, she was sent to quarantine. The bark Koophandel, had arrived from Rio in Brazil, on the 27th June, after a passage of 49 days. Having left a port infected with yellow

fever, she was put into quarantine on her arrival, although none of her crew had been sick on the voyage. No cases of fever occurred on board after her arrival. On the 11th of August, the Koophandel and the Johannes, came together at quarantine station, for the purpose of exchanging ballast, and remained lashed side by side until the 18th of August. The crews of both vessels intercommunicated freely. On the 25th of August, the Johannes left quarantine station to go into Ashley river. On the 27th she sent two of her crew sick into the Marine Hospital; on the 28th one threw up black vomit, and both were decided to be suffering from yellow fever. They were immediately sent to the lazaretto, where they both died. No other cases occurred in the Hospital, nor were any others received there during the year.

The mortality of this quarter, 416, is a large increase over the two preceding, being almost double. It is a larger mortality than for the corresponding period of '55, but not so large as for the same perod of '56. This last year, however, was one of our seasons of epidemic yellow fever. The zymotic mortality of the quarter, is, however, proportionately less than for the preceding years, being 25 per cent of the whole for '57, whereas, for '55, and '56, the zymotic mortality was 30 per cent. Diseases of the nervous system, bear the next largest relative proportion to the total mortality for the quarter, being 16 pr. cent of the whole. Of this class of diseases convulsions and acute diseases of the brain, among the whites, apoplexy, convulsion and trismus nascentium among the blacks, are the chief causes of death. Diseases of the respiratory system bear also the proportion of 16 per cent to the general mortality of the quarter; 51 per cent of the deaths among the whites were produced by consumption, while pneumonia and bronchitis make up almost the entire of the rest. Among the blacks 66 per cent of diseases of the respiratory organs result from consumption. Pneumonia and congestion of the lungs constituting the remainder. For the corresponding periods of '55 and '56, the proportionate mortality by consumption was 80 per cent, while the deaths by pneumonia were very few. The mortality of children under

10 years of age is extremely large, being nearly 52 per cent of the whole. As compared with '55, it is larger by 6 per cent, and 1 per cent larger than in '56.

The fall months of September, October, November, '57, were generally cooler than the corresponding periods of the two preceding years. With this difference, however, that although in '57 the day temperature was much lower, the night temperature was rather higher than in either '55 or '56. The general thermometric average of the whole quarter was lower by 3 degrees than during the preceding two years. In November of this year, occurred a lower temperature than was found in the succeeding three winter months. The thermometer marking 29 degrees during this month, whereas the lowest degree of December, January or February, was 35 degrees. The atmospheric pressure was low. The barometer averaging 30.13. for the quarter, the same as during the preceding quarter. The quantity of watery vapour was less than during the preceding quarter, but larger than during the spring months;. 887 was the average quantity of moisture for the quarter. Fifty-seven days of the quarter only were fair, the remainder cloudy or rainy. The quantity of rain which fell during the quarter was 5.22, a larger quantity than during either of the preceding two years.

The zymotic diseases of the quarter were hooping cough, measles, typhoid fever, dysentery and diarrhœa. In my note book I find a reference to a number of cases characterized by fever, varying in duration from 6 to 36 hours, characterized by severe frontal headache, intense muscular pains, not confined to the limbs, but involving the trunk also, unattended by coryza, but almost invariably followed by bronchitis, after the cessation of the febrile paroxysm. Some cases were succeeded by pneumonia, after the cessation of the first acute febrile symptoms. In a few cases attended by only moderate bronchial symptoms, extreme depression of several days duration, succeeded the febrile paroxysm. These cases commenced early in September, and continued until late in October. They were not fatal, and yielded readily to moderate anti-phlogistic treatment. They probably might be classed as influenza. Although the symptoms of catarrh

were not marked as among the early symptoms. During this quarter, also, several deaths occurred from yellow fever, these were 12 in number, 10 whites and 2 blacks. The first death occurred on 30th September. The patient had contracted the disease on Mount Pleasant, where it was prevailing, was brought to the city and died. The second death was that of a policeman living in the lower part of King street; he died on the 3d of October. The 3d case was in the neighborhood of the case brought to the city from Mount Pleasant; this patient died on the 7th of October. On the same day, also, died in Queen street a young gentleman, a clerk on East-Bay. On the 9th October, a woman who had been employed as servant in the house, to which the first case was carried, died. A negro man, brought from Mount Pleasant, ill with fever, was the sixth death. The 7th death was, also, a negro who died on East-Bay; he had been sent on a message to the house in Queen street, where the young clerk was sick, and died. Two deaths then followed, of Irishmen living in the Eastern section of the Upper Wards; one in Henrietta st., the other in Reid street. Another death then occurred in the lower part of King street, and a young German, a grocer's clerk, was taken to the hospital from this part of King street, he recovered after throwing up black vomit. Another case was taken to the hospital from Queen street, from the neighborhood of where the clerk from East-Bay died. One or two other cases occurred, but the disease did not spread into an epidemic. Of the number of cases which occurred, it is impossible form a correct estimate. They, perhaps, did not exceed twenty-five in number, as the largest estimate; even assuming this as the number, the proportionate mortality was large, being 50 per cent. Of the origin of the disease nothing can be said with certainty. The early cases occurred very nearly at the same time, and about the time that the first fatal case was brought over from Mount Pleasant, and were scattered over distant sections of the city.

In commenting on the diseases of the first quarter of the present year, it was remarked, that about the time of the introduction of a single case of small pox, from abroad, other

cases made their appearance in different and distant sections of the city without traceable communication between any of the first cases. It is a remarkable coincidence, that during the same year, the same phenomena should be presented in relation to a disease believed by many to be endowed with contagious properties, and capable of transmission by contagion. Such isolated facts are of little value in themselves, and in the language of Sydenham, already quoted, "the lifetime of no single physician would suffice for the collection of a sufficiency of data to reduce into classes all the species of epidemics, and to arrange them according to their phenomena." But it must be by the accumulation of such facts, that the materials may be laid for our successors to make those deductions, and generalizations, without which no useful progress can be made in tracing the origin and extension of diseases which possess either purely epidemic or mixed epidemic and contageous characters.

The mortality of this quarter was only 320, nearly 100 less than the preceding quarter, of which 23 per cent only was from zymotic causes. Among the whites more than one-half the deaths by zymotic disease were from remittent, typhus, and yellow fever. Among the blacks, dysentery, typhoid fever, and hooping cough, have the largest proportion of deaths. Of the deaths from other causes, diseases of the nervous and respiratory systems, have the largest proportion, and are very nearly equal in amount. Diseases of the digestive organs, which, during the summer months gave a larger mortality than in the winter and spring quarters, now begin to recede, and during the present quarter present but a small proportionate mortality. The infant mortality, which, during the last quarter, had mounted up to 52 per cent, during the present falls as low as 34 per. cent. This is a higher mortality than that of the winter quarter, which was only 12 per cent, but lower than for the spring quarter, that rising as high as 42 per cent.

The winter months of December, '57, January and February, '58, were characterized by extreme mildness of temperature. The general thermometeric average being much above that

of the preceding quarter. Ice was never seen in the city during the two first months of the quarter, and only once during the last month, the thermometer, during the whole quarter, did not fall below 35 degrees. The mean of the thermometer, during the quarter, was as high as 48 degrees; the day mean was 58 degrees. The atmospheric pressure was high, the barometer averaging 30in. .22., a higher mean than any quarter of the preceding year. The quantity of atmospheric moister in December, was as low as .862, rising in January to .925, and in February to 1000., the point of complete saturation. The amount of rain during the quarter was very large, reaching 12in. .97. The prevailing winds were E. SE. and S. and SW.

The mortality of the quarter was 236, very nearly the average of the corresponding periods of '55 and '56, rather higher than that of '57. The zymotic mortality of the quarter was 14 per cent of the whole, a larger proportionate mortality from this class of diseases than during any of the preceding three years. The proportion which the deaths from zymotic diseases bore to the whole mortality of the quarter, was, in '55, 13 per cent; in '55, 9 per cent; in '57 only 5 per cent. The winters of '56 and '57 were cold and dry; those of '55 and '58 warm and wet. The deaths from this class of diseases, during this quarter, were nearly equally distributed between measles, hooping cough, typhoid fever and croup, the two latter offering the largest mortality. It was remarked, in commenting upon the winter quarter of '57, that no death by zymotic disease among the colored population is registered for February. During the month of January, '58, the same fact may be stated for the whites. Of the deaths from other causes during the quarter, diseases of the respiratory organs bears the proportion of 28 per cent to the whole. This is a higher rate than that of '55 and '56, but almost exactly the same as that of '57. Of the causes of death from this class of diseases, 29 out of 34 deaths are due to consumption and pneumonia, among the whites, and 28 out of 31 to the same causes among the blacks.

The diseases of this quarter were chiefly influenza, which frequently terminated in pneumonia, or capillary brochitis, and typhoid fever of rather an aggravated type. Mild cases of scarlet fever and measles were very frequent; hooping cough was also constantly met with. Two cases of varioloid were seen during the quarter, the origin of which could not be traced. Various eruptive affections were also seen during the quarter, the chief of these were varicella, pemphigus, herpes, in all its varieties, roseola, urticaria, &c. These and allied eruptive disorders had made their appearance in a moderate degree during the preceding summer, as was mentioned, and had gradually increased, until during the present quarter they became very abundant, but always mild.

The spring months of '58, March, April and May, were pretty uniformly cool. The day temperature was rather lower than that of the preceding three years, and the night temperature a little higher, making a little higher thermometric average for the whole twenty-four hours, than in the corresponding period of '57, and somewhat higher than that of '55 and '56, although the difference in degrees of the thermometer is slight. Uniformity of day and night temperature is perhaps, the characteristic feature of this quarter over that of the preceding 3 years. During the early part of the month of March, more intense and continued cold occurred than during any of the winter months, the thermometer fell to 29° in the early part of this month, a point which it had not reached since November, of '57, and ice remained unthawed for four days, a degree of cold not experienced during the whole of the winter quarter. The barometric average was very low 30.05, indicating but little atmospheric pressure. The quantity of watery vapour was also small, .889, marking the degree of moisture in the atmostphere as the average of the quarter. The spring was also a dry one, only 6in. .90 of water having fallen during the quarter, the greater part of which was in May. Rain fell on only 14 days during the quarter, and chiefly in short heavy showers. The prevailing winds were S. SW. and W.

The diseases of this quarter were chiefly of a catarrhal type. Bronchitis, pneumonia and influenza, were frequent, but unusually mild. Measles and hooping cough still prevail as epidemics, but are of mild type. Scarlet fever existed in a great many limited localities in various parts of the city, but its character were benign—although some cases were fatal from sore throat. Varioloid appeared in several individuals without any trace of personal contagion. The various eruptive diseases, herpes, pemphigus, varicella, erysipelas, Urticaria, &c., were very frequent but mild. There seemed to exist during the quarter, a strong disposition to the developement of exanthematous affections, many of which were so anomalous in their character, as to defy classification. Inflammations of the fauces and air passages, with tendency to the formation of membranous deposits, were also frequent, and as allied affections may also be mentioned, cases of bronchitis, with a tough plastic secretion, difficult to expectorate, and harrassing from the incessant and painful cough, necessary to dislodge the secretion. It is also worthy of mention, that malarial fevers made their appearance during the last month of this quarter, earlier than they usually appear, and in strong contrast to the preceding year, when they scarcely were noticed until the fall months.

The total mortality of the quarter, was 255, a small diminution over that of '57, but a small increase over that of '55 and '56. The zymotic mortality of '58 is larger by 2 per cent than that of '57, for the same period, and about equal to that of '55 and '56. The deaths from zymotic diseases are due chiefly to scarlet, typhoid and remittent fevers, among the whites. Among the colored population they are divided between typhoid fever, measles and influenza. The spring disorders of children which usually make their appearance during this quarter, are scarcely noticed in the mortuary books or private records of this year. The mortality of children under 10 years of age, is less by 6 per cent than that of the corresponding quarter of the preceding three years. The mortality of this quarter seems to have been divided nearly equally among a few acute diseases, as apoplexy, dis-

eases of the brain, trismus nascentium, and pneumonia and the ordinary chronic diseases inevitable in all climates and seasons.

The summer months of '58 were marked by extremely high temperature, and unusual stillness of atmosphere. The heat commenced in the latter part of June, and continued almost without interruption to the end of August: during this last month, the thermometer ranged between 90° and 94°, for eight successive days; the nights were frequently still, and without the refreshing breezes common to our latitude. The first month of the quarter was dry, the two latter, marked by frequent and heavy showers, accompanied by much and vivid lightning. The barometric average was highest of any quarter of the two years, the mercury standing at 30 in. .23. The quantity of atmospheric moisture was also very great, .933 representing the amount of watery vapour. The prevailing winds were chiefly from SE. S. and SW. The prevailing diseases of the quarter were measles, hooping cough and scarlet fever. The exanthematous affections remarked during the last quarter, also continued to prevail extensively during the present. In addition to these, diarrhœa, dysentery and cholera infantum, the usual accompaniments of heat, appeared during the quarter. Remittent fevers were also frequent during the quarter, and the cases in many instances assumed a serious or even fatal type. During the last month of the quarter, yellow fever appeared and speedily assumed an epidemic type. As the largest number of deaths from this disease occurred during the succeeding quarter, comments upon it will be reserved for the present.

The mortality of the quarter was very large, amounting to 479, almost double that of the preceding quarters, but much smaller than for the corresponding period of '57, if the deaths by yellow fever be subtracted from the general mortality. The proportion which the zymotic deaths bear to the whole mortality of the quarter is very large, being 42 per cent; if however the deaths by yellow fever be thrown out, the proportionate zymotic mortality is nearly the same as for the preceding four years, viz: 24 per cent. Excluding yellow fever, the

deaths from zymotic diseases among the whites were from scarlet, typhus and remittent fevers, cholera infantum and diarrhœa; among the colored population from typhus and remittent fevers, cholera infantum and diarrhœa. The relative proportions which deaths from diseases of the nervous and respiratory organs bear to the mortality of the quarter, of course exclusive of deaths by yellow fever—is much smaller than during the corresponding period of any of the four years examined, amounting only to about 16 per cent. Diseases of the digestive organs, show a large increase of proportionate mortality over all the preceding years, rising as high as 14 per cent. Enteritis, hepatitis and teething among both classes of the population, are registered as the fatal diseases.

The fall months of '58 were cool, with a lower average temperature for the quarter, than for any of the three preceding years. The early part of September was warmer than the average temperature of the season, but after the 10th September, the weather became dry, cool and without alternations to the end of November. In '55 the temperature fell during the month of October to a degree sufficiently low to produce frost and ice, but in '56, '57 and '58, no frost occurred until November. The uniform but moderately low temperature of the present year, was sufficient without the occurrence of frost, to modify and eventually to arrest the summer diseases, especially yellow fever, before the absolute occurrence of ice. The barometric average of the quarter was moderate, 30in. .20 being the average height of the mercury during the quarter. The quantity of atmospheric moisture in September was great, .900 being the average for the month; in November it fell as low as .822, giving an average below .800 as the mean of the quarter. The prevailing winds were from NE. E. and SE. There was rain on only 22 days during the quarter; the quantity of water which fell was considerable, being 10in. .72. The greater part of this was in the early part of September, the rest of the quarter being dry.

The prevailing diseases were during the early and middle portions of the quarter, yellow fever and remittent fever

with a few cases of typhoid fever. A few cases of remittent fever occurred in the lower wards of the city; in the upper wards they were more numerous, and modified in some instances, in the unacclimated by the prevailing type of fever; in the acclimated no modification of type was noticed. During the latter part of the quarter, scarlet fever began to appear, and many of these cases made their first appearance in the very localities where yellow fever had been most abundant. During November also, cases of membranous inflammation of the fauces and air passages, began to present themselves. During this quarter occurred the largest absolute mortality that has ever been known in the city. The number of deaths during the quarter was 955, 23 deaths more than for the corresponding period of '54, which was known as the worst epidemic of yellow fever since 1817. Of the whole mortality of the quarter, 69 per cent was due to zymotic disease alone. Of zymotic diseases 91 per cent of the whole mortality resulted from yellow fever. Remittent, Typhus and Scarlet fevers, and croup make up the rest of the zymotic mortality.

Of sporadic diseases, convulsions, appoplexy and diseases of the brain, bear the largest share of the deaths from diseases of the nervous system. Of diseases of the respiratory organs, consumption produces 81 per cent of all the deaths.

The most important feature of the two years comprised in the above report, is the severe epidemic of yellow fever which marked the summer of 1858. Commencing early in August, the last deaths reported were for the week ending November 20th. The absolute mortality exceeds that of all former years, 715 deaths being reported from it, a larger majority by 100 than occurred in the epidemic of '54. To endeavor even to trace the whole history of so grave an epidemic, would be out of place in a paper like the present. Some of the most important features connected with its origin and progress, must, however, make a part of the sanitary history of the year.

The first deaths of yellow fever occurred on the 15th and 18th of July, in a house in Tradd-street, two doors west of

King. One of these patients, the first who died, had been recently attached to the Steamer Catawba, just arrived from Havana. The second death was a man living in the same house, whose brother-in-law had also recently left the Catawba, and gone to live with him; the brother-in-law had also been sick, but had recovered. His attack however, was only nervous and gastic disarrangement from intemperance. About the same time cases of yellow fever appeared on board the Catawba, then lying at quarantine. The third death occurred also in Tradd-street, near the Bay; he was a policeman, his antecedents not known. On the 5th August, a negro recently from the country, living in Broad-stseet near the Court House, died. On the 12th August two persons living in Tradd-street, near the corner of King, died. On the 11th and 12th August, three native children living at the corner of King and Tradd-street, were attacked, two were colored one white. A native child living in Meeting-street above Queen, was also attacked on the 12th, this child had been passing along Tradd and King-streets several times at night during the few days preceding his attack. The seventh death occurred at the corner of Tradd and King-streets, on the 13th. The eighth on the same day in Queen-street, west of King, and on the 14th another died in the same locality. The next death was also on the 14th, and on the 15th a carter died in Linguard-street. The cases now rapidly increased, spreading up King-street to the north, and along Tradd-street to the East. In Linguard-street also, where the carter died, cases began to occur in great numbers. During the early part of the epidemic, the larger majority of cases were in the lower wards; later in the season the disease spread into the upper wards, leaving the lower comparatively free.

The following abstract made from a table prepared by the Howard Association, of the date and number of applications for relief will best exhibit the gradual progress of the fever, from the Lower to the Upper Wards. It may be premised that this table relates entirely to the fever cases, all others being excluded. It may be further stated, that the two first deaths occurred in Ward, No. 2; the 3d death in Ward, No.

1; the 6th case occurred in Ward, No. 4, and the 11th case was in Ward, No. 3. All these cases were previous to the 15th of August. On the 25th and 26th, the first application for relief from the Upper Wards was made to the Howard Association; these patients lived in Wards 7 and 6; Wards 1 and 2 are the smallest wards of the city. With these explanations the progress of the disease will be better understood.

During the month of August, from Ward, No. 1, 16 applications for relief were made; from Ward, No. 2, 15; from Ward, No. 3, 53, and from Ward, No. 4, 7. From the Upper Wards only 3 applications were received, two from No. 7, and 1 from No. 6. During the month of September, the applications were as follows: from Ward, No. 1, 51; from Ward, No. 2, 40; from Ward, No. 3, 210; from Ward, No. 4, 108; from Ward, No. 5, 45; from Ward, No. 6, 42; from Ward, No. 7, 35; from Ward, No. 8, 107. During October, from Ward, No. 1, 14; from Ward, No. 2, 7; from Ward, No. 3, 44; from Ward, No. 4, 34; from Ward, No. 5, 88; from Ward, No. 6, 57; from Ward, No. 7, 56; from Ward, No. 8, 91. In November, there were only 7 applications from the Lower Wards against 26 from the Upper. To sum up the above statement, it appears that, during the month of August, 91 applications were received from the Lower Wards, only 3 from the Upper. In September, 409 applications were received from the Lower Wards, 229 from the Upper. In October, 99 applications were received from the Lower Wards, 292 from the Upper. Hence, it appears that, during the whole month of August, the disease was confined to the Lower Wards, and did not spread extensively in the Upper, until September; the greatest number of cases in these Wards occurring in October, at which time the fever had diminished very much in the Lower Wards.

From the same table we extract the following statements, which are interesting, as shewing the progress of the fever. The largest number of applications for relief, on any one day, were 35, on the 12th of September, during this week, the sixth, since the commencement of the epidemic 195 individu-

als applied for assistance; during the succeeding week, the applications fell to 137, but rose the week after, viz: that ending 2d of October, to 159. An examination of the table shows that this increase was due to the increased number of applications from the Upper Wards, where the fever was then beginning to assume its greatest degree of extention.

A marked feature connected with the epidemic of this season, was the small number of cases existing along the wharves, East of East-Bay street, and the few cases occurring among the sailors in the vessels lying at the wharves. During almost all former epidemics, the earliest cases of fever occurred among the ship's crews, and extended from thence into the city. During this year the reverse occurred, the fever commenced in the city, and extended, late, to the shipping. In evidence of this it may be stated that in '54, 149 cases were received into the Marine Hospital; in the present year, only thirty-six.

It should be stated that since '55, no vessls arriving from ports infected with yellow fever, are allowed to come up to the wharves, but their cargoes are discharged from quarantine and sent to isolated stores in the extreme West of the city.

The absolute mortality of the disease has already been stated to be 715. The relative mortality for Hospital practice, was, in the Roper Hospital, 56 per cent, in the Marine Hospital, 55 per cent. In the same hospitals, in '54, it was much lower, being 34 per cent in the Marine Hospital, 36 per cent in the Roper Hospital, and 47 per cent in the Alms House Hospital. In the report made by the Secretary of the Howard Association, the number of cases of yellow fever relieved by the Association, is stated as 986, of which 311 died, or 31. 5 per cent. Of the whole number 106 were natives of Charles ton, of these only 12 died, or about 11 per cent.

The number of native children attacked during the present epidemic was large, and they were seized much earlier than usual in the course of the epidemic. The relative mortality of children was greater, during the present epidemic, than in '54, but less than in some other years. Thus, of white child-

ren under 14 years, in '58, the deaths were 11 per cent of the whole mortality, while in '54 it was only 5-8 per cent; in 1817 the relative mortality of native children was 15 per cent, and in 1827, it was as high as 15½ per cent.; in 1824, it was 14 pr. cent; in 1819, it was only 6 per cent, while in '38 it was as low as 4½. Thus, as compared with other seasons of severe epidemics, in regard to its fatality in native children, '58, ranks above '54, '19 and '38, but falls below '17, '24 and 27.

A marked feature of the epidemic of this year was its proclivity to attack the coloured and black population. This tendency was somewhat developed in '54, but acquired a great increase during the present season, and the deaths among this class of the population, was more than double that of '54. The mortality was very small, however, in proportion to the number attacked.

Some few instances occurred of individuals being attacked during the present season, who had previously had the disease, indeed the second death of the present year was that of an individual who had been one of the earliest cases of '56, and had then been ill with black vomit and hemorrhage. But these cases of second attack were too few to invalidate the generally received rule, that a first attack secures immunity from a second.

The general symptoms presented by the epidemic offered no such marked or striking peculiarities as to call for especial comment here. It may be stated, however, that black vomit, hemorrhage, and collapse were not as frequent as in '54.

It only remains to say a few words on the general mortality of the two years under review, as compared with each other, and with the two years preceding them.

From the first December, 1856, to 30th November, 1857, the whole number of deaths amounted to 1,238, 39.3 per cent of which were white, 60.7 per cent black. Of the whole number of deaths, 18.5 per cent were from zymotic diseases; 20.9 per cent from diseases of the nervous system; 19 per cent from diseases of the respiratory organs; 9.4 per cent from diseases of the digestive organs, while from old age, and diseases classed as of uncertain or general seat, the proportion of deaths is as high as 20 per cent. Comparing the two

classes of the population, the relative prrportion of deaths among the whites from zymotic diseases is 20 per cent; of the blacks only 17 per cent. From diseases of the nervous system 20 per cent is the relative proportion of the whites, 21 per cent of the blacks. From diseases of the organs of respiration, 20 per cent is the proportion of whites, 18.6 per cent that of the blacks. 10 per cent of the deaths among whites were from diseases of the disgestive organs, only 8 per cent from the same causes among the blacks.

From the 1st December, 1857, to 30th November, 1858, the deaths amounted to 1925. This is the largest absolute mortality that has ever occurred in the city. In 1854, the deaths for the same period were 1902. Comparing these two years, the mortality from other diseases than yellow fever has been less in 1858 than 1854. Subtracting the deaths by yellow fever (715) in 1858 from the general mortality, it leaves 1,210 as the number of deaths from other causes. In 1854, the deaths by yellow fever were 627; subtracting these from the total number of deaths, and it gives a larger mortality—1275, as resulting from other causes.

In 1858, the proportion which the deaths from zymotic diseases bore to the general mortality was extremely large, being 48.5 per cent; a higher relative mortality than in '54, the proportion of deaths in that year from zymotic causes being 46.8 per cent. In 1857, as appears above, the greatest mortality occurred among the coloured and black population; in '58, the figures are entirely reversed, 65 per cent of the deaths were white, 35 per cent only blacks. From other than zymotic causes, the relative proportion of deaths by other diseases during the year was small. Diseases of the nervous system offered only the proportion of 11.7 per cent to the general mortality; diseases of the respiratory organs, 12 per cent, and diseases of the digestive system, 7 per cent.

In comparing the 5 years, from 1854 to 1858, inclusive, two of which were years of violent epidemic visitations of yellow fever, if the deaths by the epidemic are excluded, with the exception of one year, viz, 1858, there is a remarkable uniformity in the number of deaths. Thus, in 1854, the deaths

from all causes, excluding yellow fever, were 1,275; in 1855, when no cases of yellow fever occurred at all, the mortality was reduced to 1,088; in 1856, a year of moderate visitation, the deaths rose to 1,216; in 1857, when only a few cases of yellow fever were reported, the deaths were 1,224; in 1858, a second year of severe epidemic, the deaths were 1,210. Future investigation may probably show that it may be established as a rule, that during seasons of epidemic yellow fever, other diseases are more violent, and less amenable to treatment than in other years, when the atmospheric condition is more favorable to life.

ART. II.—*Extract from M. Clerc's Lectures on Venereal Diseases.* By our Paris Correspondent, J. K. KANE, M.D., of Philadelphia. No. 3.

That the inoculation of a healthy individual with chancrous pus, will be followed by the developement of a chancre of the same variety, as that from which the pus was procured, may be regarded as a fact established by actual experiment. This cannot be said however, of M. Clerc's Third proposition that the soft chancre like the varioloid, is a hybrid disease, owing its origin to the renewed exposure to contagion of an already tainted constitution. It must, as yet be considered only as a specious theory, imperfectly sustained by proof. It would consume more time than the general reader should perhaps, be expected to devote to the subject, for me to mention all the arguments which he brings to its support. A favorite train of reasoning with him is the following: Small pox is always more severe in its characteristics when it appears for the first time among a people—the hybrid varioloid is not seen until the parent disease has existed for some time. The longer small pox has existed in a communi-

ty, the milder is its form and the greater the frequency with which we observe its hybrid. So syphilis, which on its first appearance in Europe in 1495, was of appalling malignity has in succeeding years materially lessened in violence. The simple chancre with its single suppurating bubo, is now the most common form of the disease, yet it is not until after the time of Nicholas Massa, 1532, that we find it mentioned for the first time. I leave, however, M. Clerc's theory, which, though ingenious, is of little practical importance to the physician, and devote my remaining pages to some points of diagnosis and treatment, of whose truth and importance I myself was a witness, while following the clinic of that acute observer. First, with regard to the diagnosis of infected chancre.

It is a strange fact, that of all the authors who have written on the subject of constitutional syphilis, not one has given a correct description of *the infectant chancre.* Even M. Ricord, who has described so perfectly the simple chancre, has failed to depict "the infectant one, its sister."

The indurated chancre of M. Ricord, is sufficiently to the life; but it is a picture taken at too advanced a period, and resembles the infectant chancre in the commencement of its career, as the portrait of an adult, may that of the same individual in his boyhood. It is not until the chancre has lasted for weeks, that you see the craterine or cup-shaped ulcer, surrounded by the well defined induration so accurately described by M. Ricord. On the contrary, when ulceration does take place in an infectant chancre, it is always at the expense of the induration, and by M. Ricord's own showing, the induration does not supervene until after the fourth day of the chancre's duration. The general tendency of infectant chancres, moreover, is to heal, a large majority of them getting well without proceeding to ulceration; so that the cup-shaped cavity of M. Ricord, far from being the rule, is a comparatively rare exception and one which is *never* seen during the earlier stages of the chancre. It is easier however to find fault with the categorical descriptions of others than to

originate better ones, and after all these strictures upon *Ricord*, I shall not risk offering a definition or description of my own, for future criticism. I prefer to follow the example set by this very Monsieur Clerc on such occasions, and offering his favorite excuse, that "the jewel loses its lustre when deprived of its setting," beg the reader to place himself face to face with an imaginary patient. A patient presents himself then, with several indolent buboes of the groin, which cause him but little pain. When you ask him if he has a chancre, the chances are ten to one he will tell you, no. Question him more closely, and he will say that he has slight excoriation on his penis, but that he is sure it cannot be a chancre, for it does not pain him at all.

Turn back the prepuce and on the delicate mucous membrane which lines it, you will see what appears to be a simple abrasion of the skin, such as might be produced by too hard friction with a coarse towel, resting upon a hard resisting base, whose limits are perfectly well defined in relation to the surrounding tissues. You can seize and roll it between your fingers, to which it imparts the sensation of a piece of hard cartilage inserted beneath the skin. You can see the edges of the abrasion. They are of a slightly reddish hue, but the centre is covered by a smooth grayish *plaque*, slightly raised above the surrounding level, closely resembling the exudation membrane of apthæ, though more gray, from whose surface exudes a muco-purulent liquid. This excoriation is the type of infectant chancre in its highest degree of perfection. The grayish plaque is the characteristic false membrane, and the liquid it secrets is the true chancrous inoculable pus. At a later stage, one or two things may occur; either the excoriation may heal and the indurated base be absorbed, or ulceration may take place at the expense of the induration, in which case we shall see the cup-shaped cavity of M. Ricord. But it is not always that the infected chancre is so easily recognizable. It not unfrequently happens that one or the other of its leading features is ill defined or even wanting, and we are obliged to have recourse

to other signs, in order to make out our diagnosis. The character of the buboes, &c., will in general aid us, but at times cases will present themselves in which it would puzzle the most experienced practitioner to say at first sight, whether the accidents are of an infectant character or not. Cases of multiple infectant chancers of phagedenic form, accompanied by a single suppurating bubo are rare; but they do occur sometimes, and it is in such cases that inoculation becomes of inestimable value as an aid to diagnosis. As a general rule, after an infectant chancre has existed for some days, it loses its power of reproducing itself on the patient, whereas the simple chancre retains its reproductive properties, almost to the end of its existence.

M. Ricord having observed this fact, has inferred that the pus of the infectant chancre is less virulent and loses its inoculative force more readily than that of the non-infectant variety. Is it not rather owning to the fact of the patients becoming syphilized, and hence incapable of receiving a fresh impression? If, eight days after the vaccination of a child, we re-vaccinate it with pus taken from the vaccine vessicle, already existing on its arm, we will have a negative result, yet no one pretends that the vaccine matter has lost its power to affect unvaccinated subjects. M. Ricord's inocculations were made on persons already affected with chancre, and whose constitutions, consequently were already infected. Of course he obtained negative results, and he would have obtained them just the same, had he experimented in the same manner on patients affected by small pox, varioloid, vaccine, or any other virus which is capable of reproducing itself by inoculation.

From the consideration of this subject, we are led naturally to that of M. Ricord's favorite treatment for the prevention of constitutional disease by the cauterisation of the original chancre. Can we by destroying the chancre kill the syphilis? M. Clerc thinks not, and I cannot reply to the question in better language than his own, as I find it on my notes of his clinic, "If," he says, "a chancre is infectant, it is infectant from its commencement and no cauterisation can

prevent the syphilis already in the blood from showing itself. Nobody doubts now-a-days that the infectant power of chancrous pus is due to a virus which it contains. M. Ricord himself has been and is still one of the most ardent promulgators of the fact "that the syphilitic poison is a virus, as much so as the virus of small pox or vaccine." If this be the case, is it logical to suppose that this particular virus acts differently from every other? If we vaccinate a child to-day and the day after to-morrow we cut out with a bistoury, or burn out with a cautery the little puncture we have made, will the child be the less vaccinated? No! Over and over again have re-vaccinations been made under such circumstances, and the results have always been either entirely negative, or at most the production of a false vaccine, as though the original button had not been interfered with in its course. The same holds good of variola, varioloid and every other inoculable virus.

Why then make an exception in favor of the syphilitic virus? There is no such exception. If a man be inoculated with the pus of an infectant chancre, whether naturally by the sexual coitus or artificially by the lancet, his constitutional injection dates from the moment of the inoculation. M. Ricord is very positive in his assertions to the contrary; but I defy him to show a single positive case in which a real infectant chancre having existed, he has prevented constitutional symptoms by burning it out. M. Ricord gives gross results, but not particular cases. He simply says that out of so many chancres cauterised before the fifth day at the Hopital du Midi, none were followed by constitutional syphilis.

This sounds fine, but we must not trust too blindly if we would not be led into error. I would rather hear of a few by name than a greater number nameless. In the first place, How does M. Ricord know that none of these cases of cauterised chancre were followed by constitutional symptoms? If we look to his Hospital register, we find his cases discharged after one, two and three months residence. May not M. Ricord be considered over hasty in pronouncing a

decree of cured absolutely on such cases, merely because no symptoms of constitutional syphilis had yet appeared? Has not he himself avowed, that the period during which constitutional symptoms may appear, can be limited only by the life of the patient? It is time that owing to the well known fact that non-infectant chancres are much more frequent than those of the infectant variety, a large majority of his formidable numbers would not have had constitutional symptoms even without their timely cauterisation. Besides it is but fair to suppose that some at least of the remaining cases were endowed with constitutions naturally *refractory* to *syphilis.* Of the public prostitutes who come to be examined at the prefecture of police, and of whom accurate registers are kept, there are many who have never had constitutional syphilis. I know and examine every week, women who have been exposed to contagion daily, I might almost say hourly, during the last twelve years, and who in all that time have never had the slightest symptom of constitutional syphilis. We cannot account for such a fact as this, otherwise, than by supposing them to be constitutionally refractory to the influence of the virus. Were it not so, it would be manifestly impossible for them to escape, for on *Saint's* days some of these women have connection with as many as thirty different men in the course of the twenty-four hours! Judge then, if after such excesses, the mucous membrane of the vagina must not be sufficiently excoriated to allow of the absoption of syphilitic pus should any come in contact with it.

Yet these creatures have absolutely contaminated their lovers by mediate contagion, without incurring themselves the slightest accident.

Last of all comes the painful fact, that cases have occurred even at the Hôpital du Midi, in which constitutional symptoms have persisted in presenting themselves in spite of the infallible burning. How then does M. Ricord extricate himself from his embarrassment? When such cases occur—when constitutional symptoms do appear and there is no blinding ones eyes to them, M. Ricord will say, not that the cautery

has failed, but that the chancre was more than five days old; it had existed previously without the knowledge of the patient; the induration never well marked, had been absorbed etc., etc., etc. Thus leaving the matter open to doubt. How then are we to know the truth? A single perfectly authenticated instance of failure, on the part of the cautery in a case of chancre, certainly less than five days old would settle the matter. Happily we have not only one, but several cases in which all the conditions are so clearly ascertained, as to leave no room for doubt. They are the cases of medical men who have been bold enough to inoculate themselves with chancrous pus, and in whom constitutional syphilis has supervened in spite of cauterisation.

Alas! I know personally, quite a number of such cases among the enthusiasts of Dr. Clerc's investigating circle. Assuming that their disease was contracted in the pursuit of science, I may close with the passing tribute of a *French* tear to these youths.

"Qui meurent tristes victimes
Sans q'un frere sauve vive par leur trepas."

ART. III.—*Anatomy, Physiology and Pathology of the Supra Renal Capsules.* BY J. THOMPSON DARBY, of St. Matthews, So. Ca. Presented as a Thesis for graduation at the University of Pennsylvania, and read before the Biological Department of the Academy of Natural Sciences, April 19th, 1858.

Of the numerous organs composing the human organism, none have met with such neglect as the Supra Renal Capsules.

The earliest writers of Medicine, from disadvantages of necroscopic observations, were only able to give a sciolous account of these organs, and for this reason, for many ages, all knowledge of these glands was vague. Even after the prejudices of nations had passed concerning the study of Anatomy, it appears that those who followed directed their attention to other organs of more vital import in the economy. Thus we perceive in the old works little more than visionary speculations advanced relative to the functions of these organs.

In works of more modern date, little more is said, and the ideas given, equally speculative in many respects, appear to be based upon antecedent opinions. It has been only at a very recent period, that the now innovating spirit of science has thrown aside these older ideas, and in bringing forth new endeavors to show that these glands hitherto neglected, occupy no insignificant position among the many which compose the organism.

Our science is now too far advanced to leave aught untried. The anatomist, physiologist and pathologist are now on the alert, and with their handmaids, chemistry, the microscope, and scalpel may soon reveal the functions of the bodies.

The influence exerted by these organs, in the opinion of the ancients, is interesting for its peculiarity. Thus Hippocrates, Galen, Aetius, Aretius, Rufus of Ephesus, and others, considered that there was a thick black acrid fluid or humor, which was secreted by certain glands, productive of hypo-

chondriasis, mania, and similar diseases. Among these, it was the opinion of some that this humor was generated by the Supra Renal Capsules, and with others by the pancreas.

It appears that much later, Caspar Bartholinus followed the similar opinion, for the name atrabiliary was given to these organs, as furnishing this black bile, or atrabilia.

This opinion was for many years entertained by his successors. At a later date, and even at the present time, some believe that these bodies are concerned in lymphosis, and others think an important part in fœtal life is exerted by them. Various opinions have, in later years, been given by Krause, Ecker, Nagel, Kölliker, Wagner, Frey Müller, Cuvier, and others. More recently Addison, Hutchinson, Sequard, Gratiolet, Andral, Trousseau and Taylor. It is due to Dr. Addison, however, that so much attention is now drawn to this subject, as he first pointed out, in a publication, in 1855, the pathalogical condition of several cases, pathognomic of Supra Renal Capsular disease.

It is my object from these many opinions to record facts, and bring forward the most plausible theories resulting from investigations in anatomy, physiology and pathology.

First, the anatomy of these parts will be given, called by anatomists, Glandular Supra-Renales, seu Renes Succenturiati, seu Atrabilariae, Supra-Renal Capsules, consisting of two bodies generally in the vertebrata, and altogether absent in the invertebrata; classed with the spleen, thyroid, and thymus, or ductless glands. They have been also called the "vascular glands," on account of being so rich in blood-vessels. This unmeaning term should no longer be adopted particularly in respect to the Supra-Renal Capsules, as it is now known that the blood-vessels are not more abundant than the nerves and ganlia which abound in these bodies.

The use of these bodies being as yet not understood, the classification is difficult, and hence the term, "anonymous glands" (Leidy) is better suited. In mammalia these bodies resemble each other in structure, (Frey.) They differ, however, in number, form, size, and situation. Thus, in man, the left Capsule is larger than the right, higher up, longer, and

more oblong in shape. At times on the inner edge or imbedded in the surface, are small round bodies, the so called Accessory-Capsules.

(Martini mentions that in a dissection he found no Capsules, and the kidneys united into one body situated at the promontory of the sacrum.)

This difference in form, size and situation, is more variable in the lower order of vertebrata. In the mammalia, the situation is usually constant. In the rodentia, they are quite large. In the Guinea pig, (Cavia Cobaia) the Capsules are triangularly pyramidal, and bear the ratio in weight to the kidney of 1: 3.2. In the rabbit they are flattened ovoids, and are in weight to the kidneys, as 1: 49.8. Among the carnivora, those of the dog are cylindrical, thick and dense in structure. In the cat somewhat flattened ovals. Among the pachydermata, the pig's are found to be in form prismoidal; in the horse, the shape is that of man. The ruminantia present other forms as in the sheep, they are cylindroid; in the ox, crescentic. These examples, with others, will show that the form and size vary. In the negress they are larger than in the white, and negro.* (Meckel.)

In the monkey, the resemblance to those of man is very close, though larger, (Cuvier.) The weight of the capsule in the seal is to the kidney as 1: 150. (Cuvier.)

The elephant has elongated capsules, conoidal, divided into two lobes, according to the same naturalist. Their size in birds is small in comparison to the weight of body, varying much in shape; in cetaceans the same is the case; in insectivoria as the mole, they are found to be triangular. (Frey.)

Little is known concerning these bodies in reptilia. Ecker and Nagel found small, long, lobulated capsules in the sauriaus. In the ophidia the capsules are lobulated and highly vascular. (Retzius.) Among fishes, Retzius and Ecker agree that the capsules vary in size, situation and form, more than the higher order of vertebrata. Frey corroborates this state-

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* I have not found this difference.

ment, and has found in the pike several capsules surrounding the kidney.

In man, the capsules are two triangular bodies, fitting on the superior portion of the kidneys, and resembling a cocked hat. Both look inwards towards the spinal column; the right, opposite the middle of the tenth dorsal vertebra, in contact with the vena cava; the left, opposite the cartilage, between the ninth and tenth dorsal vertebræ, and touching the aorta. The right has on its anterior surface the liver, its posterior surface the diaphragm; the left has on its anterior surface the pancreas and spleen, its posterior surface is in contact with the diaphragm. Their inner borders are in relation to the splanchnic nerve, and semilunar ganglia. The inferior border is convex, thin, looking inwards and backwards. The base rests upon the kidney, on its superior, inner part is concave, having a groove for the passage of vessels. These bodies are surrounded with fat, and covered by a fibrous investment, which adheres closely to the proper substance of the organ, and entering, forms furrows on the anterior and superior surface. It is united to the kidney by loose connective tissue. In the interior of the organ is found a space, in shape resembling the organ, filled with a black, dirty red, pultaceous mass, the result of cadaverous decomposition or degeneration from disease. The capsule on the right kidney measures from an inch to an inch and a half in length; in height, from three-fourths to an inch, and in thickness from two to three lines. The left capsule measures in length from an inch and a fourth to an inch and three-fourths; in height from half an inch to three-fourths, and in thickness from three to four lines. I found the weight in several, was from one drachm to two drachms and a half, varying with the age. The capsule is composed of two structures, a cortical and a medullary. The cortical is easily separable from the medullary, and presents two portions, an external yellowish red, and an internal brown border. It is readily torn, and to the eye appears striated. The microscope has revealed this striation to consist of elongated cells, one above the other, with longitudinal, transverse and oblique stroma of connective tissue, thus forming com-

partments of larger or smaller dimensions. These compartments, filled with granular substance, are again, by delicate dissepiments made up into sub-compartments, called by Ecker *gland follicles*, surrounded by structureless membrane, and having within granules, cells and nuclei.

Kölliker does not corroborate this statement, upon which Frey appears to agree with Ecker, but states that he has only found in the *cortical cylinders* (as called by him,) angular cells, isolated from the sub-compartments by alkalies or pressure, free from structureless membrane or ovisac, as stated by Ecker, but having the same connective tissue, which encloses the compartments, from which the cell was expressed. He says that in the minor portion of the cortical layer, he has found *follicles*, which he considers *enlarged cells*, containing no nuclei, or granules, but filled with oil. The cells he states contain nitrogenous granules, with fat granules in the external cortical portion; in the brown internal portion of the cortex, the cells are filled with pigment granules. This difference of the contents in cells composing the external and internal cortical portion, accounts for the color of each. The medullary portion is found to vary. In those examined by myself, some presented a dark venous appearance, in others it is of a lighter hue, and with more or less consistence. In persons killed by accident, this portion is grayish white (Wilks); in those long dead, it is more soft and of a reddish brown or black color. The components of this portion are supposed by some to be similar to the cortical. It is more clear under the microscope, containing fewer fat and pigment granules; trabeculæ from the connective tissue pervade it and divide it into minute compartments. The cells within contain a nucleus, with nucleoli, and resemble the nerve cells, (Kölliker). As yet no excretory duct has been found.

Vessels.—Three arteries supply these bodies generally; the arteria supra-renalis superior is from the inferior phrenic; it is often absent. The arteria supra-renalis media, from the aorta, is the largest, and is, as examinations prove, constant. The arteria supra-renalis inferior, is from the renal

and is seldom absent. These arteries take two courses after reaching the capsules. On the surface you can see distributed as many as twenty, (Kölliker,) and of these, some after coming along the cortical surface send down twigs which ramify into the dissepiments made by stroma of connective tissue, thus surrounding the "cortical cylinders" with a capillary coat; the others run immediately at right angles to the medullary portion, where, forming minute ramifications, they return again to the cortical portion according to Nagel and Frey, and these, anastomosing with those which ramify in the cortical portion, cause the medullary substance to be less vascular than the cortical.

Veins.—These vessels are supposed to take origin in the central portion of the medullary substance, coursing to the surface, uniting in one larger vein, running in the fossa at the base of the capsule, to empty, on the right side, into the vena cava, and on the left, into the emulgent vein. This is not always the case, though stated by Nagel as such; for in the capsules examined, often and generally it may be said, two or three veins run off from the capsule corresponding in their course to the arteries supplying the organ. *Lymphatics.*—"These vessels are large and numerous, and terminate in the lumbar glands (Wilson)." This statement is not correct, for in fact little is known concerning them. Arnold asserts that he found superficial and deep seated ones. Ecker denies this, by saying that he only found the superficial, and Kölliker agrees that these even are few, having seen in his examinations a few small trunks.

Nerves.—No organs known are so liberally supplied with nerves; as many as thirty-three trunks, (Kölliker) have been counted. They come from the cœliac and renal plexuses, and the semilunar ganglia; and from the pneumogastric, and phrenic, according to Bergmann. Connected with the sympathetic they have ganglia especially apparent on the inner and lower border of the organ, (Kölliker). They can be traced no farther than the medullary portion, and appear to belong entirely to that portion.

2d. The physiology of these organs is involved in so many

hypotheses, that it is with hesitation that any one could select any one opinion as descriptive of their true function.

Pathology in the last few years, has to a certain extent opened for the physiologist, a way in which he does not grope so blindly as those who wrote prior to the monograph of Addison. Physiological facts have been also deduced from many vivisections, performed especially among the French physiologists, principally Messieurs Sequard, Gratiolet, and Phillipeaux. There has been much dispute concerning the development of these organs; some asserting that they are parts of the Wolffian bodies, while others deny this statement, and state that they come from a separate blastema formed from the germinal lamella. These bodies to the tenth week are much larger than the kidneys. Gradually, from that time, they diminish, until becoming equal in size to the kidneys, they remain in this ratio to the third month; (Kölliker,) decreasing from this time, they are found at the sixth month to be in ratio to the kidneys as 2: 5, in the mature embryo, 1 : 8 (Meckel), as found by myself 1 : 10, and in the adult varying from 1 : 16 to 40. In those examined, the diminution noticed by some at the period of very old age, has not been so perceptible, no greater variations occurring between the adult and aged, than between adults.

In mammalia this greater proportion in embryonic life between the kidneys and these organs, does not exist, the ratio being the same at all periods, (Frey). It has been supposed from the large size in the embryonic state in man, that these glands exerted an important influence in the formative process of the embryo, but according to Frey the fact of the "gland vesicles" being unformed, when in this enlarged condition, and only being fully matured after the tenth week, when they are gradually becoming equal in size to the kidney, while the organ has remained at rest and is diminished in ratio to the weight of the kidney, would seem to contradict this supposition. Ecker gives an opinion that the supra-renal capsules and other "vascular glands," take from the blood a part, which is afterwards secreted by these glands in a modified condition, rich in protein and fat, to be

again taken up by the circulation for the benefit of the nutritive process. Frey disagrees in this opinion, but Carpenter appears to follow Ecker. It is reasonable to suppose with Carpenter that the blood conveyed to these organs, undergoes some change for the purposes of the economy. As the *anonymous glands* have no excretory duct to convey their secretions to particular organs, it is again taken up by the blood and carried through the systemic circulation. The spleen* is an exception among the so-called "vascular glands," as it empties its blood into the portal vein, to be distributed through the liver, before entering the vena cava. The blood entering the *anonymous glands* passes out in a purified condition, into the systemic circulation, passing only through the depurating organs, the lungs. These organs cannot exert any peculiar influence upon these changed products in their passage, nor can these products afford any pabulum for respiration on account of the small amount of fat contained in these organs during their functional activity; (Carpenter) so it must be for nutritive processes either of the system or of the blood itself. From the size of the *anonymous glands* in early life, when there is greatest need for the nutrive functions, we should infer that they played an important part in this process when these glands are greatest. The Thyroid and Thymus diminishing, when the call for nutritive material for formative processes is less, would go to show that they do exert this nutritive influence, as in the case of over-driven lambs, the diminution of the thymus is great, but plentiful nutriment being afforded, it is gradually re-distended to the natural size, (Carpenter).

The supra-renal capsules do not diminish in this formative period, but still hold their influence, and for this reason, act more as modifiers of the blood itself, but less than the thyroid and thymus in assimilating albuminoid plasma for formative operations.

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* The spleen should no longer be classed with the supra-renal, thymus, and thyroid glands, or anonymous glands.

Cuvier supposed that the functions of the capsules and kidneys were the same, but the difference in structure and tissues, together with the absence of an excretory duct, and the quantity of urine being normal when these organs are ablated in lower animals, would be sufficient to show that the physiological distinction is marked and dissimilar. So also in the hypothesis of Meckel, who supposed that they were intimately connected with the sexual function; we have never seen any communication with the organs of generation by absorbents or other modes, to serve to draw a conclusion that they act in this particular, any more than the kidneys. On this point, Nagel has proved that there exists no such relation. In a mechanical point of view, the gravid uterus may exert some influence on these bodies, by pressure, thus, by congesting these organs as is the case with the kidneys in gestation, preventing their functional activity in modifying the blood, occasioning a deposit of pigment in different portions of the body in pregnant women.*

In the embryo of three months as found by Ecker and Kölliker, there exists a cortical and medullary portion, with cells and fibres, colossal nuclei, with nucleoli, fat molecules, but no nerves. These molecules resist the strongest chemical reagents. Kölliker regards the cortical and medullary portions as physiologically distinct.

The cortical he says, may be classed with the so-called blood vascular glands, but the medullary, as an apparatus of the nervous system, in which the cellular element and nervous plexus exert a similar action, as they do in the grey nerve substance, or as entirely distinct.

In researches made by Leydig on the supra-renals of mam-

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* Dr. Taylor of New York, originated this view, but denies that it is from mechanical pressure, as the kidney becomes congested even in the first week or at two months, when congestion of these organs could not take place from pressure of the uterus. The kidneys becoming congested, would, from close proximity, cause the congestion of the capsules. So to my mind, it is mechanical and not from any peculiar relation existing between the functions of the two organs.—*Vide, N. Y. Journal of Medicine, September*, 1856.

malia and amphibia, two different kinds of cells were found; those composing the cortical portion in mammalia, corresponding to the yellow granular, striped portion found in fishes and amphibia, whilst the medullary portion in the mammalian organ corresponds to the ganglionic cells in the sympathetic ganglia. These views of Kölliker and Leydig, serve to strengthen the statements made first by Bergmann, of the connection between these organs and the nervous system. He thought that they bore the same relation to the ganglia of the sympathetic system, as the pineal body does to the brain. It is true that these bodies are subject to calcareous deposits, as found in the pineal body. In brainless monsters also, there is generally an absence of the supra-renal capsules, going to show a relation between these organs and the nervous system.

Moreover, in wounds of the spinal column, these organs are congested, and become hypertrophied as found in animals. The experiments made upon animals, will also prove that such a relation does exist. In animals from which the capsules have been removed, changes take place in the economy, showing the important part exerted upon the nervous and vascular systems. In experiments performed upon ten animals, it was found that the sensibility was increased, when one or both capsules were removed; temperature of body was diminished; great debility, independent of that caused by the operation; digestion ceased; the appetite was lost or greatly diminished; respiration slower previous to death, but temporarily increased immediately after the operation; secretion from the kidneys remained normal; convulsions of the tetanic and epileptic character in the last stages, prior to death, and the animals died asphyxiated or from syncope; a rolling movement resembling that seen when the encephalon is wounded, and contraction of the pupil.* These effects exhibited by ablation of the cap-

* I have noticed that this rolling movement is towards the side, opposite to that from which the capsule was ablated.

sules, on animal or organic life, have been to a greater extent and more minutely drawn out, by Brown Sequard. In 90 experiments made by him, the mean period of survival in 51 rabbits, was a little over 9 hours, the minimum 5½, and the maximum 14½. In grown dogs and cats, the mean was 14 hours, and in guinea pigs, the same.

The ablation of both capsules causes death sooner than one, and adult animals die sooner than those very young. The removal of one does not always kill, as found by Sequard, though the proportion is small, 2 surviving out of 37.

Phillipeaux asserts that death does not take place in the albino rat, when both capsules are removed. Gratiolet from experiments on young animals, found the mean duration of life longer than Sequard. The duration of life in experiments of my own, coincides with Sequard, but in young animals the period of survival is longer, extending, as I have seen in a kitten, to 36 hours, and in a guinea pig over 31.* Gratiolet thinks death takes place from severity of the operations, not from the influence exerted on the organism by these organs. To prove whether ablation, or the operation caused these symptoms enumerated, and the speedy death, the following experiments were performed.

Similar incisions were made upon a guinea pig, rabbit and dog, as are made for extracting both capsules; my thumb and fore-finger were then introduced into the wound, and brought in proximity to the organs. In no one of the three cases were symptoms produced as when these bodies are removed, but the appetite was good; the sensibility only increased in those parts contiguous to the wound; the temperature remained normal, etc.

The dog on the eighth day had sufficiently recovered to get away; the pig died on the 13th day, and, as a post-mortem revealed, from lesion of one of the lobes of the liver;†

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*In my own experiments, death followed sooner the removal of the *right* than the left capsule.

† The liver is beautifully divided into 5 lobes in this animal, one of which only was affected.

and the rabbit, after becoming perfectly tame from my feeding him every morning for 21 days, died from the effects of a cold night.

Brown Sequard's experiments, together with these, have proved to my own mind, that peritonitis, lesions of organs, phlebitis or hemorrhage, are not the cause of the rapid death seen in animals from whom these organs have been ablated. If then these be not the causes, the inference is drawn that ablation of the capsules must be.

The blood is much modified as found by Sequard; for the pathological condition of pigmentary deposit in the skin of persons coexistent with disease of these organs, as found by Addison, has been verified by Sequard, who found the pigment cells much increased in the blood of animals operated upon. He has procured also crystals spontaneously formed differing in character from hematoidine.

In the skin of animals there was no abnormal increase of pigment, on account of the speedy death, but in two cases of those experimented upon by myself, there were present on the peritoneal coat of the stomach, black spots resembling those described by Addison as occurring on the mesenteric and omental portions of the peritoneum of persons dying from disease of these bodies. These experimental facts, with pathological conditions now known, go to support each other strongly as to the influence exerted on the nervous and vascular systems. The marked debility, want of voluntary motion with other enumerated effects, produced by ablation of these organs, resemble the disorded nervous condition of those cases, reported, where pathological conditions of these organs prevent their proper functions.

In addition to this, the vascular system is deranged, as seen in the abundant deposit of pigment in different portions of the body of those suffering from disease of these organs; and in animals, though the deposit is not increased in the skin, from their rapid death, yet an abundant supply of pigment cells is found in the blood. The spontaneous production of crystals, and the fact that blood from the animals ex-

perimented upon, injected into the veins of other animals, causes death as shown by Sequard, would show that there is a change and an important one. It is seen from these facts that these organs exert such an influence on the economy as is essential to life.* The cortical, exerting probably on certain constituents of the blood a transformation, which prevents these constituents from being transformed into pigmentary matter by modifying processes undergone in the passage of the blood through them; and the medullary co-operating with the nerves, in preserving the proper influence of the nervous system.

Finally, it remains to speak of the pathology. The first notice given of the morbid influence produced by these organs was by Dr. Addison of Guy's Hospital, London. Much praise is due to him for having first directed the attention of pathologists to these organs, and whatever praise is due to others, giving accounts of these organs, they should bear "proximos illi honores."

Mr. Hutchinson of England who published an analysis of 27 cases, confirming the accounts of Addison, and Trousseau, adds testimony, by asserting that he has noticed similar appearances in the French hospitals. In our own country, Dr. Taylor of New York, has been most zealous in pursuing the pathology of these organs.

Symptoms, Cause, &c.—The disease comes on in a gradual insidious form, characterized by resemblances to an anaemic more than any other condition of system. This state of system is described as being *cachectic* rather than anaemic, by Dr. Taylor. The morbid changes vary in rapidity, persons affected notice a general falling off in health, without being able to state the cause; loss of appetite, indisposition to mental or physical exertion, without that shrivelling condition seen in other malignant anaemic diseases arising from a stru-

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* A two-fold influence, the medullary on the nervous system and the cortical on the vascular, and this influence is such as to make them essential to life.

mous diathesis, or malarial influence. This condition is masked by derangement of the constituents of the blood. There being a leucocythemic state co-existent with dull pain about the loins in the region of the kidneys, a peculiar change of skin, accompanied with greater languor, irritability of stomach, nervous and vascular derangement. Those affected, present more flabbiness of the solids than an emaciated condition, with sickly, pearly aspect of the conjunctivæ. The debility is peculiar, without great loss of flesh; there are frequent faintings, indisposition to move, palpitation of the heart from the slightest exertion. The tongue does not differ from that seen in debility. The pulse is marked by softness and compressibility, is of the normal frequency, except when exertion excites it. The urine is normal in quantity and composition. Great irritability of stomach, sinking feeling in the epigastrium, loss of appetite, with costiveness in the majority of cases reported. The cerebral functions are disturbed, dullness, listless stupid expression, indisposition to exert the mind. In the latter stages, epileptiform convulsions, coma, paralysis, loss of mind, &c., indicate depression of the nervous system.* The most striking point in diseases of these organs is a discoloration of the skin, so peculiar as to be noticed by those affected and their friends. Deposits of pigment, consisting of small irregular patches of a dirty chestnut-amber, brown color, pervading especially those parts where pigmentary matter is deposited. The absence of this matter in those parts of the body, as the matrices of the nails and the palms of the hands, is confirmatory of its being pigmentary matter. Hence, on the penis, the scrotum, the mammae, the umbilicus, face, lips, superior extremities, axillae, and groin it is seen. At times, the discoloration pervading the whole body, gives those affected the appearance of a mulatto. Mr. Hutchinson uses the term, "bronzed," as applicable to this discolored state, or that appearance which is seen

* Dr. Taylor, who kindly afforded me information on this subject, says in 20 cases, the nervous system was deranged, except in one. The extended experiments of Brown Sequard exhibited this phenomenon, and it was always present in animals experimented on by myself.

on a bronzed statue with the gloss rubbed off. Dr. Taylor agrees with Addison in saying it resembles more the mulatto, or West Indian, and says, that the bronzed condition is only seen in late stages of the disease. The integument is blanched in spots, from deficiency of coloring matter, and the abdominal viscera, particularly the mesenteric and omental portions of the peritoneum exhibit these brown spots. The course of the disease is gradual and slow. In 27 cases reported by Hutchinson, the duration varied from three weeks to three years. In proportion as the lesion of the organs is acute, so is the discoloration and cachectic condition manifested. In the cases reported by Hutchinson, the age of those affected, varied from 12 to 60 years; in 20 cases seen by Taylor, the age varied from 20 to 73 years.

Diagnosis.—The discoloration of the skin, with depositions of pigmentary matter, accompanied by systemic debility, irritability of stomach, and nervous derangement, are the guiding points; yet a conclusion is best derived from negative diagnosis. The disease must not be confounded with discoloration produced by exposure to the sun; for the skin only being discolored at those parts exposed, the hair being changed in color, the ear being discolored, and not where there is disease of the capsules, the gradual clearing up of the complexion by removal of the cause, absence of nervous derangement, of pigment on the lips, and the voluntary movements, physical and mental, being unimpeded, would prevent an incorrect diagnosis from this cause. In cases of jaundice, the matrices of the nails, and conjunctivæ are discolored, the change of color does not occur in patches, but is generally diffused, and absence of pigmentary depositions in the course of this disease, would prevent it from being mistaken for Supra-Renal disease.

In cases where muddiness, debility, and cachexia, occur from malarial, or other influences, the history of the case, the symptoms in the first stages, the absence of pigment, change of color more generally diffused, are guiding points. It is more likely to be mistaken for pityriasis versicolor, but the absence of nervous derangement, the patches being smaller,

the itching and contageousness of this affection, with presence of microscopic fungus are differences sufficient to distinguish one from the other.

Prognosis and Treatment.—Of the cases reported, none as yet have recovered ; hence the prognosis can be but most unfavorable. The treatment adopted has been of no avail, and no remedies, yet known, act even as moderators of the disease. Nutritious diet, tonics and alteratives, thus supporting the system, and producing a change in the blood, would appear to be rational. Iodide of potassium and a mercurial course have been recommended.

Morbid Anatomy.—In all cases reported, there has been lesion of one or both capsules. In every case where discoloration existed, these bodies were diseased, and in proportion to the duration of disease, and amount of discoloration, was the lesion more extensive. The cases reported by Hutchinson proved, by examination, that death occurred from abscess in acute inflammation, atrophy from fibro-calcareous deposits resembling those found in the pineal gland, hypertrophy with induration, fibrinous effusions resembling tubercle, and cancerous degeneration.

Causes.—It is not understood what is the theory of this disease, consequently the causes are conjectural. We see, however, that symptoms, as described, occur, and autopsies prove that where these symptoms occur, there exists lesion of the capsules—arising from perverted action, as in carcinomatous and scrofulous diseases; we see when these organs are diseased, the symptoms described, and the changes which take place. Liability to fibrinous effusions resembling tubercle, with fibro-calcareous deposits, go to show this to be the case. This perversion is not understood further than are the causes productive of carcinoma and tuberculosis. As has been already mentioned, the portions composing these organs being considered "physiologically distinct," it may be probable, that as lesions affect the cortical or medullary portion, so do symptoms arise pathologically distinct. As, however, in all cases examined, both portions have been diseased, a combination of the effects exerted on the system by both, is

shown. The medullary portion composed of nerve ganglia, liable to degeneration, often of the calcareous kind as the pineal gland, would cause from intimate relation to the nervous system, those phenomena of nervous derangement exhibited. The cortical portion belonging to the vascular system, (probably exerting a modifying influence on certain components of the blood which go to form pigmentary matter,) from perverted action, being unable to enact this normal influence, becomes more irritated from accumulation of pigment, and thus causes increasing masses of pigmentary matter, which acting as barriers, prevent circulation in the capillaries supplying the nerve centres, and produce the disease.

ART. IV.—*A Lecture on the Prevention of Post-partum Hæmorrhage.* One of a course delivered in the University Medical College, N. Y. By T. GAILLARD THOMAS, M.D., Lecturer on Obstetrics; Secretary of the New York Academy of Medicine: Physician to St. Lukes Hospital.

Gentlemen: Before entering upon the all important subject of Post-partum Hæmorrhage, I will beg your attention for the investigation of the means of preventing its occurence, which, in my mind, demands your earnest reflection and study quite as imperatively as the treatment of the accident itself. The imminent danger, awful responsibility, and manifest necessity for knowledge, developed by the occurrence of Post-partum Hæmorrhage, have arrested the minds of teachers and writers so prominently on its treatment as to have led, I think, to a laxity of emphasis with regard to those precautions, which, if thoroughly instilled into the minds of practitioners, would in a great degree diminish their experience in that complication of labour. I have often observed, too, that students of medicine about to take upon themselves the duties of physicians, could stand excellent examinations upon

the treatment of hæmorrhage, while they would be entirely ignorant as to the means which would render the application of that knowlege unnecessary, and going into practice prepared, as they supposed, to cope with this formidable adversary, they have appeared to court rather than avoid its approach. Bear in mind that, if any where that trite house-wife's adage with regard to the superior claims of prevention over those of cure, is applicable, it is tenfold more so here; and never lose sight of the fact that a flooding, once fully under way, is not, by any means, always susceptible of relief; you will find cases which will baffle your best directed efforts, and others where death will slowly but surely track and overtake your patient, even after you have overcome the immediate danger.

The frequency of Post-partum Hæmorrhage is, in great degree, due to a neglect of measures for preventing its occurrence. In the truth of this proposition, I feel sure that every logical mind will agree, after examing the statistics on the subject, and to assist you in doing so I cannot do better than to place before you, at a glance, the statements of the most reliable authorities.

Dr. J. Clarke	met with	post-partum	hæm.	in	1	out	of	ev'y	1038	cases
" Merriman	"	"	"	"	" 1	"	"	"	589	"
" Collins,	"	"	"	"	" 1	"	"	"	153	"
" Cusach	"	"	"	"	" 1	"	"	"	140	"
" Lever	"	"	"	"	" 1	"	"	"	133	"
" Reid	"	"	"	"	" 1	"	"	"	120	"
" Churchill	"	"	"	"	" 1	"	"	"	78	"
" Von Siebold	"	"	"	"	" 1	"	"	"	121	"

It will be here perceived that while Dr. Clarke met with the accident in less than 1 out of 1000 cases, Dr. Churchill, practising in the same city, and therefore under the same climatic, hygienic, and national conditions, saw it in 1 out of 78, and that while Dr. Merriman's patients suffered from it in 1 out of every 589 labors; those of Dr. Reid were exposed to its dangers in 1 out of every 120. Now it may be said that this vast and startling difference was merely the result of accident, but would such a conclusion be rational or logical? A slight difference, or even a difference of two-thirds,

might be thus accounted for, but when, as we see here, one practitioner meets with it ten times as often as another, what conclusion can be drawn but that it was due to some tangible and efficient cause?

The same difference will be observable, I think, in the results of the experience of practitioners with whom you will meet in your professional career—some will tell you that Post-Partum Hæmorrhage is the bane of their obstetrical practice, while others will assure you, truly, that they have never seen a case of any obstinacy or violence. In hospital practice, I have seen two violent cases, in private practice two, and in cases attended by students of this college, three or four, and I am free to confess that in my opinion, every one (not excepting one which occurred to myself, and in which the patient narrowly escaped with life,) could be traced to negligence on the part of the practitioner, with regard to the precautions which should, have been taken to prevent the accident. Do not understand me that all cases of Post-partum Hæmorrhage can be prevented; such is not my belief, but I deem it of importance to impress upon you what I feel to be a fact, that with the preventive means about to be recapitulated, you will meet with it as rarely, perhaps, as 1 in 1000 cases, while without them you may have to deal with it as often as 1 in 78.

But it will be impossible for you to get a proper appreciation of this subject without an investigation into the *causes of Post-partum Hæmorrhage.*

Gooch and others have cited a kind of Post-partum Hæmorrhage occurring with a perfectly contracted uterus; this is calculated greatly to confuse and mislead the young accoucheur, since it tends to induce in him a distrust of the basis upon which all his efficiency in treatment must be founded. All admit that an injury to the os uteri, an adhesion of a small portion of the placenta to the uterine wall, or the previous attachment of the placenta over the cervix, may give rise to such a condition, but it is of the first importance for you to engrave upon your minds the fact that *contraction of the uterus and that alone will check the flow of blood from its cavity.*

When we reflect upon the condition of the wall of the uterus, after the avulsion of the placenta, its numerous large mouthed veins left open, and its muscular structure laid bare by removal of its entire mucous covering, it must be a matter of surprise, not that hæmorrhage occurs, but that it occurs so rarely. And indeed, did not nature, at once, come to our aid with her wondrously effective and ingenious mechanism, there would not be a possibility of avoiding a flow from the open gateways which would soon extinguish life.

Hæmorrhage, from other surfaces, is checked, as you know, by the formation of a coagulum in the mouth of the vessels; here that process would be too slow, and nature adopts another and more speedy method. The uterine vessels are surrounded on all sides by the muscular fibres of the uterus, between which they pass, and to which they bear very much the same relation as that which your finger would bear to the threads of a netted purse if pushed between its meshes. Draw upon the ends of the purse and your finger is compressed in proportion to the force applied; let the uterine fibres contract and the extremities of its vessels are closed in like proportion; conversely, as the removal of force from the ends of the purse removes contraction from your finger, so does relaxation of the uterine fibres leave gaping the mouth of its large and flaccid veins. Before proceeding, let me beg you to appreciate this in its full force, for it is the keystone of the arch which bears up the whole pathology and treatment of puerperal hæmorrhage.

We may say then, in general terms, that any thing which induces an inert and sluggish state of the uterus, or anything which defeats its efforts at contraction, would constitute causes of post-partum hæmorrhage. As an instance of the first class of causes may be mentioned inertia uteri, and of the second, partial attachment of the placenta, or formation of clots within the cavity. Let the non-contracted condition of the organ be due to what it may, it is, in itself, the immediate cause of the sanguineous flow, and we may enumerate as special causes,

First.—Inertia uteri.

Second.—Uterine exhaustion.

Third.—Sudden evacuation of uterine contents.

Fourth.—Adherent placenta.

Fifth.—Distension of uterus by blood.

Sixth.—Mental emotion.

Seventh.—An excited state of the circulation.

Inertia Uteri is the name applied to a sluggish state of the uterus, apparently due to some derangement in the nervous influence normally distributed to it. During the labor, while stimulated by the child's body, the organ has contracted feebly and irregularly, and having lost that stimulous it in no wise improves in its action, but lazily lies within the pelvis a soft and flaccid bag, its fibres relaxed, and its vessels left unligated and patent. There is a gush of blood from the vulva, and, unless intelligent and efficient aid is at once rendered, the uterus bellies out under the influence of its accumulation, within its cavity, and the manifold risks of post-partum hæmorrhage are at hand.

Uterine exhaustion gives us a state resembling this very closely, and yet is by no means an identical one. Here, the organ which, perhaps, for days, has been doing its duty with faithfulness and vigour falls feeble and exhausted on the accomplishment of its task. The uterus in the first case resembles the "baulky" horse, the energies of which are unimpaired and undeveloped; in the second it may be likened to the wearied steed, the over-taxed powers of which have yielded to fatigue and prostration, after having been fully developed and employed.

The sudden evacuation of the contents of the uterus proves, I imagine, in the hands of beginners, a more fruitful source of uterine hæmorrhage than either of those just mentioned.

At the beginning of labor, the fundus uteri is at a point, mid-way between the umbilicus and ensiform cartilage; as the expulsion of the child proceeds it slowly descends, inch by inch, until at the accomplishment of the process it forms above the symphysis a globular mass not larger than the fœtal head. Thus nature performs her work by gradual advances made with intervals of rest, in which the nervous energies of

the organ have had time to rally and recuperate. Now the minutes which elapse after the birth of the head and preceed that of the shoulders and body are pregnant with anxiety and suspense for the inexperienced, (and sometimes even the experienced) accoucher. He sees that the uterine efforts have ceased, that the child is not breathing, and fearful lest his inactivity may permit its death, and often urged on by those who surround him, he passes his finger into the axilla next the perineum and speedily the body is drawn from (what was deemed) its position of danger.

A moments reflection will show you how unreasonable, how rash, and how useless is such conduct. It is surely unreasonable to expect uterine efforts, which have been coming on every four or five minutes, to come on now in one, merely to relieve our groundless fears; unreasonable likewise to expect that the uterus will, at a moment's warning, and contrary to the requirements of nature, suddenly contract from a point near the umbilicus to one near the symphysis pubis; it is surely rash thus suddenly to withdraw from a cavity the body which has stimulated it to action, and by its distending force, prevented the separation of the placenta, and it is useless to deliver, at such risks, a child which is, as a rule, in no way endangered by a delay, instituted by nature herself.

The organ, bereft of the stimulus given by the body of the child, and suddenly transformed from a contracting wall into an empty bag, seems staggered by the boldness which would improve upon the workings of nature, and before its contractility has returned, the gaping sluices which the partial separation of the placenta has exposed, give forth a fearful flow.

I desire to speak particularly of this cause of hæmorrhage, because I know, from experience, how much fortitude it requires in a beginner to repel the advice of lookers on, and overcoming his own anxiety, to allow nature to finish the task for which she is so much more competent than he.

Adherent Placenta, or rather adhesion of a part of the placenta, is a prolific and dangerous cause of hemorrhage. If

the whole mass is detached, the uterus contracts upon it no flow occurs, for the necessary ligation is applied to the vessels, and if the adhesion is entire no hæmorrhage takes place, because the utero-placental canals are unbroken. But if partial and firm adhesion exists, the most terrible flooding may arise, the blood escaping either from the uterine surface or by passing into the attached portion of the placenta, and out of the free; the organ being at the same time interfered, with in its contraction by the "splint," as Dr. Meigs styles it which mechanically separates its approaching walls.

Distension by imprisoned blood may likewise prevent contraction, and produce hæmorrhage of so insidious and stealthy a nature that intimation of its occurrence, may be given only by the evidences of approaching dissolution. This may be due either to the fixation of the placenta over the os or in the vagina, preventing discharge; or from clots of blood forming there, or again (as I saw only a few months ago,) from the gross ignorance of a practitioner in applying a tampon.

When thus distended by fluid or clotted blood, the best effort of the uterus are frustated, and though its contraction be powerful, they meet with defeat from the resisting mass against which they act.

Mental Emotion is a well known cause of suspension of uterine action, during the parturient process. A strange physician a nurse who was not expected, or an unwelcome guest, enters the room, and pains previously vigorous and frequent, vanish entirely. In the same way the advent of zealous but injudicious friends, just after labor, whose congratulations serve but to harass and excite the already exhausted woman, tends much to prevent the proper condition of the sensitive and impressible organ, upon the vigorous action of which safety depends. In the more refined walks of life, this will rarely prove a cause of hæmorrhage, but among the lower classes it is not an infrequent one.

An excited state of the circulation undoubtedly predisposes to hæmorrhages post-partum, as has been emphatically stated

by Gooch, Prof. Michælis of Keil, and others. This acts probably not so much by causing the flow in spite of the contracted condition of the uterus, but by disordering such contraction and rendering it imperfect.

Means for preventing post-partum haemorrhage.—The successful ship's surgeon is not he who cures scurvy, but he who in a long voyage prevents it appearance ; the army surgeon who treats with good results the camp fever, is not deserving of the palm, but he who protects from its ravages those entrusted to his care ; and the accoucher who meets with courage, skill and efficiency, the appalling accident of post-partum haemorrhage, is less deserving of our admiration and the confidence of his patients, than he who avoids the danger by careful and judicious means. Do not imagine that I am straining at this parallelism, for all who have had experience in the lying-in room, will assure you that it is a just one, and the statistics already adduced, will of themselves, I believe, incline you to give credence to their statement.

If I have succeeded in convincing you that the frequency with which you will meet with post-partem hæmorrhage, one of the most disagreeable of all the accidents of the lying-in room, will be very much governed by your uniform observance of precautionary measures, I will be quite sure of your attention to the following rules which I will arrange collectively, and analyse as we proceed.

RULES.

1st. After delivery of the head, let the body be expelled by the uterine efforts ; never pull the child out by the shoulders, unless absolutely forced to do so.

2d. Let the hand of an assistant firmly compress the uterus and follow the child down in its birth.

3d. After the birth of the child, let the uterus be *constantly* held in the hand until the bandage is applied.

4th. Never under any circumstances, apply the bandage until the placenta is delivered.

5th. If the placenta is detached, remove it within 20 minutes.

6th. Avoid stimulants during and after delivery.

7th. Never leave the *lying-in chamber* for one hour after delivery.

8th. Before leaving the house, always apply the child to breast.

9th. Enjoin perfect quiet and exclusion of visitors.

10th. If there is reason to anticipate hæmorrhage, give ergot as the head is passing the perineum.

Now all these rules are acknowledged as expedient, and the propriety of their universal application no one will deny, and yet who is not aware that many practitioners are in the constant habit of neglecting some or all of them? Analyse them and they appear small, unimportant and even insignificant, but compare the risk of hæmorrhage attending the practice of one who observes, and that of one who neglects them! One who, for example, as soon as the head has been delivered for a minute, or at most two, despairs of a pain and helps the body out without one; then leaving the uterus uncompressed (as I have so often seen done,) at once applies the bandage and perhaps retires to an adjoining room, there to await with becoming dignity his recall by the nurse, who informs him that the secundines have been expelled. Or, again, one who, after the expulsion of the placenta, following the advice of Dr. Ramsbotham, leaves the application of the bandage to the nurse and retires from the room. It strikes me that such an one is much more likely than his more cautious confrere, to be paralysed by the cry from the nurse that the patient has fainted and is bleeding to death.

Of the first rule, I have, I hope, already said enough, to have impressed upon you its great importance.

The second and third are surely of a value which cannot be over-estimated, and the practitioner who has lost a patient by post-partum hæmorrhage, without having availed himself of their rich results, should feel that he has recklessly disregarded one of the most precious preventive (and curative) means of which art is possessed. Should the non-contracted condition of the uterus be due to inertia, uterine exhaustion,

sudden evacuation of contents, or some strong mental emotion, this pressure will mechanically produce a diminution in the size of the organ, stimulate the flagging fibres to their task, and at last, by pressing the opposite walls against each other, thus plug the bleeding mouths until a vital effort can be effected. Should the placenta be adherent, or should it have fallen like a valve over the os and caused the uterus to fill with blood, this firm and steady force will often expel its contents and incite the emptied organ, at once to become hard and contracted. It constitutes also the best method of removing the detached placenta, and gives to the hand affecting it, the constant assurance that all is, as it should be. For it to prove all this, however, it should be done properly; the organ is not to be flattened by pressing it backwards against the spine, but the wrist should be pressed down, until it rest if possible on the spinal column above the fundus, which will thus come into the palm, the fingers being extended towards the symphisis; the whole organ will thus be grasped and the force should be directed towards the valve as if we were attempting to produce inversion, which however will never occur from it.

The bandage should never be applied before the delivery of the placenta, for these reasons. 1st, It becomes saturated with blood, which has collected on the bed, an dwith that which accompanies the expulsion of the placenta. 2d, It prevents our fulfilling the 2d and 3rd rules, and prevents our recognizing the dilatation of the uterus. 3rd, Its application annoys the exhausted patient, just when she needs rest most, and 4th, it is very little value in itself as a preventive of harm. It is a dangerous error to suppose that the bandage prevents hæmorrhage. If applied very firmly, and with a compress above as well as on the uterus, it certainly aids in keeping up its contraction, but the main purpose subserved by it, is the support given to the large vessels on the spinal column, pressure upon which has been suddenly removed, rendering thier increased capacity dangerous to the sanguineous supply to the brain. In the wards of Paul Dubois at Paris, I never

saw a bandage used, and yet hæmorrhoge is as rare there as elsewhere.

If the placenta is detached, remove it within 20 *minutes*, for if the uterus does not contract upon it in this time, we know that the stimulus of its presence, so well pointed out by Dr. Murphy, has been overcome by a tolerance, established in the organ, and as long as it remains the organ cannot perfectly contract; which at the same time, there is danger of its acting as a tampon. Its removal should be accomplished by the gentlest means; if in the uterus, by firm pressure after the method mentioned above, and if in the vagina, by traction upon the chord.

Should these means fail, the rules applicable to the treatment of "retained placenta" must be followed. I am convinced, however, that if through practice you follow the 2d and 3rd rules here given, you will very rarely meet with retention of the placenta.

Avoid stimulant during and after labor, unless as sometimes occurs syncope or extreme debility call for them. They increase the force of the hearts action, render the patient restless and uncomfortable, and produce a condition of excitement just the contrary of what we desire, and one which predisposes to hæmorrhage.

Never leave the lying-in chamber for one hour after delivery, for a large majority of cases of hæmorrhage occur within this period, and the majority of these within half an hour, during which the placenta is undelivered. By being present, we are able to grapple with the complication at its very beginning, and when the patient's health is unimpaired by loss of blood. During the 20 minutes in which the placenta is undelivered, the accoucher cannot better employ his time, than by sitting at the bedside of his patient and keeping the uterus in his grasp, while the nurse attends to the child, and thus during the period most fraught with danger, he is as secure as forethought and caution can make him. It may be objected that his presence in the room for an hour will annoy and inconvenience the delivered woman, but this is not so.

Every woman at this time, never mind how often she has been in labor before, feels a sense of impending danger, and so far from the presence of her physician inconveniencing her, it gives her a feeling of security and renders her composed and quiet. If detected in its very commencement and met with efficiency and intelligence, post-partum hæmorrhage is generally controllable, but if the loss must continue, until an uneducated nurse (are nurses in America ever otherwise ?) is impressed with the necessity of calling the attendant, if only from the next room, the phase of the matter is changed.

Before leaving the house, always apply the child to the breast, unless the mother does not intend to nurse it, for by exciting the nerves distributed there, we exert a reflex influence on the uterus, of a most salutary kind. So marked is the sympathy between these organs, that Scanzoni has recommended irritation of the mammæ to produce abortion, and of late the application of a child to the nipples has been strongly advised as a mean of spurring on the inefficient pains of tedious labor. Many excellent observers and writers, state it among the means even for checking hæmorrhage, and so highly is it esteemed by the celebrated obstetrician Dr. Rigby, that he uses concerning it the following language, " when every means has failed to induce a sufficient or permanent degree of contraction, we believe that the only certain means which remains is putting the child to its mother's breast, Under no circumstances do we see the sympathy between uterus and breast, so beautifully displayed as here, and we most truly affirm, that we have never known it fail, where the mother was sufficiently conscious to know that it was her own child," and again he says, " In no way can we ensure our patient so completely against this kind of danger, as by putting the child to the breast, uterine contraction which it exerts is not only powerful, but permanent, nor do we consider that a practitioner is justified in leaving a patient in whom the uterus has shown a disposition to inertra without having ensured her safety by this simple and efficient safeguard."

Dr. Marshall Hall accorded fully in this view. Now we are never sure, when leaving the lying-in chamber an hour or even two hours after delivery, that hæmorrhage will not occur, and suppose that it did and that we could not be found. The results would be terrible indeed, in all probability. Who can think it right then to neglect so simple and so accessible a means of safety, when recommended in such terms, and by such men? Always then, and in every case, observe and fulfil this 8th rule. No harm can arise from it, and that much good may, you are told by excellent authorities—upon one point, however, let me guard you, put the child to the breast yourself or see it done; nurses are frequently prejudiced against early suckling, and if you simply leave an order with them, the strong probability is that it will never be obeyed.

Enjoin perfect rest and exclusion of visitors, unless you wish to work for evil upon the already hyperæsthetic nervous system of your patient, and produce a corresponding depression of nerve power. And not only this, but the simple act of sitting up in bed, performed in a moment of forgetfulness, may be sufficient to produce fatal syncope. By the latter of the two injunctions contained in this rule, you also avoid many chances of mental emotions of various kinds, which could only exert a deleterious influence upon your patient. The propriety of the 10th rule which I have given you, no one will dispute. The patient having in previous deliveries suffered from hæmorrhage, or having had an extremely lingering and exhausting labor, we naturally resort to some means which will, before delivery, cause a firm and sufficient contraction, and ergot is the only medicine which secures us this. There is, however, this precaution to be observed in giving it; namely, beware that it is not given before the perineum has yielded, and inflict a rupture upon it by the constancy and violence of the efforts which it exerts. When we have good reason for the anticipation of post-partum hæmorrhage, of course we unite with this, all the other precautionary means mentioned, and as additional ones of much utility, I may mention the application of cold to the fundus

uteri, and the administration of an enema of ice cold water, at the same time that the ergot is administered.

I have remarked that practitioners who have never lost or seen lost, a case by puerperal hæmorhage, are very right to conclude that its dangers are much exaggerated and that death is an infrequent result of it. Let such inform themselves on this point, by reference to the excellent tables embodying the experience of the "Royal Maternity charity," London, compiled by the Drs. Ramsbotham, and given in the appendix of "Ramsbotham system of Obstetrics." They will there learn "that of the 162 deaths from puerperal causes, 66 were from hæmorrhage, either immediate or remote," and that of those attacked by this accident, a little over 10 per cent died. No compromise for carelessness and no comfort for voluntary ignorance can be gleaned there in this direction. Let me in concluding, gentlemen, advise you strongly to follow the first nine of the rules given here, in every case which you may attend; believe me that if you do so, in the beginning of your practices you will never depart from the habit, and I assure you, that you will find, that by taking these trifling precautions, your practices as obstetricians will be bereft of much of their burthen of responsibility and anxiety, by a feeling of security and confidence.

Like a single strand of a cable, any one of these rules separated from its neighbors is weak and unreliable, but the whole combined will give you a power which nothing but the united force of a combination of unfortunate circumstances will break. And who is there who would not take tenfold the trouble, which is attendant upon their observance, if it added one iota to his chances for escaping that stunning calamity, the loss of a healthy and robust woman, in the most interesting hour of her existence.

ART. V.—*Case of Traumatic Tetanus*—Recovery. By THOS. P. BAILEY, M.D. North Santee, S. C.

This disease, so generally fatal, discourages and impresses us with the inefficiency of our therapeutical means. We are still in much ignorance as to its true pathology, and until further developments are made, we must confess our want of a proper *ratio medendi* for our guidance in the use of remedies. Its mortality is very clearly shown by Dr. O'Beirne, who stated that of 200 cases observed by him, not one recovered. As an exceptional instance, therefore, it affords me some gratification to record the subjoined.

Case —, I was summoned, on the morning of the 7th inst., to see Isaac, a negro slave, aged about 40. The following are the facts I learned on my arrival. About ten days ago the two phalanges of the third and middle fingers were injured in the rice thrashing mill. He has since been at work the wounds having nearly healed, and causing him very little inconvenience, until Saturday the 5th inst., when he complained of pain and stiffness of the arm of the side affected, extending up his jaws, and between his shoulders. This continued until the following (Sunday) night, when he was taken with well defined spasms during the night. A dose of Ol. Ricini and several doses of Tinc. Opii, also, six grains solid opium had been given previous to my arrival which tended to lengthen the intervals between the spasms, he having had three paroxysms since the last dose was given. During my examination he was taken with a most frightful spasm, the muscles of his face contorted—with opisthotonos and pleurosthotonos—pulse full and hard, tongue slightly furred, bowels not acted since he took the oil. I immediately laid open the partially healed fingers and filled up the incisions with sulph. morphia, had a blister applied the length of his spine and gave one grain sulph. morphia ; also, ordered him to be well rubbed with ung. hydrarg, and prescribed the following:

℞ Tinc. Cannabis Indica ʒi.
Inf. Lini. - - ʒiij.—m.

A tablespoonful of the above to be taken every two hours, and if the spasms increased to be given every hour.

Tuesday, March 8th,—was surprised to find his condition much improved this morning. Spasms gradually subsided since my visit yesterday. Has had a very good night and appears tranquil. Blister drawn well. Has had two or three stools from oil. Has taken several doses of the mixture. Continue mixture occasionally.

9th. Patient still continues to improve—continue the same.

11th. I saw the overseer to-day, who reports patient much better. I have since heard that he is walking about.

It will be seen from the above that no treatment has been pursued worthy of special comment. How far the cannabis indica contributed to the favorable termination, I am unable to say, for, as it will be perceived, he had taken a quantity of laudanum and opium.

I think counter irritation to the spine in these cases very important, and should I meet with another I will, perhaps, try the actual cautery. In conclusion, I would remark, that I lay claim to no particular merit because the case terminated favorably, but simply wish to add another to the small list of recoveries.

ART. VI.—*Surgical Cases and Clinical Notes.* By J. J. CHISOLM, M.D., Prof. of Surgery in the Medical College of the State of South Carolina.

We have been induced to request, from Prof. Chisolm, a short report of a few of the many very interesting cases which were operated upon by him, so successfully before the medical class of the past season. The clinic just closed has been exceedingly rich, both in the number and interest of the cases, and many of these are well worthy of record. We congratulate the Professor upon the success of this, his maiden effort, and, also, upon the course which he pursued, so profitably to the class, of exhibing the results of the operations, in order to show the advantages of a judicious course of treatment.

He enforced upon the class, by precept and example, the advantages of the simple and efficient cold-water dressing for wounds, by exhibiting to them perfect cures by quick union with straps, after amputations, in ten days.

Irrigation is the course of treatment used by him in all wounds, and always with the best results. He never allowed an opportunity to pass without impressing upon the class, not only the importance of this course, but the essential advantages to be derived throughout the domains of surgical treatment, by guarding against the "nimia diligentia," an error which beginners are so apt to embrace. He showed them, by numerous examples, the impropriety of preparing every case for surgical operation, whether it required preparation or not, and waged war against the common enemy, systematic purgation. If the patient was in good health, blessed with the harmonious performance of all his natural functions, that it was the most injurious folly to disturb that harmony by pouring irritants into the stomach. That when the economy was at peace with itself, was the very best time for the performance of operations of whatever character, and that to the purge of preparation could often be traced the source of many of the bad results after surgical operations. He sedulously avoids this kind of preparation, except in those cases which require the suspension of evacuations, by stool, for some days after being operated upon. We have seen him perform some of the most delicate operations in Surgery, viz: in cataract, for the extraction of the lens in one or both eyes, and, also, for the removal of false membranes, without a single bad symptom, where the patient being healthy, no preparation was instituted.

Chloroform was used in all the cases brought before the clinic, whether simple or serious, without a single bad result; the amount of chloroform used, varied from 1ʒ to 5 oz. for each case. It was always inhaled from a handkerchief twisted into a cone, and was administered by a capable assistant, who was sufficiently intimate with surgical operations not to have his attention drawn from the very important duty in which he was engaged. We can very easily understand why deaths

sometimes occur in the administration of chloroform, and are only surprised that these accidents are not of more frequent occurrence. An assistant is detailed to administer the anæsthetic. As soon as voluntary motion is in abeyance, and the operation is about being commenced, in his anxiety not to loose a motion of the surgeon, he holds the cloth containing the chloroform firmly over the mouth and nose of the patient, so that he may fix the patient's head and compel him to inhale, without being watched, in order that his whole attention may be devoted to the surgeon. The patient is so stupified and paralyzed by the æther, that he is incapable of resistance. He has no longer the power of displacing the cloth, which so effectually excludes atmospheric air that the patient is killed by suffocation. In many cases chloroform innocently bears the blame, when, if the truth could be known, a more responsible agent would be found in the active partisan. We have often been made nervous by seeing the recklessness with which this blessed drug has been abused, and have been surprised that the most serious consequences have not followed its mal-administration. Prof. Chisolm gave the class ample proof of the value and safety of the remedy, when administered with caution. In his hospital as well as in private practice, he does not hesitate to use it, wherever pain can be prevented. He administers it as freely for surgical dressings, when painful, as for the performance of operations.

RADICAL CURE OF CONGENITAL HERNIA.

Robert, aged 12, a healthy boy, the subject of a double congenital hernia, was operated upon by the injection of tinc. iodine diluted with equal parts of water. A strong double truss was applied to prevent the injection passing into the abdomen. A good deal of inflammation was excited, by which the right scrotal sac became permanently obliterated; the left remained open, although the inguinal canal on that side was closed by the inflammation produced, so that a hydrocele formed in this left scrotum, which was easily cured by the

iodine injection.. A fortnight after the operation, the truss was dispensed with, as by that time, consolidation of the canal was perfect. The case was kept under observation for some time after the radical cure had been effected.

James, a healthy boy, aged 8, with congenital hernia, was operated upon by the injection of tincture iodine; the truss as usual, having previously been applied. This case, like the preceding one, progressed in a most satisfactory manner, and a radical cure was obtained. In this case, the pure tincture of iodine was injected into the sack, which contained fluid. The small trocar syringe, of Dr. Alex. Wood, used for the subdermic application of anodynes, was found very useful. The muzzle of the syringe having been plunged into the sac, and its presence there recognized by the escape of a few drops of urine, the pure tinct. iodine was thrown directly in, and left to itself.

RADICAL CURE OF HERNIA, BY WÜTZER'S METHOD.

Isaac, a hearty man, aged 24, with a recent inguinal hernia, was operated upon for its radical cure, by Wützer's apparatus.* The instrument used was that modified by Rothmund. The apparatus was retained eight days without pain, and comparatively little inconvenience. No excitement of the pulse was at any time induced; and the only annoyance was from the recumbent posture which the patient was compelled to keep. Laudanum was given, for a few days, to control the action of the bowels. After the removal of the apparatuss, a truss was applied over the canal to induce a matting of the tissues. The invaginated portion was thought sufficiently excoriated to adhere without using vesicating applications. After an interval of a few days, however, strong liquor ammonia was applied within the invaginated canal, and the truss

* This operation has been very generally introduced here, Dr. Chisolm has lately performed it five times in a single week.—Ed.

used to mat the whole together. which it effected beyond our expectation. No deformity existed after the cure.

SYME'S PERINEAL SECTION FOR STRICTURE.

Andrew, aged 50, in good health, was brought before the class, suffering from a chronic stricture, with a perineal fistula of three years standing. He had been for years under medical treatment with no benefit. Through the stricture a fine bougié, No. 12, could be introduced with difficulty. The shouldered staff of Syme could not be introduced as a guide. The perineum was cut down upon, and by keeping in the median line the urethra was found after a search of ten minutes, and a large catheter passed into the bladder. The catheter was retained three days, the bowels having been controlled by opium. An interval of five days intervened before any attempts were made to pass a bougie, when the largest size entered with facility. The perineal wound healed by quick union. On the 11th day after the operation, a large quantity of bloody urine, with clots, was passed without any apparent cause. The bougie had not been used for two days, and the urine, on the day following its last application, had been perfectly natural. Under the use of the nitrate of potash, ℈i every four hours, all trace of blood disappeared in 48 hours. The hæmorrhage was from the bladder ; when the catheter was used, the flow of bloody urine was frequently interrupted by the discharge of clots. This patient was kept under observation two months, at the end of which time the patient could, himself, pass the largest bougie with facility.

Jackson, aged 35, a delicate looking man, with bad habits, had been suffering from stricture for 15 years, resulting from a fall. He had been repeatedly, and for lengthy periods, under surgical attendance without benefit. The finest bougie had not been introduced for some time, and at intervals he suffered from complete retention. Great irritability of bladder existed, with a thick turbid urine. An attempt was made to introduce a very fine conical bougie, and after a tedious sitting it passed the stricture. Its introduction was followed

by a violent chill, with continued vomiting and collapse which required the free administration of opium and brandy. When this case was presented for operation, as Syme's staff could not be introduced as a guide, a fine bougie, after some delay, was passed into the bladder, followed by a lithotomy staff in the urethra, leading down to the strictured portion. The perineum was incised upon this groved sound. Upon exposing the end of it the continuation of the urethra with the bougie could not be found. Fears were entertained that this had been divided by the knife, and that one portion had been drawn into the bladder. Upon withdrawing the bougie from the meatus it was found whole and not bent, showing that it had traversed the urethra and that the sound had followed a false passage. The sound was again introduced, the bougie having been previously passed and was directed to the opposite side of the perineum to avoid the false passage; it was cut down upon through the wound and exposed, but no clue to the bougie, nor the continuation of the urethra could be discovered; the sound having followed a second false passage. This was then withdrawn as useless, and careful diligent search was made for the bougie, which was at last discovered. The urethra, was opened, and with a grooved director passed through the perineal wound, into the bladder, guided by the fine bougie, no further difficulty to the introduction of the catheter was met. The patient informed us that attempts had been made by many physicians to introduce catheters, which caused violent pains and profuse hæmorrhage; notwithstanding the force which they had used, they had not succeeded in entering the bladder. This patient, as well as the one whose case was previously related, inhaled 4 oz. chloroform, each, before anœsthesia was effected. The introduction of a bougie guide proved of the greatest service, as the operation without its aid, from the number of false passages, would have been one of extreme difficulty.

The catheter was allowed to remain in 48 hours, when it was removed.

The day after the operation, the patient was seized with chills, followed by fever. Erysepelas attacked the wound and

spreading at once to the scrotum, with its great amount of œdema and swelling, threatened gangrene. The urine passing through the perineal wound produced torture. The third and fourth nights after the operation, the patient lost a good-deal of blood from the wound, which was followed by a marked diminution of the scrotum, but great prostration of the pulse, which became irregular and very feeble. The tongue became dry and brown, the patient looking haggard; morphine and the mur. tinct. ferri, had been given up to this time with plugging of the wound. An unsuccessful attempt under chloroform was made on the 5th day after the operation, to introduce the catheter so as to prevent the escape of urine over the perineal wound, of which the patient complained so much. Four powders of calomel grs. ii., opium gr. ss. were given, at four hours interval, to restore the secretions and soften the tongue, whilst strong nourishment, with brandy, was taken freely to support strength. After 24 hours the tongue assumed a more natural appearance; the pulse improved gradually under the nourishing and stimulating diet, and in 15 days the patient was sitting up, decidedly convalescent. The perineal wound sloughed extensively, making a large open sack of the scrotum. Without local treatment except the liberal inunction of olive oil to protect the excoriated parts, the wound rapidly granulated, and two months after the operation it had altogether closed; a large bougie passes with great facility into the bladder.

In this case six weeks was allowed to elapse from the removal of the catheter, two days after the operation, before any attempts were made to catheterize him, when a large bougie entered without difficulty, which it continued to do, the patient being his own surgeon.

NECROSIS OF SUPERIOR MAXILLARY BONE AND ITS REMOVAL, ENTIRE, WITHOUT THE USE OF THE KNIFE.

Lizzy, aged 5, apparently in good health, had a tumor over the right cheek, over which large veins were seen coursing.

Upon examining the mouth the entire buccal surface of the upper jaw was found uncovered by mucous membrane, all the teeth had disappeared from the alveolæ, except the two incisors which were still attached to the gum, and were quite moveable, having lost their osseous support, and a constant purulent discharge from the surrounding tissues acted as an annoying irritation upon her system. Her history given was, that after an attack of fever, accompanied by great prostration, and emaciation, this trouble commenced in the mouth, two or three teeth were drawn without allaying the pain or remedying the evil. Six months elapsed before the case was operated upon. In the meantime the sequestrum had isolated itself from its periosteal envelope, and its contiguous connections had become so weakened, that by steady and firm traction with broad, heavy forceps applied through the mouth to the alveolar processes, the sutures yielded, and the entire upper jaw came away without any incisions being made, and without hemorrhage. As the two incisive teeth were useless, and moreover were a deformity, they, with the gum attached, were removed; the cavity rapidly filled up by granulations, and from the day of the operation all pain and restlesness disappeared. The deformed swelling of the face rapidly subsided; no after treatment was used; chloroform to anæsthesia was administered.

EXTIRPATION OF ENCEPHALOID EYE.

Mary, aged 4, a very healthy child, from very healthy parents, was brought before the class for the removal of the eye. The globe was very much elongated, so as to protrude for three-quarters of an inch beyond the eyelids, without, however, having increased in its transverse diameter. The metallic lustre firstmade its appearance nearly 6 months before. The protrusion of the globe dates 2 months, although severe suffering existed from an anterior date. The cornea is now completely effaced, with the hypertrophed conjunctiva thrown into a series of folds around the opaque relic of the cornea. The mass still moves with all the sympathetic motions of the sound

eye, showing that probably the disease is confined within the sclerotic. Under chloroform the eye was removed with facility. The patient at once lost all the restlesness and suffering under which she had been laboring for weeks, and slept throughout the night of the operation, without assistance from anodyne, a gratification she had not known for days; and without confinement in bed or any restrictions in diet, she at once became convalescent.

At the end of a month a small tubercle, upon examination, was found shooting from the upper lids, with this, the neighboring lymphatic glands had become enlarged, showing systemic contamination.

COMPOUND DEPRESSED FRACTURE OF SKULL NOT OPERATED UPON—RECOVERY.

Hagar, aged 8, a very healthy girl, was kicked in the head by a horse, on the morning of the 16th of February, and was brought to the city, a distance of twelve miles, to be operated upon. When seen she was lively, suffering but little pain. Upon examination, a wound two and a half inches long existed over the frontal bone, parallel to and an inch above the super-cilliary ridges. The os frontis was laid bare for some distance, and when examined by the finger and probe, a fissure, corresponding to the scalp wound, could be easily felt, the upper edge of which was gradually depressed as it approached the median line, until near the sagittal suture, it reached the depth of one-third of an inch, the inferior lip retaining its normal elevation. The lips of the wound were brought together by sutures, and the patient was put under cold water treatment, with avoidance of all stimulants, mild diet and no medicine.

The case progressed uninterruptedly to a successful cure, without the slightest bad symptom.

This case well exemplified the rule of strict non-interference, where cerebral symptoms do not urgently demand relief.

EXTIRPATION OF TWO SMALL GLANDULAR TUMORS—DEATH FROM GANGRENE

Jack, aged 40, a fine specimen of a man, in apparently good health, had been under treatment for stricture. When about being discharged, as cured, he called attention to a small tumor over the spine of the tibia, which he desired to have removed. The tumor was glandular, about the size of a pullet's egg, and about two inches from it, on the inner side of the knee, was a second gland, not larger than a bean. He had, at the same time, a small lipomatous tumor over the lumbar region. Powdered ice and salt was used as an anæsthetic, but not sufficiently long to annul pain. The tumors were subcutaneous, and the hæmorrhage was very trifling, although the pain, during removal, appeared to be severe. The lips of the wounds were brought together by wire sutures and cold water applied. By the next day, reaction had come on in the wound, and internal hæmorrhage sufficient to distend the skin to the utmost, in the site of the tumors, had occurred, necessitating the division of the sutures and removal of the clots. The leg then assumed a glistening appearance, swelling ensued, accompanied with intense pain, and general febrile excitement.

℞. The limb was elevated, cold water constantly applied, and a dose of sulph. magnesia administered. Laudanum ʒi. at bed time.

On the following day, the third after the operation, the patient continued to complain of agonizing pain in the leg, which continued swollen, with phlyctinœ containing a dirty fluid on the under side of the middle of the calf; skin cool, pulse feeble.

℞. Deep incisions were made into the leg, four inches long. The superficial tissues were found gangrenous, and without sensation; from these neither blood nor pus was discharged. The textures below the fascia lata were healthy; the foot retained its natural temperature.

℞. Pyroligneous acid was constantly used to the leg; whilst

nourishing food, brandy, and opium was freely given to sustain life.

The following day, the fourth after the operation, the patient died, notwithstanding all attempts to keep him alive by the constant and very liberal administration of stimuli.

On autopsy, the leg was found gangrenous, with pus in the internal saphenous vein; the femoral vein, up to the groin, being filled with clotted blood.

This case was one, marking the very responsible position of the surgeon, and showing with what serious feelings he should undertake the most trivial operation. After contemplating this, we could comprehend Sir Astley Cooper's reason for declining to operate upon the royal head for the removal of a small atheromatous tumor. In the presence of majesty he felt the responsibility of the undertaking.

This case has made its corresponding impression upon us, teaching us the lesson of regarding all operations as serious, however trivial they may appear. For a more trivial operation than the enucleation of a small glandular tumor could hardly be conceived, and yet, a more sudden and terrible result could not occur. The patient, when operated upon, was apparently in perfect health. Two days after the operation, when mortification commenced, his pulse sunk so rapidly, and the skin became so cold and clammy that amputation was already out of the question ; neither loss of blood, nor any debiletating remedy had been used to account for the sudden collaps.

KELOID TUMORS.

Molsey, a healthy stout negro girl, aged 20, was operated upon for two keloid tumors, one of the size of a hen's egg, but of button shape, pedunculated, was seated on the nape of the neck. The second, larger than the first, nodulated, was attached by its entire surface to the left side of the face and neck, involving the lower lobe of the ear. These tumors were supposed to have originated from the blow of a whip. Under chloroform, they were removed after troublesome dissec-

tion. The pus oozing from the surface was controlled by cold applications. The skin, when stretched by iron wire sutures, could not be made to cover the raw surface, a third of which remained exposed. Ice dressings were constantly used for two days, which allayed all pain and prevented inflammation. The wound was then painted every second or third day with very strong tinct. iodine to prevent, if possible, the return of the affection.

TAPPING FOR DROPSY.

A child aged 5, was brought before the class, suffering from abnominal accumulation which was so excessive as to embarrass respiration. The antecedents of the child were not known. She was tapped in the linea albæ, eight quarts of fluid were withdrawn, with a marked relief of the respiratory symptoms.

NECROSIS OF STERNUM.

A Portuguese aged 55, with necrosis of the upper part of the sterum, was brought before the class for operation. The soft tissues were much thickened over the clavicular junctions, with the sternum, having over the left articulation, two sinuses leading to exposed bone. Over the left side of the chest was a sympathetic abscess, containing an ounce of healthy puss. The disease was of eight months standing. By making a crucial incision, the bone was easily exposed, and by the careful use of the saw, a ledge of bone was removed, which made a sufficient opening for manipulation, and, by the use of strong forceps, three fragments involving the entire thickness of the sternum, including over a square inch of surface, was removed, leaving a narrow bony ridge on the right side which connected the two portions of the sternum. The sympathetic abscess was then freely opened. The case was treated by cold water dressings, with very satisfactory results.

VESICO-VAGINAL FISTULA.

Rachel, aged 35, a woman in good health. Her last labor, which was very tedious, prolonged through three days, was followed by extensive sloughing, which involved both the vesico-vaginal surface and the neck of the uterus. Under no treatment, for her condition was never suspected, the parts healed to a certain extent, leaving an opening in the anterior cul-de-sac of the vagina, through which the end of the little finger can be introduced. Upon examination with the speculum, the anterior lip of the uterus is found wanting, and the fistula surrounded by cicatricial tissue, with a band running across the vagina transversely, which makes quite a ridge, when the parts are stretched with Sims' retractor. An irregular fissure bounded by cicatricial tissue leads to the left side of the vagina, and is the outlet for the neck of the uterus, the relic of which has been drawn out of its normal direction. The position of the fistula is such, that when the surrounding tissues are sufficiently pared for obtaining union, the outlet from the uterus will be somewhat encroached upon. With Sims' retractor the parts were easily reached, and the mucous surface of the vagina pared by the scissors, respecting the lining of the bladder which is necessary for success. The rapid secretion of urine and excessive hæmorrhage rendered the operation very tedious. Seven sutures brought the parts well together. During the following eight days the patient passed her water through the catheter, retaining towards the end of that period, nearly a pint before she would void it. On the eighth day the vagina was examined and found much inflamed with lymphy deposit in patches. Most of the silves sutures had sloughed out and were removed. A small opening still existed, through which urine oozed. The patient was now washed out daily, and was allowed to get up.

When examined a month afterwards, the opening was found much constricted, but still large enough to allow the free escape of urine, wherever the vagina was examined, although the leakage was so trifling as to be no longer a cause for com-

plaint. She passed urine ten or twelve times in the twenty-four hours, and could retain two or three gills. At an interval of six weeks, the remaining orifice was again pared, and five silver sutures applied, with one over the whole, from one little hillock of vagina to the other, which mounted up as ridges on either side of the line of suture One suture from these ridges removed all tension from the wound, imbedding the sutures in a fixed fold of the vagina. An open catheter, as on a former occasion, was left in the bladder, and an opiate given, morning and evening, to control the bowels.

Nothing of special interest occurred during the treatment. The catheter was left in until the tenth day, remaining open. Upon examination, at the end of that interval, all the sutures were found sloughed out, and although the opening was much contracted a small orifice still remained, from which, however, very little urine escaped. Further contraction of this afterwards took place. The condition of the patient is infinitely improved, as the leakage only occurs, now, in drops, when the parts are stretched by instruments.

REVIEWS.

ART. VII.—*A Treatise on Human Physiology, designed for the use of Students and Practitioners of Medicine.* By JOHN C. DALTON, Jr., M.D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, New York; Member of the New York Academy of Medicine; of the New York Pathological Society; of the American Academy of Arts and Sciences, Boston, Mass.; and of the Biological department of the Academy of Natural Sciences of Philadelphia. With two hundred and fifty-four Illustrations. Philadelphia: Blanchard & Lea. 1859. 8vo. pp. 608.

A new work on Physiology, if it emanates from a respectable source, and professes to contain the results of original investigation and personal experiment, is as eagerly anticipated and hastily devoured by the reading medical public, as is the last chapter of a popular serial by the people at large. The sheets are scarcely dry before every one has conned their imprint, and formed and expressed an opinion concerning the merits and faults of the book, usually as dogmatic as hasty. Nowhere is this vicious criticism so generally indulged in as in our own country, and to no class of productions is it so unfair and unwise as when applied to the labors and deductions of the painstaking and earnest worker in the obscure department of Biology.

The reaction of this pernicious habit upon the press is very widely exhibited, and what should be the really grave organ of unbiased and deliberate conviction, is often degraded into the blatant and silly mouthpiece of uninformed and unconsidered eulogy or censure.

We premise the above remark with no reference to the reception of Dr. Dalton's book, for the journals have not yet published their pronunciamento, but it is suggested by the recollection of the manner in which the last American contribution to our text books on Physiology was received. That really admirable work which now bears the honorable seal of European recognition and approval, was the subject of the strangest animadversions on the part of American critics, and with a few honorable exceptions, its brilliant and masterly analyses, profound erudition, and original discoveries and suggestions, were either altogether unnoticed, or slurred over with the stereotyped phrases of formal and unmeaning commendation. We sincerely wish our author a better fortune, and trust that his work will obtain, more generally, the careful and thorough examination, which it is honestly entitled to.

The title of the volume before us, will offer every apology for passing over a considerable portion of its contents with but a single word of approbation or dissent, for purporting to be a text book for students, it must necessarily embrace much that is familiar to the older reader, and even where well-known views are sustained by personal experiment, the mention of the latter will be generally sufficient. Its title, too, will give force to the expression of surprise that will be uttered by every one on finding that certain subjects are passed over, without even a hint on the part of the writer that they are either essential to the completeness of his book, or of any moment or interest to teachers or students. Before entering upon our task, therefore, it will be well to dispose of this very singular anomaly *in limine*, and leave the reader to account for it by such reasons or apologies as may suggest themselves to the individual.

The only hint at the existence of, or excuse for, these deficiences which Dr. Dalton offers is contained in the concluding paragraph of his preface, where he says, "physiological questions which are in an altogether unsettled state, as well as purely hypothetical topics, have been purposely avoided, as not coming within the plan of this work, nor as calculated to increase its usefulness." Now, the omissions complained of,

relate not to unsettled or hypothetical questions, but to subjects in regard to some of which our information is full, accurate, and satisfactory. The special senses are barely alluded to in connection with their special nerves, whose functions are very summarily treated of, under the division entitled Nervous System, in the brief space of two pages and a quarter. To say nothing of the immense importance of the senses of taste, touch and hearing considered in their diverse and complicated relations to the functions of both animal and organic life, or of our knowledge concerning their instruments, modes of action, and final as well as efficient causes, it will be generally conceded that of no organ in the body are we more thoroughly informed, both in regard to its physical and physiological functions, than the eye. Besides numerous monographs and works on optics and natural philosophy, the labors of such men as Cooper, Jacob, and, in our country, Draper, have lately done much to perfect our knowledge of the very interesting subject of vision, and of the mode in which its organ brings us into relation with the great world of sense.

In treating, again of digestion, no reference to the excretions from the intestinal canal is made ; the ingesta are carried as far as the ileo-cœcal valve and left to make their way out in such manner and shape as they best may. It is true that the subject of fæcefication is by no menns perfectly understood at present, so far it may be defined to be in an unsettled state, but the French and Germans have contributed enough towards our knowledge of their formation, to require some notice to be taken of them in all modern text books.

So much for faults of omission which, though striking, can, and we are confident will be rectified in future editions ; to the student they are very important, and will be generally felt, and as a *text book*, therefore, its incompleteness creates an obstacle in the way of its general introduction into schools not easily overcome.

In his introductory chapter, the author points out what he conceives to be the proper objects and position of the science. We are compelled to make issue with some of the opinions

here expressed, as tending not only to degrade physiology, but destroy all hopes of its ultimate perfection as an inductive science. We perfectly agree with him that the essential nature of the vital processes, and their ultimate causes are questions beyond the reach of the physiologist, that the latter should confine himself strictly to the study of the vital phenomena themselves, their characters, regularity and condition; but Dr. Dalton goes on to say:

When he has discovered that a certain phenomenon always takes place in the presence of certain conditions, he has established what is called a general principle, or a LAW of Physiology.

As, for example, when he has ascertained that sensation and motion occupy distinct situations in every part of the nervous system.

This "Law," however, it must be remembered, is not a discovery by itself, nor does it give him any new information, but is simply the expression, in convenient and comprehensive language of the facts with which he was already previously acquainted. It is very dangerous, therefore, to make these laws or general principles the subjects of our study instead of the vital phenomena, or to suppose that they have any value, except as the expression of previously ascertained facts, such a misconceptiou would lead to bad practical results. For if we were to observe a phenomenon in discordance with a "law" or "principle" we might be led to neglect or misinterpret the phenomenon in order to preserve the law. But this would be manifestly incorrect. For the law is not superior to the phenomenon, but, on the contrary, depends upon it, and derives its whole authority from it. Such mistakes, however, have been repeatedly made in Physiology, and have frequently retarded its advance.

There is only one means by which Physiology can be studied: that is, the observation of nature. Its phenomena cannot be reasoned out by themselves, nor inferred by logical sequence from any original principles, nor from any other set of phenomena whatever.

In Mathematics and Philosophy, on the other hand, certain truths are taken for granted, or perceived by intuition, and the remainder afterwards derived from them by a process of reasoning. But in Physiology, as in all the other natural sciences there is no such starting point, and it is impossible to judge of the character of a phenomenon until after it has

been observed. Thus, the only way to learn what action is excited by nitric acid upon carbonate of soda is to put the two substances together and observe the changes which take place; for there is nothing in the general characters of these two substances which could guide us in anticipating the result.

Neither can we infer the truths of Physiology from those of Anatomy, nor the truths of one part of Physiology from those of another part; but all must be ascertained directly and separately by observation.

For, although one department of natural science is almost always a necessary preliminary to the study of another, yet the facts of the latter, *can never be in the least degree inferred from those of the former but must be studied by themselves.*

In the first place what meaning does the author attach to the word facts. The ancients held it to be a fact that the earth was an extended plain around which the sun moved, and might have appealed with the strongest emphasis to universal experience for its truth, the moderns on the other hand assert that the earth is a spheroid and moves round the sun, and prove the correctness of their belief by showing that upon this and no other hypothesis can we explain all of the other phenomena related to the motion and position of these bodies. Fact and theory are only other terms for things and ideas necessarily connected, and each to some degree includes the other; all observation implies reasoning, that is we translate sensations into the forms of our understanding, and only thus do we acquire knowledge. A *law* is not merely a convenient expression of recognised facts, if by the latter term we mean only objects of sensation, for it not merely includes these, but our idea of their relation. The theory thus becomes itself a fact and supplies materials for a wider generalization, and thus by successive steps legitimate induction establishes truths above and beyond the reach of mere sense perception. The highest law therefore must comprehend all the lesser circles from which it has been evolved, and is hence not only superior to the phenomena but derives its authority from other and higher sources than sensation.

Observation of phenomena is only a part of this inductive

process, whose complement is to be found in our mental reaction upon the objects of perception. If no such antithesis existed, all nature would be for us but a collection of independent facts having no relation or interdependence, the notions of causality, successive or continued existence would be impossible, and no science could ever be constructed. While vital phenomena therefore are the subjects, general principles or laws are the objects of all biological study, and the latter must inevitably flow from the first so long as the construction of our minds be what it is.

It is not, therefore, impossible to judge of the *character* of a phenemenon until after it has been observed. Take for example the author's own illustration. If an acid composed of nitrogen combined with oxygen in the ratio of 1 : 5, were just discovered, the law of affinity between these elements and carbon and sodium being already perfectly established, we could predicate absolutely the precise character of the phenomenon which would ensue on bringing the new acid and carbonate of soda together. Examples of this kind are too numerous and familiar to need quotation. Nay, more, as in chemistry the composition of substances has been legitimately and correctly inferred from pure analogy, so many physiological truths may be inferred from those of Anatomy and other parts of the same science, and afterwards only verified by direct and separate observation. Given our present knowledge of the cranio-spinal axis and its functions, if a new nerve was discovered, and its peripheral distribution traced to the skin we would at once and correctly infer it to be a nerve of sensation, whilst if its branches were wholly spent upon the muscles of a limb we would as legitimately conclude it to be a nerve of motion.

The difference between the deductive and inductive sciences, after all, relates chiefly to the mode by which a correct starting point is attained. All of the inductive sciences as Comte has pointed out, have, in their progress towards positivism passed through certain phases which are a part of their common history, and an expression of those fixed laws of human thought in obedience to which the philosophy

of all ages has evolved itself, nor have the organical sciences proved an exception to this normal law of growth. Every science is based upon certain fundamental ideas which themselves contain the elements of those truths which are afterwards discovered and enumerated. Upon the determination of these, that is upon a correct apprehension of the conceptions they involve, and the demonstration of their accordance with observed facts depends the existence as well as certainty of the science to be constructed. In all alike the fundamental idea is at first but dimly and imperfectly appreciated, only by slow degrees do the conceptions arising from them become clear and distinct, and not until these have worked themselves out into perfect precision are those postive truths discovered which form the basis of successive generalizations. Where the truths are necessary and universal—that is not derived from observation but imposing their conditions upon that knowledge of which observation only supplies the materials—as the ideas of space, time and number, which form the foundations of geometry and arithmetic, the sciences based upon such have no process of proof but deduction, and their progress consists solely in tracing the consequences of the ideas themselves, and in detecting analogies and connections more or less general and intimate among the conceptions derivable from these. In the inductive sciences, on the other hand, general laws are to be collected from observed phenomena, and true theories from limited facts; in unfolding their fundamental ideas therefore, by a reference to which, combined with experience, the special laws of the science can alone be determined large space for error exists, and the first advances must in every instance be made through the instrumentality of hypothesis alone. As observation and experience correct the faults and point out the deficiences of these, we find the circle gradually narrowed until the centre is finally reached from which a new and more certain method of progress becomes possible.

If Dr. Dalton's position is correct, each physiological fact is an isolated thing, and no such relation subsists among their totality, as will render possible a science of biology. We,

therefore, wish distinctly to aver our dissent from this view, and our profound conviction that the day is not far distant when physiology may claim with confidence the right to be considered not the least noble of the purely inductive sciences. In the one event an insuperable barrier to true progress is raised in its limitation to narrow experience and personal observation, and as all facts however numerous are limited, no universal truth can ever be discovered. If the other opinion is correct as we confidently believe, the ultimate certainty of biology is assured, and the highest inducements held out for perseverance in the method of inquiry now generally agreed upon, of whose value the triumphs of the past are ample and splendid guarantee.

Dr. Dalton treats of his subjects under three great divisions, a convenient and natural arrangement; the first, entitled Nutrition, including everything which relates to the preservation of healthy structure and function: the second, devoted to the phenomena of the Nervous System, or those means by which the animal is placed *en rapport* with the external world, and the third embracing the whole process of Reproduction, by which the continuance of the species is ensured. The two first, in a word, are concerned in the study of the vegetative and animal functions, on which the well being of the individual depends, the last with that wonderful provision by which through all the changing accidents of time, the unaltered type and permanence of the race is secured.

Under the first head, that of Nutrition, the author in his initial chapter, discusses the proximate principles in general. As is true throughout the entire work, the definitions are clear and precise, the arrangement admirable, the argument briefly and well stated, and the style nervous, simple and concise. By a proximate principle is meant, any substance, whether simple or compound, chemically speaking, which exists under its own form, in the animal solid or fluid, and which can be extracted by means which do not alter or destroy its chemical properties. In connection with the latter restriction, the important caution is given, that in extracting the proximate principles from the animal body, only such

means should be adopted as will isolate the substances already existing in tissues and fluids, without decomposing them or altering their nature. The object of the operation being simply the separation of the ingredients from each other, and not their decomposition or ultimate analysis, the simplest forms of chemical manipulation should be employed, or the analysis will prove deceptive and useless. In his classification of these elements Dr. Dalton adopts the arrangement and nomenclature of Robin and Verdeil, which he regards as the best yet offered.

1st. The INORGANIC, derived from the exterior, presenting the same forms and properties everywhere, crystallizable, having a definite chemical composition, and comprising such substances as chloride of sodium, water, carbonate and phosphate of lime, &c., &c.

2d. CRYSTALLIZABLE SUBSTANCES OF ORGANIC ORIGIN, making their appearance only in the interior of organized bodies, and represented by sugar, oil and starch.

3d. ORGANIC SUBSTANCES, sometimes known as albumenoid substances or protein compounds, solely organic in their origin, of indefinite chemical composition, not crystallizable, and including such substances as albumen, fibrin, casein, &c.

In the succeeding three chapters, occupying 45 pages, the above elements are treated of in detail, and in so condensed and succinct a manner, as to place the whole subject with sufficient elucidation before the reader in a very brief space. The fullness of original investigation by which the views here expressed have been confirmed gives additional emphasis to the author's statements and conclusions. In speaking of hæmatine although Dr. Dalton gives the formula for its composition adopted by Lehmann, he says nothing about that author's proof of its near relation to, if not indentity with, hæmato-crystallin, its transformation in extravasted blood into a crystalline body hæmatoidin, the transmutation of bile pigment into a substance similar in appearance and behavior with the latter, or the change of *bilifulvin*, found in stagnated bile, into hæmatoidin by ether. We shall see farther on that these omissions are the consistent results of his views in re-

gard to the blood-globules and crystals, a consistency which with his characteristic clear headedness and logic he uniformly preserves, without committing himself even indirectly in any portion of the work.

As anticipatory, Food is next discussed, and here as we expected Dr. Dalton promulgates the opinions now generally entertained by intelligent physiologists, that all artificial divisions and restrictions are absurd, that inorganic substances, albumenoid matters, starch and oil, in fact everything which nourishes, is essentially food. Liebig's distinctions between histogenetic and calorifacient elements are disregarded, and the only difference in the decomposition and disappearance of the albumenoid and oleaginous substances really existing, is justly asserted to be one of time purely; the transformation taking place slowly in the first, more rapidly in the last. No one alone of these bodies can sustain life; all are concerned in the process of nutrition; the presence of oil is essential to the formation, probably to the decomposition of every tissue; they are each more or less concerned in heat-production, and different articles of food therefore depend for their alimentary value upon the amount of the above substances which they contain, and the proportion in which these are mingled. From personal experiments the author estimates the quantity of food required during 24 hours by a man in full health, and taking free exercise in the open air, as follows:

Meat, - - - - -	16℥
Bread, - - - - -	19℥
Butter, - - - -	3½℥
Fluids, - - - - -	52℥

That is to say, rather less than two and a half pounds of solid food, and over three pints of liquid.

It will be observed that no remarks are offered on the subject of those interesting and curiously related systemic and organic conditions, hunger and thirst; none on tea, coffee and narcotics, which, indirectly, by retarding the waste of the tissues, and directly, by some obscure influence, supply in part and temporarily the place of food, or more correctly speaking

obviate the necessity for its introduction into the system in the usual quantity, and at the ordinary periods.

The whole subject of Digestion is handled in a masterly style, and indicates varied scholarship and excellent judgment. We cannot agree with the writer, however, in all of his views, nor are we sure that it is the most important characteristic of the process of digestion, as established by modern researches, that *different elements of the food are digested in different parts of the alimentary canal by the agency of different digestive fluids.*

Our space forbids anything like detailed criticism or quotation, but we are inclined to regard these questions of specific digestion in the stomach and various parts of the alimentary canal, heretofore considered tolerably well defined, not merely as being re-discussed and examined, but in a state of very remarkable revolution. In a general way we may state, that recent experiments tend to prove that the action of the various digestive juices is supplemental as well as complemental; that they do not interfere with each other's activity to the extent formerly supposed; that some of them enjoy functions, analagous and subsidiary, as well as contrasted, which vary chiefly in the degrees of intensity with which they are exercised, and—which has not been heretofore suggested so far as we are aware, and is offered as an unsupported hint, merely—that their different chemical reactions have a higher anatomical than physiological significance; that is to say, are more related to local structure than specific digestive uses. The experiments of Colin and Corvisart among others give considerable weight we think to the above opinions.

Passing over the chapter on Absortion, we come next to a discussion of the uses and final disposition of the Bile. Dr. Dalton's view here are generally in accordance with those commonly entertained by the best observers. Several series of highly interesting experiments performed by himself are detailed, from which he concludes that the bile is poured into the duodenum in greatest abundance immediately after a full meal; that this is not to take any direct part in the

digestion of food which it effects but slightly, but because the intestinal juices being then present in the greatest quantity can at that time decompose the largest amount of bile; that while it was always present in the small intestines, it could never be detected in the large intestine. Bidder and Schmidt, as is well known determined, by ingenious experiments showing that the quantity of sulphur present in the fæces is far inferior to that contained in the freshly discharged bile, that the biliary substances were actually re-absorbed. Dr. Dalton, however, after examining the blood in the portal veins of dogs, killed at various hours after feeding, by Pettenkoffer's test, has failed to recognize the biliary substances either here or in the venous blood of the general circulation, and hence concludes that "they must undergo certain alterations in the intestine previously to their absorption, so that they can no longer give the ordinary reaction of the biliary substances." These alterations he regards as transformations of a catalytic nature produced by the contact of the bile with the intestinal juices.

The experiments of Bidder and Schmidt, in which they tied the common biliary duct in dogs, and then established a permanent fistula in the gall bladder, allowing its contents to escape externally, are cited in proof of the author's belief—now commonly held—that the bile is not simply an excrementitious fluid, but has some important purpose to subserve in the intestinal canal. All of the dogs in whom the fistula was permanent died of progressive emaciation; the loss of flesh in one animal—whose appetite remained good to the last, amounted to nearly one-half the weight of the body. Dr. Dalton has also minutely and beautifully described Pettenkoffer's delicate test for bile, which is the only one so far given on which infallible reliance can be placed.

Dr. Dalton adopts the views of Bernard in regard to the glycogenic function of the Liver of which chapter ninth treats. His discussion of the question, however, appears to us strangely one-sided. In March, 1857, Bernard presented a paper to the Academy of Sciences on the "physiological mechanism of the formation of sugar in the liver;" in this

he maintains that the livers of dogs fed altogether on meat exhibit the special, peculiar power of creating a glycogenous material analagous to vegetable starch, and ultimately capable of being transformed into sugar. Two months after, M. Sanson of Toulouse addressed to the same body a memoir entitled "the origin of sugar in the animal economy," in which he joins issue with Bernard and establishes, he thinks, the purely external source of this substance, which is to be looked for in the amylaceous principles of the food ingested by the herbivora. This is converted into dextrine, a portion of which undergoes metamorphosis, the rest accumulates in their tissues. The Carnivora using the flesh of the former as food, find in this excess of dextrine "the prime element of the sugar necessary for the accomplishment of their functions, and have not therefore to elaborate it."*

Upon these two papers M. Poggiale, one of a Committee appointed for that purpose, made a report, published in the July number of Brown Sequard's Journal of Physiology.

This paper Dr. Dalton has evidently read, but although he quotes the statement of the commission, that in their experiments glycogenic matter was detected in butcher's meat only once, he suppresses the conclusion of the same paragraph in which they add: "In other experiments we have found it (glycogene) *constantly* in the muscular tissue of dogs in good health; but this interesting fact, which is due to the researches of M. Sanson, does not prove that glycogene is *always* furnished by the aliment." Neither does our author anywhere refer to M. Sanson's paper, its facts, suggestions and conclusions, nor to the experiments which he performed before the commission of the Academy.

Since the volume before us went to press, the experiments of Dr. Pavy of London have reached us, which prove very conclusively, we think, that during life there exists in the liver a substance, to which he gives the name of hepatine, readily convertible into sugar by fermentation, but resisting

*Journal de Physiologie, April, 1858, p. 274.

this change in the healthy organ whilst the animal is alive. As soon as death occurs this resistance ceases, and the liver becomes, especially at high temperatures, surcharged with sugar.

Chapter tenth reviews very cursorily the histology of the Spleen, and is purely non-committal on the subject of its function, of which, as the writer justly remarks, little is known. Its anatomy of course suggests its relation to the other *vascular glands*, and Dr. Dalton thinks that its physiological action corresponds probably with the lymphatic and mesenteric glands in their influence on the blood; the latter supply its place after its removal.

Chapter eleventh on the *blood* furnishes the key to many of the peculiar opinions entertained by Dr. Dalton. The following resumé expresses with sufficient accuracy and detail his individual views in regard to the most important constituent of the circulatory fluid, the blood-globules. Of the *red*, he says: their structure is homogeneous; they consist of a mass of organized animal substance, of the same color, consistency and composition throughout; in some of the lower animals they contain a granular nucleus, but in no instance can any distinction be made out between an external cell-wall and internal cavity.

The *white* are described as being globular in form, not homogeneous like the others, but composed of a substance filled throughout with minute, dark molecules which give them a finely granular appearance. "If treated with dilute acetic acid they swell up and become smooth and circular in outline; and at the same time a separation or partial coagulation seems to take place, in the substance of which they are composed so that an irregular collection of granular matter shows itself in their interior, becoming more divided and broken up as the action of the acetic acid upon the globule is longer continued. This collection of granular matter often assumes a curved or crescentic form, and sometimes various other irregular shapes. *It does not indicate the existence of a nucleus* in the white globule, but is merely an appearance pro-

duced by the coagulating and disintegrating action of acetic acid upon the substance of which it is composed."

The two are to be regarded he thinks as distinct and independent anatomical forms; the white are never converted into the red globules by a gradual transformation; there is no other connection between them, so far as their formation is concerned, than that of juxtaposition. It is not probable that the red globules are produced or destroyed in any particular part of the body; there is no reason for believing that that they are any less permanent, as anatomical forms than the muscular fibres, or the nervous filaments. They undergo, like all of the constituent parts of the body, a constant interstitial metamorphosis, but do not perish bodily, so far as we know, in any part of the circulation. "It is not the *anatomical forms*, any where, which undergo destruction and renovation in the nutritive process; but *only the proximate principles of which they are composed.*

Consistent with the above views is the omission to mention the blood crystals, to suggest any thing in regard to the development of red corpuscles, or their disintegration in the spleen, to notice the identity of hæmatoidin and the coloring matter of the bile (bilifulvin) and the perfect silence which the author maintains in regard to the appearance of the chyle corpuscles, after the passage of that fluid through the mesenteric glands, and the change which these undergo in shape, size and color, towards the termination of the thoracic duct. We do him the justice of asserting that he has committed himself by no inconsistent remark throughout his entire work.

We are aware that Dr. Dalton has been anticipated, and is sustained in most of these views, by a few celebrated moderns, but the weight of testimony and authority are equally against him, and we might fill a page with the names of famous living physiologists who differ widely with him on these points. We have not space to discuss the evidence derived from examination of the blood in various parts of the system during health, or the corroborative testimony afforded by such pathological conditions as that known as leucocythemia

but must content ourselves with stating distinctly his views, merely for the information of our readers. We offer, in passing, however, a single objection to Dr. Dalton's belief, which is to us, apparently, unanswerable. If the red blood-globules are permanent forms, nowhere destroyed, yet being constantly produced, a stasis of the circulation must at some time take place by obstruction of the capillaries with the crowded disks. If they are neither destroyed nor produced, then it is perfectly feasible, by small venesections repeated at long intervals to reduce their number so greatly as to render the blood incompetent any longer to discharge its functions. It is a legitimate deduction, moreover, that when a sudden and almost fatal hæmorrhage has occurred in a robust and healthy person, by which the blood-globules are diminished to the minimum consistent with life, that however perfect, apparently, may be the restoration of such an individual, his anæmic condition must forever remain constant, and the loss of a few ounces of blood, after such an accident must prove inevitably fatal. From one or the other horn of this dilemma, we see no escape.

The succeeding chapters on Respiration, Animal heat, and the Circulation are models of sound philosophy, clear, distinct, and graphic description, and exhibit much scholarship and discriminating judgment. We are happy to add that their conclusions are now generally acquiesced in by all thinking physiologists.

Chapters fifteen and sixteen are devoted to the examination of the secretions and excretions. They cover the whole field of modern investigation thoroughly with the single omission in regard to the fæces already mentioned. Under the head of Urea, Dr. Dalton has failed to notice the important conclusion arrived at by Dr. John C. Draper, whose experiments he quotes, which differs from the views of all who have preceded him in this field, viz: that exercise does not increase, to any extent the amount of urea in the urine.

The second section embraces the general structure and functions of the Nervous System. We have before remarked upon the meagreness of space given to the nerves of the spe-

cial senses; with this single qualification—a most serious one however—we can honestly commend the entire section to the careful study of the student, the portion relating to the pneumogastric, and great sympathetic, are specially worthy of attention.

Section third, occupying 154 pages, and treating of Reproduction, is a monograph of unapproached excellence, upon this subject, in the English tongue. The author of that brilliant paper, contributed to the American Medical Association, eight years ago, on the corpus luteum of menstruation and pregnancy, is familiar with every inch of the ground here traversed, and justly entitled to speak "as one having authority." It gave him then, and since, a trans-atlantic reputation which this portion of his work must largely increase. For precision, elegance and force of style, exhaustive method and extent of treatment, fullness of illustration and weight of personal research, we know of no American contribution to medical science which surpasses it, and the day is far distant when its claims to the respectful attention of even the best informed scholars will not be cheerfully conceded by all acquainted with its range and depth. We have already, so far exceeded our limits, that, to our serious regret, that we can only indicate its contents.

Chapter first, treats of the nature and objects of the function of reproduction, and the origin of plants and animals. The theory of spontaneous generation is well combatted.

Chapter second, on sexual generation, and the mode of its accomplishment; considers plants and animals, their sexual apparatuses, distinct species, and the characters of the sexes.

Chapters three, four, five and six discuss the ovum and spermatic fluid, the female and male organs of generation, menstruation and ovulation, and the corpus luteum of menstruation and pregnancy.

From chapter seven to eighteen, the development of the different portions of the organism, from the primary segmentation of the vitellus to the changes in circulation at birth, are described and illustrated with an accuracy and completeness beyond all praise.

The eighteenth and last chapter briefly notes the developmental changes which occur in the body from the time of birth to the period of puberty.

We thus hastily close the review of a volume which we took up with a keen expectation of pleasure and improvement that has been amply satisfied. As an experimental physiologist of considerable performance and large promise, Dr. Dalton has, for some years, been generally and most favorably known. Young, enthusiastic and laborious, with a fine field for research, and excellent means and position, he has enjoyed no common opportunities and has used them well.

The volume before us is evidently written by one who thinks and looks for himself, and that very independence of belief and method which brings him into constant collision with the critic, holds out the strongest promises and proofs of future progress.

We have been careful to note almost every point of difference between his views and those generally entertained, and, as we believe, best supported; of what importance and how many these are it has been above estimated, but when all the errors—if they be such—are sifted out, a large residuum is left of choice excellence and well worth the garnering. With careful revision, occasional pruning, the filling up of the gaps indicated, and extended experiment and observation, Dr. Dalton may make this not only a most valuable class book, but a permanent contribution to the literature and science of our day; it will, even with the faults of detail pointed out, give him at once a position in the very front ranks of American physiologists.

It only remains to add, that the work reflects much credit on the liberality and taste of its well known publishers. Of its two hundred and fifty-four illustrations, eleven only have been borrowed from other writers, and the wood cuts throughout are exquisitely finished. The paper, type and pressework are also unexceptionable.

BIBLIOGRAPHICAL NOTICES.

ART. VIII.—*Nature in Disease; illustrated in various Discourses and Essays.* To which are added miscellaneous writings, chiefly on medical subjects. By JACOB BIGELOW, M. D., Professor of Mat. Medica in Harvard Univerity, &c., &c. Second edition, enlarged. 12mo., pp. 410. Boston: Philips, Sampson & Co. 1859.

To the physician of reflecting mind, there is no subject more momentous, and at the same time, by reason of inherent conditions, more difficult of ascertainment than that of the power over disease possessed, respectively, by nature and by art. To determine accurately the part performed by nature and that by art, as affecting the issue of disease, is a desideratum which we would fain see attained. How far nature may be trusted, when art should interpose, how much influence is extended by art over nature in regulating and directing her—these are themes of absorbing interest.

The path of investigation, by philosophic minds of the profession, lies in this direction; and we may venture to predict that, henceforth, a reliance upon the power of drugs will be limited by reason. We have perceived our error in placing too much confidence in medical means, and are gradually returning to the habits of our predecessors in the close observance of the workings of nature, aided as we are, by the improved appliances of modern times for the detection and guidance of her ways.

There is something strikingly defective in the constitution or education of the minds of the mass of the profession in the matter of weighing evidence and drawing logical conclu-

sions therefrom. The *post hoc propter hoc*, in other words, hasty generalization, was the bane of previous, as it is of the present generation. A fact is one thing, and the interpretation of the fact quite another thing. Says a recent reviewer: "The wide and uncontradicted assertion of a fact alone gives it the stamp of genuine value. Let us consider this by reference to a few examples. Is it not certain that there is somewhere a fallacy in the conclusion that mercury is the proper remedy for secondary syphilis, when we find that half the profession administer mercury for it, and half the profession regard salivation as a positive poison in this disease? Must not there be something wrong, either in the observation which teaches us that without salivation there can be no hopeful cure for pericarditis, or in the observation which demonstrates that salivation so used is a very hurtful appliance? Can their observation be received as true, who tell us that quinine infallibly cuts short continued fever, so long as the observation of others contradicts the assertion? We must assuredly act contrary to all rules of reason, if we accept as a true cure any method of treatment which does not bear the test of experience, as above enjoined. Wherever serious discordance of opinion reigns concerning the influence of a remedy over disease, we may be sure that its real effect has yet to be proved. We admit as an established fact the influence of quinine over ague, of hydriodate of potass over periostitis, of the salts of potass over acute rheumatism, because there is no dissonance of opinion respecting the influence of the remedies over the diseases; and consequently the facts obtained are the result of a true experience."

It has been said that figures cannot lie. We maintain that in medicine they are made to lie, and to lie enormously. Who is there, when attempting to prove the efficacy of a mode of treatment which he has originated, that cannot array, in support of his statement, any number of cases of success with it? In the last issue of this Journal there is an extract from a paper in Braithwaite's Retrospect, in which Dr. Black reports 200 cases of Asiatic Cholera in all of which arsenic produced a speedy and permanent cure! Dr. Begbie

has never seen arsenic fail in effecting the cure of cholera, &c., &c. So, Drs. Wildman and Harris of our sister city, Savannah, met with no failure in the use of the Tinct. Ferri. Mur., in the epidemic yellow fever of 1854, yet both died of that disease, specific in hand!

The field of inquiry, which forms the text for these remarks, points as prominent among its cultivators, to the names of Sir John Forbes, Drs. Laycock, Holland, Hughes Bennett, in Great Britain, and to those of Dr. Jacob Bigelow, Bartlett, W. Hooker, Cotting and others in this country. The moral courage exhibited by Dr. Forbes, whilst in editorial conduct of the leading medical journal of the world, in questioning the degree of control of remedial means over disease, at a time when medicines were addressed (as, indeed, they now frequently are), to the relief of secondary and consequential as well as primary conditions—the old adage *sublata causa, tollitur effectus* being entirely overlooked or ignored—and when polypharmacy was rampant, is worthy of the highest praise and admiration. We refer to his work entitled, "Homœopathy, Allopathy and Young Physic," and subsequent productions during and since his controversy with Dr. Henderson.

As far, however, as the modern view of the subject is concerned we must enter a claim for priority in its pursuit in favor of our countryman, Dr. Jacob Bigelow, who in 1835, long anterior to the time of appearance of Dr. Forbes' work, read before the Massachusetts Medical Society a paper on "Self-limited Diseases" in which the author essays to show how much control art has over disease. But if we wish to make the award to whom it is due, we must go to the last century—to the sagacious Dr. Pitcairn, whom we find enunciating the same proposition in treating of the management of continued fever, that *fever cannot be cured but may be conducted to a favorable termination;* a proposition, the truth of which is applicable to a great many other diseases besides fever.

This volume of Dr. Bigelow is composed of papers on various subjects, which have emanated from his pen at dif-

ferent times. The most prominent of them are, that on "Self-limited Diseases," "The Treatment of Disease," "The Medical Profession and Quackery," and "The Burial of the Dead."

Dr. Bigelow, it seems to us, is entirely too latitudinarian in his view of self-limitation in disease. In illustration of what he means by self-limitation, he says: "We may suspect those complaints to be self-limited, in which it is observed that the unwary and sceptical, who neglect to resort to remedies recover their health without them." Again: "We may attach the same suspicion to cases in which the supposed cure takes place under chance applications or inconsiderable remedies; as in the empirical modes of practice, on the one hand, and the minute doses of the homœopathic method on the other." "Lastly, we may apprehend that cases are fatally self-limited when enlightened physicians die themselves of the diseases which they had labored to illustrate—as in the case of Corvisart, Laennec, Armstrong and others."

The *non-sequitur* here is so palpable that it is superfluous for us to do more than to call attention to it. According to the first and third of these definitions of self-limitation, all diseases are self-limited, for if left to themselves their course would be limited by recovery or death. In our humble opinion, the property or attribute of self-limitation in disease should refer to duration, uniformity of course and phenomena generally, rather than to spontaneity of termination. *Time*, as affecting its course or progression is, we apprehend, an important element in constituting self-limitation. Variola, Scarlatina and Rubeola, for instance, run a definite, nearly uniform course, their rate of progress, increment, culmination and decline varying very slightly in any number of cases. So is it with other diseases of the class of blood-poisons—to which we think the term should be restricted—typhoid or enteric fever, typhus fever, yellow fever, pertussis, hooping-cough, vaccinia. With regard to erysipelas, (which the author places in the category of self-limited diseases,) not only the pathology of the disease is not accurately understood, but its duration is uncertain. He classes it with the self-limited

diseases, because he believes that it cannot be prevented from extending from one place to another, as for example, from the head to the neck. At the time at which he wrote this paper—1835—it was thought impossible to arrest the ordinary superficial erysipelas, that affecting the skin and subcutaneous areolar tissue. Since that day, it has been ascertained that certain means will, in the great majority of cases, prevent the extension of the inflammation beyond the point of their application. These, as every surgeon knows, are Nitrate of Silver, Iodine, Cantharides, &c., locally applied. That in rare cases the inflammation is not circumscribed by those means as desired, is explained by the fact that the morbid state had seized upon parts beyond, before the application was made; the inflammation, as denoted by redness, heat, burning, &c., showing itself subsequently. But admitting that there may exist a difference of opinion in respect to the possibility of controlling the progress of erysipelatous inflammation by remedial means, by what process of reasoning can Dr. Bigelow class dysentery with self-limited diseases, which he does in a note at the bottom of page 32? That dysentery as well as its congener and frequent antecedent, diarrhœa will get well, spontaneously, like a great many other diseases, that is to say, without the interference of art, every one is fully aware. But who can tell how long, when once established, either will last? Is there any limit to their duration? Have they not been known to continue during weeks, months, years, nay, even the whole life time of the individual, whether left to themselves or combated by remedial means? Is this the character of the strictly self-limited diseases? Can variola, scarlatina, rubeola and the like, be prolonged, even if desired, beyond the time to which nature has set the limit?

With the exception which we have taken to the too wide sense of the epithet self-limitation, we invoke in the most earnest manner, the attention of all to the wisdom of the reflections and suggestions contained in this and the works of the same scope and design of the other writers whom we have named, with the belief that the instruction which will

be derived from their study, if undertaken without bias, will enure to the honor and usefulness of the profession, and to the welfare of those of whose ills it is our province to have the care.

Let all ponder the admonitions implied in the following extract:

But that the usefulness of our profession may extend, our knowledge must go on to increase; and the foundation of all knowledge is truth. For truth then we must earnestly seek, even when its developments do not flatter our professional pride, nor attest the infallibity of our art. To discover truth in science is often extremely difficult; in no science is it more difficult than in medicine. Independently of the common defects of medical evidence, our self interest, our self-esteem, and sometimes even our feelings of humanity, may be arrayed against the truth. It is difficult to view the operations of nature, divested of the interference of art, so much do our habits and partialities incline us to neglect the former, and to exaggerate the importance of the latter. The mass of medical testimony is always on the side of art. Medical books are prompt to point out the cure of diseases. Medical journals are filled with the crude productions of aspirants to the cure of diseases. Medical schools find it incumbent on them to teach the cure of diseases. The young student goes forth into the world, believing that if he does not cure diseases, it is his own fault. Yet when a score or two of years have passed over his head, he will come at length to the conviction that some diseases are controlled by nature alone. He will often pause, at the end of a long and anxious attendance, and ask himself how far the result of the case is different from what it would have been under less officious treatment than that which he has pursued; how many in the accumulated array of remedies, which have supplanted each other in the patient's chamber, have actually been instrumeutal in doing him any good. He will also ask himself whether, in the course of his life, he has not had occasion to change his opinion, perhaps more than once, in regard to the management of the disease in question, and whether he does not, even now, feel the want of additional light.

Medicine has been rightly called a conjectural art, because in many of its deductions, and especially in those which relate to the cure of diseases, positive evidence is denied to us. We are seldom justified in concluding that our remedies

have promoted the cure of a disease, until we know that cases exactly similar in time, place, and circumstances, have failed to do equally well under the omission of those remedies; and such cases, moreover, must exist in sufficient numbers to justify the admission of a general law, on their basis. Nothing can be more illogical than to draw our general conclusions, as we are sometimes too apt to do, from the results of insulated and remarkable cases; for such cases may be found in support of any extravagance in medicine; and if there is any point in which the vulgar differ from the judicious part of the profession, it is in drawing premature and sweeping conclusions from scanty premises of this kind. Moreover, it is in many cases not less illogical to attribute the removal of diseases, or even of their troublesome symptoms, to the means which have been most recently employed. It is a common error to infer that things which are consecutive in the order of time have necessarily the relation of cause and effect. It often happens that the last remedy used bears off the the credit of having removed an obstruction, or cured a disease, whereas in fact the result may have been owing to the first remedy employed, or to the joint effect of all the remedies, or to the act of nature, uninfluenced by any of the remedies.

ART. IX.—*On Excision of the Knee.* By GEORGE MURRAY HUMPHREY, Surgeon to Addenbrooke's Hospital, and Lecturer on Anatomy and Surgery in the Medical School, Cambridge. London. 1858.

This small, but very interesting pamphlet of twenty-six pages, contains the report of 13 cases of knee joint disease in which excision of the heads of the bones was performed with marked success. Only one patient died after this capital operation, and although in four of the cases amputation was necessary for the removal of disease which continued in the old seat, life with health was saved in all. Mr. Humphrey concludes from his experience that this operation is not attended with much danger to life, as in the 13 cases reported, very little constitutional disturbance followed. He

attributes his success to the chronic condition of the disease and to the class of patients upon which he operated, agricultural laborers. The limb in each case after recovery has been a very strong and useful member. The loss of a small portion of the length of the limb was rather an advantage in connection with the anchylosis, facilitating walking and rendering the gait of the patient easy. He makes a very happy suggestion for operations upon growing bones, that section should be made through the epiphyses and not through the shaft of the bones; so that growth will continue and deformity from much shortening prevented.

ART. X.—*New Surgical Treatment for Malformations of the Urinary Bladder.* By DANIEL AYRES, M.D., Brooklyn, N. Y.

Dr. Ayres reports the mode in which he successfully removed the shocking, and often considered incurable, deformity exstrophy of the bladder, by borrowing a flap of skin from the abdomen with which the exposed organ has been successfully protected. In this case which was of the very worse description, the pubic bones were separated from each other for three inches, the posterior surface of the bladder with the ureters forming an oval and very sensitive tumor. The indications proposed were,

1st. To form an anterior wall for the exposed bladder.

2d. To restore the urinary canal.

3d. To establish the anterior fourchette of the vulva.

4th. To supply means to prevent an existing prolapsus of the uterus and to collect the renal secretions.

The report states that the result of the operation was better than had been anticipated, although no particulars are entered into in this paper to inform the reader whether the indications desired had been obtained.

ART. XI.—*Contributions to Operative Surgery and Surgical Pathology.* By J. M. CARNOCHAN, Professor of Surgery in the New York Medical College, &c., with illustrations drawn from nature. Philadelphia. Lindsay & Blakiston. 1858. Part 2d.

We have already had occasion to refer to the bold conception, as well as the successful completion of many most difficult operations in the hands of Prof. Carnochan. The present number of his contributions adds still to that list. The first article describes the removal of the entire ulna for chronic disease, an operation that has been very seldom performed. After cicatrization very little deformity existed whilst the functions of the arm for prehension, flexion, and extension, both at the elbow and wrist, with pronation and supination could be performed with facility; a proof of the wonderful adaptive powers of nature, even when apparently irreparably crippled.

The larger portion of this contribution is occupied with the surgical treatment of those fearful cases of Tic. Douloureux, in which life is a torment, that require a very large share of moral courage to endure; in which all medication having failed to offer the slightest amelioration, self-destruction continually intrudes itself as the only means of relief from such intense suffering. Dr. Carnochan conceived the bold design of following the trouble to its source and extripating the faulty nerve, thus eradicating the disease. In all the cases reported simple section, even, when frequently repeated, failed of its object. The face was opened, the antrum transfixed by a trephine, and the infra-orbital nerve with its ganglionic connections in the spheno-maxillary fossa, behind the upper jaw, was removed, and with it came again sunshine and happiness, a new life budding forth, the unspeakable joy of living without pain, that rapid destroyer of mind and body. "Risk nothing, gain nothing," is a trite proverb, which the surgeon must constantly apply; in counteracting the frailties of humanity, when governed by observation and judgment, it is the means of opening many new paths of usefulness.

ART. XII.—*Illustration of Typhus Fever in Great Britain.* By J. B. UPHAM, M.D. (*From the Boston Medical and Surgical Journal.*) Boston: David Clapp. 1858. pp. 46.

An Essay on the Climate and Fevers of the South-western, Southern Atlantic and Gulf States. By J. C. HARRIS, M.D., of Wetumpka, Ala. (*From N. O. Med. News.*) New Orleans. 1858. pp. 32.

The author of the paper on typhus fever, Dr. Upham, having had much experience with this "maculated typhus or ship fever," as it appeared in Boston, Mass., published, ten years since, his views respecting its prevalence as an epidemic at the South Boston and Deer Island Hospitals. In the summer of 1853, he prosecuted further researches in this direction, in Dublin, and at the London fever hospital, during a few weeks attendance upon the services of its physicians, Drs. Southwood Smith and Tweedy, Dr. Sankey being then the resident medical officer. Whilst there he devoted himself to describing the natural history of the disease, detailing, as minutely as possible, every observable feature which it presented, and associating this with the treatment adopted by Dr. Smith. Now, this is precisely what medical men, earnestly seeking after knowledge, desire, viz: an accurate, full, *complete* history of a disease, leaving out no circumstance or accident belonging to it. The cases which the author appends, though few in number, he regards "as *models* of the affection in its various forms of severity." He says that he endeavored to follow the maxim of Sydenham so aptly quoted by Jenner, to "note them accurately in all their minuteness; in imitation of the industry of those painters who represent, in their portraits, the smallest moles and the faintest spots," and he makes no apology for what may be called by some their tedious minuteness and peculiarity. In a note the author adds:

It was here that Dr. Wm. Jenner had, a short time previously, carried on and perfected his elaborate investigations of fever, from which resulted his well-known classification, into

four distinct and seperate diseases, of the affection so long confounded under the term "continued fever" by some of the most eminent authorities of Great Britain. I had been led to the same conclusions, he adds, so far as relates to the non-identity of typhus and typhoid, by the evidence forced upon my senses in the epidemic of 1847–48, when the two diseases not unfrequently lay side by side in the wards of the same hospital, and their phenomena were noted, *pari passu*, at the bedside, and in the deadhouse.

These reports would have been still more valuable if the author had been as attentive to the pathological indications afforded by the urine, as Prof. Joseph Jones, of Georgia, in his recent reports on intermittent and remittent fevers. The present paper is deficient in this respect, and the omission is a serious one.

In order that our readers may have an opportunity of examining one of the cases which recovered, we select case VI, as it presented one or two complications; they will be struck with the simplicity of the treatment, the avoidance of mercury and purgatives of any kind, and the improvement under the use of stimulants. In carefully reviewing this, and in fact, all the cases detailed by the author, the writer of the present notice has one observation to make, which is the singular avoidance of the use of blisters in many of these cases, accompanied by dry tongue, sordes, delirium, etc. From his experience in a large institution where he has typhoid fever frequently under his care, in every case treated successfully, save one, blisters to the abdomen and neck have seemed to contribute much to their favorable termination. An examination of the cases of typhus here detailed would imply that in this disease, even in a most aggravated form, blisters, whether as local revulsives or stimulants, were by no means essential. This point certainly requires further investigation. We may remark farther, before extracting the case, that there are two "fever mixtures" referred to in the pamphlet before us; one called the "mild," and the other "the strong fever mixture," a choice between which seems to depend upon the intensity of the attack. The following is the formula for the first: ℞ liq. ammon. acet. ʒii., mist camph., aq. distill, aa ℥ss.

For the "strong," ℞ ammoniæ sesquicarb. gr. v., mist. camphoræ ℥iss. m. ℥i. quâque quartâ horâ.

CASE VI.—Without previous history or known cause; loss of appetite—depression—dulness of intellect and senses—deafness—furred, swollen, not uniformly dry tongue—no sordes—suffused eyes—dusky skin—thirst—abundant mulberry rash—slight cough—somnolence—surface moderately hot and dry, sensitive—respiration irregular, laborious, imperfect, accompanied with moaning, 36 to 44—mucous râle—dulness on percussion at base of left lung—slight tympanitis—nervous and muscular agitation—tremulousness, subsultus at wrists—picking at bed-clothes—extreme prostration—involuntary stools and urine—muttering, raving delirium—dark, livid, petechial spots—pulse 96 to 140, moderately full, soft, compressible—disappearance of rash—convalescence and recovery.

Bridget Fitchgerald, 35 years of age, was admitted into the London Fever Hospital on the 30th May, 1853. She was brought from the work-house in the Strand, in a state of much prostration. Her previous history could not be learned. This patient first came under my notice on Friday, 3d of June. The record of her case previous to this time is gathered from the Hospital Register, and is as follows: May 30th (day of admission), there was dulness of intellect and of the special senses. General powers were feeble; patient unable to turn in bed; answers questions with hesitancy and effort; tongue is furred and dry; no appetite, much thirst; some cough; pulse 134; body covered with abundant mulberry rash. To have the strong fever-mixture (mist. carb. ammon.), wine and beef-tea. On the 2d, she is reported to have slept a good deal; general expression heavy, features dull and relaxed; patient complains of feeling cold; skin dusky; tongue dry; one stool; pulse 134, rash darker, persistent. To have four ounces gin.

June 3d.—Patient has slept during the night. There is now much pain in the head and limbs; dusky hue of face; suffused eyes; tongue is swollen, dry and crusted along its centre, furred at the edges, red at tip. There is some cough; no appetite; urgent thirst; no stool; urine scanty, passed unconsciously; rash generally diffused, of mulberry hue, fades, but does not disappear, under the finger; the pulse is 128, weak, compressible; there is great prostration; decubitus dorsal. Vin. albi, ℥x.; strong fever-mixture.

4th.—Passed an unquiet night; general expression of prostration; can be roused with great difficulty, and then understands questions partially; moans constantly; eyes

much suffused; face dusky; tongue loaded with white fur; respiration 48, irregular, laborious; chest resonant; abdomen natural; pulse 140; a little subsultus at wrist; skin not very hot; spots abundant, dark, persistent, tending to petechial; decubitus dorsal, slips down in bed. Strong fever-mixture—wine—gin—beef-tea—milk and water, *ad libitum.*

5th.—General aspect is that of extreme depression; decubitus dorsal, slips in bed; picks and pulls at the bed-clothes; moans and talks incessantly; intelligence obscured; face dusky; eyes much suffused; conjunctivæ injected; tongue dry, loaded with a brownish-white coat; no sordes; some cough; stools and urine in bed; pulse 140, soft, compressible; increased subsultus; surface of body fuliginous; spots dark, persistent, petechial. To have anodyne draught (tinct. hyoscyam., ʒ i.) *hora somni;* strong fever-mixture; wine *ut heri.*

6th;—Has slept but little; decubitus dorsal; prostration complete; delirium at night; constant and universal tremulousness; subsultus at wrists, picks at the bed-clothes; face flushed, dusky; eyes greatly suffused; tongue protruded with difficulty, furred, (not loaded); much cough; difficult expectoration; a frothy mucus collects around the mouth; universal mucous râle; the dulness on percussion is inconsiderable; respiration 36, laborious; stools and urine in bed, passed unconsciously; some sensitiveness of abdomen to pressure; pulse 120, irregular, very soft and compressible, spots dark, persistent, To have gin, four fluid ounces—wine, six ounces—strong fever-mixture—anodyne draught—beef-tea—milk and water, at first cold, then warm, *p. r. n.*

7th.—Is reported to have slept but little if any, and to have talked incoherently and raved all night. There is extreme prostration; great nervous agitation; constant tremor; face dusky, fuliginous; eyes are less suffused; tongue protruded irregularly and with much difficulty, overspread with a thin white fur, moist; no sordes; coughs and expectorates an abundant white frothy mucus. Respiration very irregular; a little tympanitis; one stool, in bed; urine in bed. General sensitiveness of surface; odor of body peculiar, very perceptible; pulse 120, irregular, soft, moderately full.—Treatment of yesterday to be continued and pushed vigorously.

9th.—Is said to have slept much better, without rambling or muttering; no delirium during the night; decubitus dorsal. Patient has this morning for the first time manifested some intelligence, asked to get out of bed to be placed on the close stool; eyes are clearer; face less flushed; tongue

has white thick coat extending along its centre, shading to brown, is protruded with much less difficulty. Resp. 44, more easy, accompanied with occasional cough; some expectoration, easy; diminished mucous râle; tympanites inconsiderable; two stools, is conscious of them; urine plentiful, highly charged with sediment; skin lighter in hue, neither dry nor hot; spots much less evident than yesterday, lighter; decubitus is still dorsal; some nervous agitation, twitching of tendons; slight deafness; moans occasionally; no pain; complains of great weakness only. Has some relish for beef-tea. To continue same treatment. On the 10th the general appearance of the patient was better—eyes clear; tongue moist, covered with a yellowish-brown coat, increasing posteriorly, edges and tip red. Resp. 38, easy, accompanied with some moaning; cough loose, expectorates easily; pulse 104, regular, weak, compressible; a little tremulousness at wrists; no pain; less thirst; some appetite; spots fading, almost gone.

11th.—Is reported to have slept well; complains of weakness and exhaustion; intelligence good; has some appetite, bears nourishing diet well; face still flushed and wears an expression of suffering—says she has no pain. Tongue moist—with a yellowish white fur upon base and along its centre, clean and natural on sides and at edges and tip; respiration 36, drawn apparently from top of lungs—accompanied with some moaning; mucous râle anteriorly; much dulness on percussion at base of left lung posteriorly; two stools, natural; urine free, dark colored, out of bed. Skin cool and moist; spots still apparent by inspection on abdomen, evident also on back; pulse 108, weak, soft, compressible. Emp. ves. to base of left lung posteriorly—wine and the strong fever-mixture—sago and eggs. On the 12th there was a noticeable improvement in the signs and symptoms; eyes clear; tongue clean, except a light fur at base; complains of weakness and pain in chest; respiration still accompanied with slight moaning; less dulness at base of left lung; breathes deeper, easier; no stool; urine free, lighter in color; spots discernible only on abdomen; pulse 100, regular. On the 13th, the mucous râle was inconsiderable; no noticeable dulness on percussion; skin moist and cool; pulse 100, regular, of better volume and strength; tongue cleaning; appetite improving. On the 16th, the pulse was 96, regular, of good volume; only occasional cough; no pain; good appetite. Convalescence seems fully established.

This case offers a good example of the disease in its severe

form, without complications, except the trifling tympanitis for a day or two, and the inconsiderable but anoying chest affection, as manifested by the physical signs, most marked on the 11th June. It showed well the depressing type of the disease, as commonly found—and the nature of the treatment adopted in such cases, the efficacy of which, in the instances just cited, cannot be questioned.

After so long an abstract we cannot make farther draughts in such a wholesale way, but the ensuing summary we recommend as an admirably full as well as condensed statement of the characteristic feature of the disease; it is worthy the attention of those who wish to compare the disease with the cases of typhoid fever so much more common among us. The writer of this notice is inclined to believe that in the city we more oftener meet with bilious remittent fever assuming a typhoid character, with dry tongue, delirium, etc., than with pure uncomplicated typhoid, scarcely, if ever, with typhus, and that these cases being much more malarial in their character than typhus, or fever depending upon crowd poison. really do require a small amount of mercurial at their onset; where the glandular secretions seem impaired, a single dose is sufficient, followed by quinine or sulph. cinchoniæ, wine or brandy, turpentine and ammonia if there is intestinal tenderness, and flatulence, with blisters to the nape of the neck or stomach, if the intellect is at all affected. But in his experience, and it has been sufficiently extensive, no additional purgatives were required or even used, and he treats many cases of malarial fever without administering a dose of calomel, using cinchonia, etc.,—the stimulants, in large amount, often more than a quart of whiskey in two hours and a half, being absolutely required in malarial fever assuming a typhoid form, together with nourishment and quinine, or cinchonia, with aromatic sulphuric acid and morphia. There were patients with malarial fever, assuming a typhoid form, and other cases of true typhoid fever in which, when the tongue was persistently dry and seemed to indicate that the glandular and secretory organs were locked up, that a few grains of calomel occasionally administered, seemed, with the stimulants, to be unquestionably useful and necessary.

Managed with the above agents, he has had but one death, in a patient who entered the institution in a speechless condition. It is his impression that the cases of typhus fever occurring in his care in the alms house of the city, several years since, when the institution was much crowded, bore a strong resemblance to, if they were not identical with, what he had seen of typhus, which was abundant in the New York hospital during the summer of '47.

The following sketch gives us an idea of "life in London" among the poor who are not slaves.

"In company with the chief of the detective police, I visited one of the most noted of these fever localities in London. A sketch from nature may not be inappropriate in this connection, and will serve to impress the facts above stated. The visit was made at night. A drive of a mile or two from Charing-cross took us into an ill-lighted, irregular-shaped court, in the midst of a densely populated portion of the city. The place was badly paved; the ground was uneven, and the whole region redolent of filth. From this court, as from a centre, crooked and narrow streets straggled out into the darkness. Down one of these we plunged, taking the middle of the way, which was also the gutter. Coming abruptly to what appeared to be the end of a *cul de sac*, we descended some steps and stooped beneath the archway of a portal. This was the entrance to one of the poorest of the cheap lodging houses of London—the temporary shelter of the most wretched and destitute wanderer, where for a penny ha'penny a head, a bed and a roof is furnished for the night. The detective knocked authoritatively at a side door, which in a few moments, was unbolted and thrown open. A most villanous stench was our greeting. The floor of this room was several feet lower than the level of the street, so that drainage was out of the question. We entered without ceremony, and saw, by the light of a lamp suspended from the ceiling, the limits of an irregularly square room, some fifteen by sixteen feet measurement on the floor, by scarcely six feet in height. The inmates, twenty-two in number, were sleeping when we entered. One or two of them started up—gazed vacantly at us, for a moment, and immediate sunk back drunk with sleep and the narcotism of the foul air. They lay in groups, in all attitudes, in beds and upon the floor—men, women and children promiscuously. I observed four men in one bed, muscular and brawny subjects, having only a single

sheet, which served, the time being, both shirt and covering for them all. Upon a mattrass hard by, lay two men and one woman, in a similar state of *deshabille.* Then came half a dozen boys, closely packed in a row upon the floor. Young girls and little children, with tattered and scanty garments, occupied every inch of space that was left. All were slumbering heavily. On lines of ropes, crossing and re-crossing each other and attached to hooks in the walls, were suspended the ragged, still crawling garments of the sleepers. It was not a place for long tarrying. The atmostphere of the room seemed, in the hot night, the very condensation of pestilential foulness. In the few moments of our stay it made an impression, not on the senses only, but upon the brain, the effect of which was perceptible the next morning in an intense headache and vertigo, which lasted for most of the day. How beasts, much more human beings, can endure a night of it, is a mystery. I have visited the crowded between-decks of an emigrant ship, on its arrival after a long voyage, and only there have found a parallel. Can we wonder that disease, in its most aggravated form, comes forth from such dens as these? Philanthropy and sanitory laws, it is true, have done much in England of late years to mitigate these evils; but much, very much, remains to be accomplished.

The government of the London Fever Hospital have published the following *rules* which may, with advantage, be deseminated. They are intended to be observed in the apartments of those who are confined by the fever:

"1. It is of the utmost importance to the sick, and their attendants, that there be a constant admission of *fresh* air into the room, and especially about the patient's bed. The door, or window, should, therefore, be kept open both day and night, care being taken to prevent the wind from blowing directly on the patient.

"2. Attention to cleanlines is indispensable. The linen of the patient should be often changed; and the dirty clothes, &c., immediately put into fresh cold water, and afterwards well washed. The floor of the room must be cleansed every day with a mop, and all discharges from the patient immediately removed, and the utensils washed.

"3. Nurses and attendants ought to endeavor to avoid the patient's breath, and the vapor from the discharges; or, when that cannot be done, they should hold their breath for a short time. They should place themselves, if possible, on that side

of the bed from which the current of air comes and carries off the infectious vapours.

"4. Visitors must not go near to the sick, nor remain longer than is absolutely necessary; they should not swallow their spittle, but clear the mouth and nostrils when they leave the room.

"5. No dependence must be placed on vinegar, camphor or other supposed preventatives; which, without attention to *cleanliness* and admission of *fresh air*, are not only useless, but by their strong smell, render it impossible to perceive when the room is filled with bad air or noxous vapors."

The following observation, made by Dr. Jenner, is also worthy of note.

The headache ceases on from the seventh to the tenth day; and if not before, almost invariably as soon as delirium commences. He further says, "this is a point of great practical importance, for if headache is voluntarily complained of by the patient, or if even declared to be severe in answer to the question of the physician, after delirium has commenced, strong suspicions, to say the least, of inflammatory action within the cranium should be entertained, and remedies adopted with that view of the case; while headache, before delirium has commenced, is in itself not the slightest proof of increased vascular action within the cranial cavity."

Postmortem examinations are asserted to reveal no adequate cause for death in typhus. "It is the blood alone that is evident to be essentially and vitally diseased," complications may arise affecting materially its aspect, duration, etc., these, however, are the accidents of typhus, and are in no way to be confounded with its essential nature.

Dr. Corrigan mentions inflammation of the lymphatics, of either upper or lower extremities, which sometimes prevails epidemically, as worthy of the earliest attention of the physician. Also adhesive phlebitis is among the troublesome seuelæ, affecting most frequently the inguinal and femoral veins. "Its first approach should, therefore, be carefully watched, the patient confined at once to the horizontal position, the groin leeched, the limb elevated, and treated by mercurial inunction, fomentations, bandaging, etc." Both jaundice, and inflammation and suppuration of the parotid glands, com-

plications of typhus, we have noticed in yellow fever likewise.

From the author's conclusion which we append, it will be seen that *mild* treatment, advisable in most diseases, of the present day, is by no means to be confounded with no treatment, a distinction which many persons fail to make. Many make a boast of giving nothing with success, when it will be found, upon examination, that they have used some of the ordinary remedial measures which were of material assistance in the management of the cases. Because they wisely do not purge their patient to death, but supply him with ammonia, stimulants, blisters, nourishment, they call this the "*medicine expectante*" system. Patients *do not* recover even in the zymotic diseases, the simplest in type, by being absolutely let alone, although the mildest elementary agents are sufficient, with the assistance of tonics, ventilation, baths, cold water and nourishment.

"Adynamia is there, as it was here, the chief element of the disease, requiring for its management the adoption of the same principles of treatment. Yet such management is the farthest possible remove from a senseless and empirical routine. And I cannot more appropriately close this imperfect sketch of typhus fever in Great Britain, than by an extract bearing on this point, from the works of the eminent observer and writer I have so often quoted in the course of these papers. Says Dr. Jenner,: "In no disease is the advantage of refraining from meddling more clearly displayed than in typhus fever. In no disease is the prompt use of powerful remedies more clearly indicated than in typhus fever. It is in determining when to act, and when to do nothing, that the skill of the physician as a curer of disease, or, rather, with reference to fever, as an averter of death, is shown. Interfere, bleed or stimulate, when nothing should be done, and the patient, but for you safe, is lost. Refrain from depletion or withhold wine, when the one or the other is required, and the patient sinks into the grave from which a judicious treatment might have saved him."

The pamphlet by Dr. Harris contains a good deal of information on the topography and diseases of many localities in the South and West, with reference to the prevalence of Remittent, Intermittent, Yellow Fever, etc. The author's

etiology of the last disease is, we are happy to believe, not generally acquiesced in by those familiar with it.

To show the *malarial*, certainly in the opinion of Dr. Harris the epidemic origin, of yellow fever, he says:

Believing there is no intelligent Southern physician who has taken the trouble to examine the subject, that pretends to deny the *malarial* origin of intermittent fever, we conclude if anything more were necessary to show a similar origin of the other varieties than what is already before the profession, that the history given us by Dr. Heustis of the bilious-remittent, or yellow fever, that prevailed in Cahawba in the summer and fall of 1821, before steamboat navigation had been introduced on the Alabama river; and that of Doctor Gantt, of its occurrence in the Pleasant Valley during the fall of 1824, and when there was no yellow fever in Mobile, are conclusive, and ought forever to set the subject at rest.

On the subject of typhoid fever, he notices the tendency of continued fever to assume intermittency and of intermittents and remittents to run into a typhoid condition. The latter we have often noticed and think that the judicious use of occasional doses of calomel is highly serviceable here. The observation of Dr. Harris in regard to its necessity in visceral engorgements fully sustains our own opinion as expressed in the first part of this paper.

Continued (Typhoid) Fever—As the records of medicine are full of proof in support of the fact that *continued fever*, originating in southern latitudes not unfrequently runs into, and assumes a remittent or even *intermittent type;* and as remittent fever sometimes during its progress, either from increased *congestion*, or the supervention of inflammation in some one or more of the principal organs of the body, loses its *paroxysmal features*, and degenerates into a low grade of febrile action; together with the great similarity of the symptoms, tendencies, terminations, and post mortem lesions of the former with those of the *continued stage of remittent fever*, we infer that they are essentially and to all intents and purposes the same. Nay, more, that the high temperature, high dew point, and sudden changes of our climate, predispose to *visceral engorgements;* and that these obscure congestions thus created, sometimes from the commencement, in those of feeble constitution, or long resident among us, entirely *prevent* or *mask* the remissional features of our fevers,

is a fact, we presume no one at all familiar with the endemic influences of our climate will pretend to deny; but that either of these varieties of fever, are ever, within the limits assigned in this essay, so far changed as to present us with anything more than a *continued fever*, the result of these causes, and with affinities and resemblances much stronger marked in favor of our *autumnal remittents*, than their supposed primary type as met with in the cities of Glasgow and Dublin, we think the conditions under which they originate and prevail *utterly forbid.*

We can't think, though that the "*typhoid* fever described by Louis and the *typhus* fever, not only of the British Isles, but also of the rest of Continental Europe are the same disease," but believing rather that this point has been definitely settled by Jenner; "and that the theatre of their origin and greatest prevalence, is in countries and localities where the malarial element has never, within the memory of man, been present in sufficient quantity to produce even the most mild and simple form of *periodical fever*; together with the fact, that under our genial sun and democratic institutions, when imported in the persons of emigrants and landed on our shores, they fail to propagate themselves and soon die out; we conclude that the term *typhoid* (which means something typhus) cannot with any degree of propriety be applied to any other period of either our pure or modified forms of *fever* than their *latter stages.*

ABSTRACTS AND TRANSLATIONS
FROM FOREIGN AND DOMESTIC JOURNALS.

PHYSIOLOGY.

Physiology of Digestion and Nutrition.—Busch furnishes (*Virchow's Archiv*, vol. xiv., 1858) valuable information on the physiology of digestion and nutrition in general, derived from observations and experiments made on a female patient, aged thirty-one, in whom, through external violence, the intestinal tract was divided into two separate portions—the superior consisting of the stomach, duodenum, and a small part of the jejunum—the inferior of the larger part of the jejunum and the whole of the ileum and colon. The author had therefore the rare opportunity of observing the phenomena, of a fistula of a jejunum in an otherwise healthy human subject. The accident had taken place six weeks before the admission of the patient into the University Hospital at Bonn; the wound in the abdominal walls was healed, with the exception of the fistulous opening. The woman had a voracious appetite, and devoured large quantities of food; the greatest part of this, mixed with the gastric, pancreatic, and intestinal secretions of bile, passed away from the fistulous opening in the jejunum; no particle of it entered the contracted and too distant aperature of the inferior portion of the intestinal tube. Extreme emaciation, loss of strength, tendency to sleep, and hoarseness, were amongst the principal constitutional phenomena.

The plan pursued by Busch, in order to restore the strength of the patient, consisted in the introduction of strong broths with eggs beaten up in them, occasionally also of pieces of boiled eggs and meat, into the inferior portions of the digestive tube. The strength of the patient increased rapidly under this treatment, and she gained also considerable in weight. This fact is of great interest, as it corroborates the view, that nutrition can take place—for some time, at all events—without the admixture of the gastric, pancreatic,

duodenal, and hepatic secretions of the food. One might feel inclined to draw the further inference, that a complete fistula situated in the upper part of the small intestines, renders the nutrition of the body through the stomach impossible; but this inference is made improbable, by the circumstance that the ingestion of food through the mouth alone, was sufficient to keep up and increase the strength of the patient, after the recovery had reached a certain stage under the plan above described.

Although our space is limited, we cannot refrain from giving some of the principal observations made by the author, referring for the details to the essay itself. 1. There are two distinct sensations of hunger; the one is caused by the nervous system in general, perceiving the want of fresh supply for the exhausted tissue; the other is produced by the nerves of the digestive organs alone. The former of these may continue even when the digestive organs are filled with food. 2. The peristalic motion of the intestines is not continuous, but shows periods of rest and of increased action; there is, however, no regularity in the alteration of rest and action. The force of the peristaltic motion could not be ascertained with precision, but it was sufficient to overcome a column of water two feet high. In the lower portion of the digestive tube, distinct antiperistaltic motion was frequently manifested. 3. The quantity of the intestinal juice (*succus entericus*) is very small and constantly alkaline. It will be recembered that this observation is in opposition to that of Frerichs, but in corroboration of Bidder and Schmidt's results. The percentage of solids varied between 3.87 and 7.4 per cent., which is considerably greater than that stated by the authors just mentioned. 4. There can be no doubt that the intestinal juice exercises a digestive influence on proteinaceous substances. This process, however, is accompanied by that of putrefaction. 5. The intestinal juice transforms starch into grape-sugar. 6. It does not transform cane-sugar into grape-sugar. 7. The cane-sugar, absorbed as such, does not re-appear in the urine. 8. Without the influence of bile or pancreatic juice, there is either no absorption of fat, or at all events it takes place only to a very limited amount. 9. The reaction of the mixture of digestive secretions, passing from the upper portion of the tube during the state of fasting, was almost always neutral, only in rare instances slightly acid or alkaline. 10. This mixture of fluids did never exhibit the reaction of saliva. We may therefore conclude that the saliva is absorbed before the mixture reaches the jejunum.

11. The average percentage contained in these fluids was 2.48. 12. The first portion of the food introduced into the stomach, appeared in the jejunum from between fifteen to thirty minutes after the commencement of eating. 13. Solutions of cane-sugar disappear to the greater part already in the beginning of the digestive canal; what reaches the jejunum is transformed into grape-sugar. 14. Raw albumen of eggs is likewise partly absorbed already in the stomach, and adjacent parts of the digestive tube; the portion which enters the jejunum is unchanged. 15. Gum passes unchanged into the small intestines. 16. Gelatine becomes dissolved, and does not coagulate again; the greater part of it is absorbed. 17. Part of the casein contained in the milk, reaches the jejunum in the uncoagulated state. 18. The mixture of the digestive fluids contained in the duodenum, emulsifies fatty substances completely, if its reaction is alkaline, but less completely if it be acid. 19. This mixture of fluids exercises likewise a digestive influence on proteinaceous substances. The lowest quantity of digestive fluids entering the jejunum within twenty-four hours, amounts to more than one-seventeenth of the weight of the body.—*Brit. and For. Med. Chirurg. Rev.*, Jan. 1859.—*Amer. Jour. of Med. Science*, April, 1859.

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On the Influence of Heat and Electricity on the Spinal Cord. By Dr. F. Kunde.—1. Two normal frogs being placed in water, the one at the temperature of 1° C. and the other at 31° C., the respiratory movements and the pulsations of the heart abate in frequency in the one kept cool, while they increase in the other.

2. The heart's pulsations of a frog kept at 1° C., average 20–24 per minute; at 31° C., they average 60–64.

3. A frog may remain for twenty-four hours at a temperature of 1° C. without dying; the muscular movements become at such a temperature slower and less intensive.

4. A frog that has spent some time at a temperature of 34° C. loses both voluntary and reflex movements. Blood and lymph hearts continue, however, to pulsate. Should, however, this temperature be kept up for a considerable time, the animal dies without the occurrence of cadaveric rigidity.

5. If a frog be for some time exposed to the influence of an interrupted current of electricity, so that tetanus has primarily occurred, after a time, when examined after cessation

of the current, it is found to have lost both voluntary and reflex motion, the blood-heart still beating forcibly. In a short time, such a frog recovers perfectly.

6. An elevated temperature and electricity remove the tetanus induced by strychnine.

7. A frog tetanized by a definite dose of strychnine, loses its tetanus at a definite temperature, and is completely restored in a short time by a proper regulation of the temperature.

8. A frog which has received a certain quantity of strychnine, becomes tetanic at 31° C., but not at 16° C. Even so a frog may become tetanic at 16° C., though it was not so at 1° C.

9. The administration of strychnine to frogs may be so regulated that they may remain unaffected at any definite temperature, and yet become immediately tetanic on a proper modification thereof.

10. A frog rendered tetanic at any temperature may be maintained in this state for fourteen days (and probably longer,) by being kept at a temperature of 1° C., or laid on ice.

11. A frog which has lain several days on ice, tetanic from strychnine, loses this condition in a very short time by the application of warmth (as by holding in the hand.)

12. A frog which has almost ceased to be tetanic at any definite temperature, (as at 15° C.,) has the tetanus immediately reproduced by being laid on ice, or placed in a temperature of 1° C.

13. If a frog, tetanized by strychnine, be exposed to a current of interrupted electricity sufficient to produce tetanus in a normal frog, it speedily loses the strychnine tetanus, both by the ascending and descending current.

14. A frog tetanized by strychnine, may, for eight hours, be exposed to the most powerful action of two Daniel's batteries; nevertheless the tetanus recurs a short time after cessation of the current, while mechanical and chemical irritants are unable to produce reflex motions, though electricity can, immediately after the removal of the wires, produce convulsive movements. (All these experiments must be made on newly caught frogs, and they must be reckoned not by the dozen, but by the hundred.)

15. If two cats, from the same litter, get the same dose of strychnine, and both become tetanic, and the one then placed in a temperature of from 16°–19° C., and the other at 40°–45° C., that cat condemned to the low temperature speedily dies, the other is rapidly restored to its *status normalis.*—

Some Experimental Researches on the Effects of Cold on the Human Body. By MM. THOLOZAN and BROWN-SEQUARD.

A series of researches permits the authors to draw up the following propositions:

1. If our extremities are exposed to the action of cold water, they can in a very short time lose a considerable portion of their temperature (according to our experiments, from ten to eighteen degrees.)

2. After an extremity has lost much of its temperature (from ten to eighteen degrees,) it does not regain it before the end of forty-five minutes or an hour, in an atmosphere varying from twelve to eighteen degrees.

3. Contrary to the opinion of Edwards, the lowering of the temperature of a small part of the human body has no sensible influence upon the general temperature.

4. The lowering of the temperature of one hand can produce considerable falling of the temperature of the other, without the general temperature of the body being sensibly diminished.

Dr. Brown-Séquard has ascertained that the latter phenomenon becomes more marked if the immersed hand is the seat of more intense pain, and if the temperature of the air in which the other hand is kept is less elevated; it is also proportional to the contraction of the vessels of the hand not immersed, and it is this contraction exclusively which produces the falling of the temperature. This phenomenon is an example of reflex action upon the blood-vessels, their muscles contracting under the influence of an irritation applied to the sensitive nerves of another extremity; it is further remarkable, that this reflex action takes place only between homologous parts, and that the immersion of the hand, for instance, into cold water, exercises no appreciable influence upon the temperature of the feet.—*Journal de la Physiologie.*

On the Variations of Color in the Venous Blood of Glandular Organs, according to their state of activity or repose. By Prof. CLAUDE BERNARD. (A Memoir read before the Academy of Sciences, January 25th, 1858.) Translated from the April No. of M. Brown Séquard's Journal de la Physiologie de l' Homme et des Animaux. By N. H. PAYNE, M.D., of Jackson County, Missouri.

Since the discovery of the circulation, two kinds of blood have been recognized: the one *red* or arterial, the other *black* or venous. This difference in color of venous or arterial blood has been considered as so characteristic, it has served, since Bichat, as the basis of an anatomical division of the organs of the circulation.

"I divide," says that anatomist, "the circulation into two parts; the one carries the blood to all parts of the system, the other returns it to the lung. The first is the circulation of red blood, the second that of black blood."*

The facts which I have the honor to communicate to the Academy will show that hereafter the two expressions, *venous blood* and *black blood* cannot be considered as synonymous. There is, in fact in its normal state, venous blood as perfectly red as arterial blood; also venous blood which is sometimes red and sometimes black.

Color of the Venous Blood of Glands.—But that which will particulary interest the Physiologist, will be to learn, as I will soon show, that these changes of color correspond with the different functional states of the organs. Some years ago (1845,) whilst making experiments on dogs, on the elimination of certain substances by the kidney, I was surprised to see the blood which ran from this organ by the vein as red as that which entered it by the artery. This reddish color of the renal vein was the more easily determined, as it contrasted strongly with the black color of the inferior cava into which it empties. Recently, in my course at the College of France, I made this observation a second time, in order to pursue it further. I found the same phenomena in the rabbit, the renal veins of which, like those of the dog, contained red blood, which was distinctly seen mingling itself with the black blood of the inf. cava. The lumbar veins which empty near the renal veins contain, in contrast, black blood, as also a small muscular vein entering the left renal vein.

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* Bichat, Anatomie Generale, II. p. 245.

However, in repeating the experiments on the dog and rabbit, and in varying the conditions of observation, I soon perceived that this usual reddish color of the renal vein might change its tint, and even become very black under the influence of different circumstances, so that this would appear contradictory if we limited ourselves to the announcement of a single result of the observation. Unfortunately, this can almost always be the case in physiology, when we do not sufficiently distinguish, in phenomena so complicated, the eminently variable conditions presented by every living organism.

After having ascertained the two possible appearances of the blood of the renal vein, it was necessary to find what relation they sustained to the functional state of the kidney. To determine this a small silver tube was placed in the ureter, through which the urine was seen flowing away, drop by drop, which, as is well known, takes place almost without intermission. It was then observed that the blood of the renal vein and also the tissue of the kidney were perfectly red whilst the urine flowed freely by the tube, but that this flow ceased under the influence of circumstances, which, in rendering black the blood of the renal vein, gave at the same time a bluish tinge to the organ. Hence it seems, we must associate the red color of the renal vein with an active state of the kidney, and its black color with its state of repose or the cessation of its functions. It was seen, moreover, that the re-action of the urine changed in no way the phenomena; the renal vein is equally red in the dog, whose urine is acid, and in the rabbit, whose urine is alkaline during digestion, and acid after twenty-four or thirty-six hours fasting.

It would be useless now to enumerate all the influences capable of interfering with the formation of urine, and of changing the color of the renal vein. I will only point out the disturbing causes which relate to the *procede* of the experiment, and will remark, that, to observe the reddish color of the renal vein, it is not simply necessary to make a large opening in the abdomen, and lay aside the intestines to expose the kidneys and their vein. So grave an operation almost always causes in the dog and rabbit, if not immediately, at least in a few moments, suppression of urine, and when the blood of the renal veins is seen to assume a deeper color, and often to become as black as the inferior cava.

The most suitable operation consists in making a small opening in the lumbar region as for nephrotomy. It is best to operate on the left side, as the left renal vein, on account

of its greater length, is more easily found. By the same wound the ureter may be separated for the introduction of a silver tube, by which we ascertain, during the experiment, whether the urinary apparatus is in a state of activity or repose.

From all we had said, it clearly follows, that the blood of the renal vein presenting usually a redish color connected with the formation of urine, which is nearly constant, is no longer included in the definition of venous blood given above.

The first question which presented itself to my mind, after the preceding observations, was, to know if this reddish color of the renal vein was an isolated fact, peculiar to the kidney, or if it might not be extended to the secretory organs, whose function it likewise is to separate in their tissue a peculiar organic liquid. To verify this idea I had recourse to the submaxillary gland of the dog, which is very convenient for the experiment. It constitutes in fact an isolated organ, and is superficial enough to be easily reached. I then sought the vein of this gland, and first ascertained that it presented numerous anatomical varieties,* which, however, could in no way modify the observation of physiological phenomena.

In my first experiment, made the 28th of last December, in my course at the College of France, I found the venous blood coming from the submaxillary gland as black as the deepest colored venous blood. This, however, was in no way contradictory to the red color observed in the renal vein, for the salivary secretion is an intermittent one, and the gland was not secreting when the presence of black blood in its vein was ascertained. It was then necessary to find if, in exciting this gland to action, any change of color might be observed in its venous blood. For this purpose a few drops of vinegar were placed in the mouth of the animal, which provoked, by reflex action, a secretion of saliva. Then were seen fully verified the expectations which had been entertained; for, in a few moments, the color of the blood in the glandular vein changed its tint, and from black soon became red, to re-assume in a little while its black color when the secretion ceased.

Finally, to remove all doubt with regard to the interpreta-

*Sometimes the vein of the gland is single, and emerges from the part of the organ to empty itself into the sub-maxillary vein; sometimes it has two origins or two branches of equal or unequal volume emptying into two distinct venous trunks.

tion of the phenomenon just observed, the excretory duct of the submaxillary gland was laid bare, and into it introduced a small silver tube; after which was exposed the branch of the lingual nerve distributed to the gland. We then had under our eye the vein of the submaxillary gland, its excretory duct, in which a silver tube was placed, and the excited nerve of secretion. We could then determine that when the organ was at rest no saliva flowed through the tube, and black blood circulated in the vein of the gland; whilst every time the nerve was excited by galvanism, and secretion provoked, the color of the venous blood became red, and again black when the excitation ceased and secretion stopped. The same experiment was repeated at different times with like results. An interval of several seconds was moreover observed between the excitation, the appearance of the secreted liquid and the red color of the blood. The latter took place more slowly as if the gland required a certain time to empty itself of its black blood, before the red could appear. For an analogous reason, doubtless, the red color of the vein lasted some moments after the secretion ceased—in other words, the change in color from black to red, and from red to black, was a gradual one. Lastly, it was found that the blood always flowed more abundantly when red, that is, during the functional state of the organ, than when black, the organ being at rest.

This experiment on the sub-maxillary gland has been repeated a great number of time on dogs, and always with similar results, with the exception of some difference which may be due to the more or less vigorous or enfeebled state of the animals.*

These experiments on the sub-maxillary gland then show that its venous blood is alternately black and red, and that these interchanges of color exactly correspond with the intermissions in the functions of the gland.

The two series of results previously cited and obtained, one on the kidney, and the other on the sub-maxillary gland, certainly do not contain isolated facts, and the same observation should, doubtless, be extended to other glands. Experi-

* The results are, as a general thing, more clear and rapid the more vigorous the animal, and the less the organs have been modified by previous excitation or by exposure to air. It sometimes happens that the vein dries and shrivels, which interrupts the circulation; it should then be cut at its exit from the gland in order to judge directly the color of the blood which runs from it.

ments, begun by me on the parotid and in the glands of the abdominal portion of the digestive tube, have furnished so far like, general results; the study will, nevertheless, only be complete when these experimental researches shall have been made in each particular gland.

On the whole, it is evident, from the facts contained in this work, that if it is proper to preserve in physiology the title of red blood or arterial blood, (which is, properly speaking, only the venous blood of a single organ, the lung,) that of black blood can not in a general manner be maintained for venous blood. We have proved, in fact, that venous blood may be red or black in the organs of secretion according as we consider these organs in their functional states or states of repose. This consideration of the activity and repose of the organ, which corresponds in some measure with its static and dynamic states, seems to me to constitute an important point to be introduced in the physiological and chemical studies of blood. In fact it is not only in color that the venous blood of an organ at rest differs from that of an organ in a state of activity, but it presents other important differential characters, which must be associated with a wide difference in chemical constitution. Thus the venous blood of the kidney in an active state, which is red, remains more fluid, and sometimes even yields no clot at all; whilst the blood of the same vein, the kidney being in a state of repose, is black, and presents a consistent coagulum, etc. Doubtless, physiologists and chemists were already aware that venous blood could not as arterial, be regarded as every where identical, and that the venous blood of each organ required to be separately analyzed; but that which has not been said, I think, and which, nevertheless, seems to me indispensable to be hereafter considered, if we wish chemical analyses to be conducive to ideas and useful as possible for physiology, is to examine separately and comparatively the composition and properties of the venous blood of the same organ, in its states of action and repose. We can already foresee, after what has been said, that often greater differences will be found in the two kinds of blood of the same organ in these states, that in corresponding blood of two different organs. This point of view is not only applicable to the glands, but should embrace all the organs of the blood—the venous blood of which should now be studied in their states of fruition and repose. We may in some measure characterize each tissue by the very different modifications impressed upon the blood passing through it by its own functional activity. Thus, if

the blood leaves a gland in the performance of its function very red, it escapes, on the contrary, very black, and with different physical properties from a contracting muscle. The mechanism of these different colorings of the blood will necessarily find its explanation in subsequent chemical analyses, only the physiological conditions of which we wished to point out at present.

With one more remark, we will close; it is that all these modifications occurring in blood in consequence of the functional activity of organs are always determined by the nervous system. It is consequently in this point of contact of the organic tissues and the blood that we must investigate the *rôle special* of the nervous system in the physico-chemical phenomena of life. The development of the facts relating to this point of general physiology will be the subject of a subsequent communication.

PATHOLOGY AND PRACTICAL MEDICINE.

A Case of Spontaneous Hydrophobia. By DR. HENRICH.—F. K., thirty years of age, suffered on the twenty-ninth of May, 1857, of cephalalgia, which radiated from the forehead to the occiput, and of all the symptoms of a cold in the head. On the morning of the thirtieth, he complained of chills, and distressing horripilations. Dr. Henrich examined the patient attentively, without finding in the throat or elsewhere, a single sign of disease. In the evening he was called in great haste to the patient, whom he found sitting in the bed, the face bathed in perspiration, pale and expressing terror; the eyes injected, brilliant, haggard; the voice hoarse, anxious, and broken. The patient complained of pain and constriction in the throat and chest, of intense thirst with impossibility to drink, and of dryness of the mouth. The respiratory movements were accelerated, superficial, and irregular; they became normal in the interval of the spasms, which followed each other rapidly; but when the throat became constricted, the patient seemed to suffocate, and carried the hand to the neck as if to remove an obstacle to respiration. The saliva flowed in great quantity from his mouth. Pulse ninety, and feeble. Pharynx a little reddened, and covered like the mouth with viscid mucus.

After earnest entreaties, Dr. Henrich finally succeeded in trying to overcome his violent horror of liquids; after hav-

ing for a long time struggled against a convulsive contraction of the muscles of the forearm, he could finally bring a glass of water to his mouth, but hardly had the first few drops touched his lips, when he was seized with a violent attack of suffocation. He threw his glass away with a gesture of despair, and taking refuge in the remotest part of the bed, cried out to take the water away; that he could not swallow; that he was suffocating.

In this condition he remained during the night. The impression of light or of a current of air exercised, however, no perceptible influence upon the spasms, and the vesicles of Morochetti were not found on the margin of the tongue. In spite of venesection, a blister on the chest, etc., all the symptoms were aggravated the next day; chloroform exasperated them; and they became less violent only for a few moments, after the patient had lost about a pound of blood through the wound made by the venesection, which had opened again; but soon they returned with greater intensity; tetanic convulsions and opisthotonos supervened, and the patient expired half an hour later. He had preserved the full power of his intellectual functions until tetanus came on.

On autopsy, a very slight swelling of the base of the tongue was discovered; the pharynx was in a healthy condition; some pulmonary hypostasis, and two hæmorrahgic suffusions in the mucous membrane of the stomach were found. All the other organs, the spinal marrow included, presented no alteration. The blood was black, liquid, and diffluent.

Dr. Henrich assured himself, by the most careful inquiries, that the patient had never been bitten by a dog, either mad or healthy, and that he did not believe himself at all attacked by hydrophobia. For three weeks previous, however, he was in low spirits, and without being otherwise sick, had a presentment of his approaching death, as he said. He indulged, however, in excessive coïtus, (he was married and kept two mistresses,) and was troubled with grief and sorrow. To these two causes combined, the appearance of the terrible malady may be attributable.—(*Henke's Zeitschrift für Staatsarzneikunde*, 1858, p. 361.)

This case, which belongs to the third class of spontaneous hydrophobia of M. Chomel, (*Dict. de Méd.*, tome xv., 1837,) is, among all the published cases, one of the most characteristic. Cases of similar kind have been reported by MM. Ely, Burggreave, (*Gaz. des Hôpitaux*, 1854,) Lessmann, (*Preuss. Vereinszeitung*, 1854,) Bulley, (*Assoc. Med. Jour.*, 1854, Nov. 11,) and Putegnat, (*Jour. de Méd. de Bruxelles*,

June, 1853.)—*Gazette Hebdomad.* 1858, 40,) from *The N. Amer. Medico-Chirurg. Review.*

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Influence of different Beverages upon the Elimination of Common Salt, Urea and Sugar in Diabetic Urine.—Dr. Rosenstein has investigated the manner in which various articles of drink influence the elimination of salt, urea and sugar in diabetes, because he believed the most important means of treatment to be found in the diabetic regimen of the patient; he has arrived at the following results:

1st. After the use of coffee, the elimination of salt and sugar was, on the average, increased, that of urea diminished.

2d. By the use of Bavarian beer, the elimination of salt was considerably increased that of urea somewhat diminished, and that of sugar also increased, though in a less degree than by the use of coffee.

3d. After the use of wine, of which Dr. R. administered three different varieties, chosen in reference to the amount of alcohol they contained (Bordeaux having the largest, Madeira a less, and pure St. Emillon the least per cent. of alcohol), the elimination of salt was scarcely increased. Still the latter was smaller, in proportion as the amount of alcohol in the wine was greater.

4th. By the use of tartaric acid, the elimination of salt was but little increased, that of urea decreased, and of sugar increased.

5th. During a moderate febrile condition, the elimination of salt was normal, that of urea (although increased in proportion to the whole amount of urine), was diminished, and that of sugar much diminished.

In reference to temperature, the observations show; 1st, that the animal heat of the (diabetic) patient is less than that of health; 2d, that there is no observable relation between temperature, and frequency of pulse and respiration; and, 3d, that during a general febrile condition, the temperature is higher than that of a healthy person.—*Virchow's Archiv. of Pathological Anatomy, Physiology, and Clinical Medicine,* xiii. 4, 5.

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Use of the Essential Oil of Turpentine and Opium in Large Doses in the Treatment of Severe Puerperal Affections.—In the

discussion going on for the last three months at the Academy of Medicine in Paris, and which has attracted so much public attention, puerperal fever has been considered by the most competent authorities as a disease almost universally beyond the resource of art, at least in the present state of our knowledge; all the means hitherto employed have, almost without exception, proved useless. This melancholy confession of the inefficacy of medicine to subdue an affection which carries off so many women in the flower of their age, is unfortunately but too well founded when we speak of the severe epidemic form, but ought not, however, to be adopted as literally true. We have lately seen a case of very severe puerperal peritonitis, which M. Antoine has cured by the method above mentioned, and which Velpeau introduced many years ago. We have since seen two cases in Velpeau's wards, both cured in the same way. This plan of treatment is by no means new, for we remember to have used it with success some twenty years ago, but it is not the less worthy of notice. It is the plan of Graves (of Dublin) which Trousseau has long employed with advantage in the treatment of puerperal illnesses; it consists in giving to lying-in women attacked with metro-ovaritis, or phlegmonous inflammation of the broad ligaments, or peritonitis, or uterine phlebitis, &c., &c., opium and essential oil of turpentine in large doses.

Dr. Bonfils has just published, *in extenso*, in the "Bulletin Therapeutique," two very interesting cases of this kind. In the first the patient was attacked after her confinement with peritonitis and double pleuro-pneumonia, and was cured of this formidable complication after seven week's treatment. The other patient was attacked under similar circumstances, with a very severe general peritonitis, all the puerperal complications were rapidly checked by the plan of treatment recommended, but after the most marked improvement, which promised to end in a perfect recovery, she was seized with symtoms of hectic, which closely resembled those of pulmonary phthisis, and she finally sunk, owing, in all probability, to a purulent infection.

Opium and turpentine were administered in both cases in the following manner:

In the first case Trousseau prescribed opium in pills, and turpentine in enemata; he gave at first 5 centigrammes of opium, in five pills, in the day; then the dose was raised to 8 centigrammes, in eight pills; then 10 centigrammes, in ten pills. The opium was continued for thirteen days.

Turpentine was administered at first in doses of 10 grammes,* divided into two enemata; one was given morning and evening; then it was gradually increased to 20 grammes, 25 grammes, 30 grammes; this last dose was continued for fifteen days. The following was the formula adopted; essential oil of turpentine, 10, 20, 25, 30 grammes; yolk of an egg; water, 100 grammes; to be divided into two enemata; add to each enema five or six spoonfuls of gum water or linseed. The enema to be retained as long as possible.

In the second case the opium was likewise given in pills, in the dose of five centigrammes continued for three days. The essential oil of turpentine was administered by the mouth, in capsules, for six days; the patient took every day six capsules, each containing 1 gramme of the essential oil; she took two, morning, noon, and night.

M. Bonfils details the following as the physiological phenomena which were noted as occurring in both cases:

In the second case, immediately after taking the capsules, the patient felt a sensation of intense heat at the pit of the stomach; a few minutes afterwards there was a very complete general reaction, characterised by heat of surface, general perspirations, increase in the volume and frequency of the pulse; then followed in succession, confusion of vision, vertigo, stupefaction, and drowsiness, and after some time, itchiness of the skin.

The physiological phenomena were less pronounced when the turpentine was administered in enemata; they consisted in an immediate sensation of heat in the abdomen, a general but moderate reaction, slight vertigo, some confusion of ideas, slight disturbance of vision, and slight itchness of the skin. Such were the phenomena which existed in the first case.—*Dublin Hospital Gaz.*, and *Braithwaite's Retrospect.*

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* *Gramme* = 15 4325 grains Troy. *Centigramme* (100th of a gramme = 0.15432 grain.

EDITORIAL AND MISCELLANEOUS.

OBITUARY OF DR. SILAS AMES.

At a meeting of the city Physicians, Montgomery, Alabama, Dr. William A. Baldwin was called to preside. The meeting being organised, resolutions were passed to publish in some of the Medical Journals an obituary of the late Dr. SILAS AMES. The Chair appointed Drs. R. S. Wilson, W. P. Taylor and G. W. Pollard that committee, &c.

It therefore becomes a duty to publish in the Medical records of the day the sad intelligence that we have lost a much esteemed fellow-laborer by the death of the lamented Dr. Silas Ames, one of Montgomery's oldest and ablest physicians, a man of extreme modesty, rare genius, and great moral worth. He was born in the city of Richmond, Virginia, about the year 1802, and in early life was bereaved of his father, by whose death almost every hope of succour fled, save a mother's care, which endeavored to supply the loss of a father's aid and protection. While long and weary years rolled on, she continued her task, cheering her son with the thought that persevering industry generally surmounts the greatest difficulties. Full of hope and blessed with intellect, he prosecuted his studies and, in his fifteenth year, had prepared himself to enter, as an assistant, a drug establishment in his native city. Here he diligently applied himself to business; his inquiring mind soon obtaining considerable knowledge of chemistry and pharmacy, as they then existsd. Remaining in this situation several years, he decided to study medicine, and applied, for direction, to Dr. Daniel Drake, then a medical practitioner in the city of Richmond, who cheerfully offered

to become his preceptor. The favor was gladly received, and his kindness ever after remembered with gratitude.

Remaining in the drug store, he worked all day, reading at night as time and circumstances permited, visiting his preceptor when opportunity offered, and thus continuing to labor, prepared himself to enter upon a collegiate course.

He attended his first course of medical lectures in Transylvania University, Lexington, Ky., and afterwards, in the same State, entered the ranks as a medical practitioner. Remaining in the State not more than ten or twelve months, he removed to Alabama in the year 1827, and began his professional career in Montgomery, then a small village. Commencing with her early settlers, his name soon became identified with the medical history of the place, where he continued to reside for more than thirty-one years as a medical practitioner. As such he had few equals and no superior. If success in practice is a test of medical skill, then he stood pre-eminent. He was never numbered with those who, although scientific men, are remarkably unsuccessful in medical practice. On the contrary, there are those now living, who think they have seen Ames stay life's ebbing tide, until disease had passed its culminating point, and the sufferer was restored to strength and health, to the joy of both patients and friends. In therapeutics he had no hobby, no particular theory to support: but generally selected his remedies with reference to the organ and tissue affected, and never long continued active mediciation. He possessed the faculty of closely observing the action of remedies, noticing early, and watching carefully the recuperative action of the system, and endeavoring to husband it until convalescence was fully established. Blood-letting, calomel and tartar emetic were not favorite remedies with him. Notwithstanding, when particularly indicated, he would use them cautiously, employing them, rarely, and only in dangerous diseases; believing that they lowered too rapidly the vital force, and sometimes permanently exhausted it. Polypharmacy and hypermedication he avoided; repeatedly saying, the simpler and fewer the remedies that will cure so much the better. In

his post-mortem examinations he was remarkably careful, employing chemistry and the microscope, as well as the scalpel, in elucidating pathological science. His great aim in all his medical efforts was usefulness, not display. Indeed he was eminently a practical man, full of tact, which was strongly exhibited when examining a patient. This he did slowly, carefully and very minutely, saying but little, except questioning the patient or nurse. After prescribing, if time permitted, he would converse cheerfully, hopefully and very pleasantly; during the conversation having often a hope-inspiring smile on his countenance, which gave great encouragement to the patient and solace to the friends.

In his intercourse with medical men he was not very communicative, but remarkably courteous, honorable, conciliatory and high minded. To his opponents in medical literature, he was very gentle, never irritable, never sarcastic, patiently continuing to investigate the point at issue. He would even reply mildly to harsh expressions; but frequently said that such language shed no light on the cause of science, being contrary to the true spirit of medical investigation. As an author, he was not voluminous, writing comparatively little. Indeed he was too busily engaged in an extensive practice to devote much time to the pen. What he wrote was generally on practical medicine, and his writings are recorded in the medical archives of the times, where they may be read and judged of without the aid of a commentator.

As a man, Dr. Ames was gentle, firm, unassuming, benevolent, industrious, ardent and honest; a lover of good men and enjoyed a large share of the esteem and affection of the community generally. But whilst enjoying the respect of surrounding friends, and in usual health, disease came suddenly upon him, and after a brief illness of eight days he died of typhus pneumonia, December 5, 1858. Thus died one, who, left an orphan in childhood, and in early life destitute of means, yet honorably worked his way to influence and distinction. The imperfectly executed task is finished. The unvarnished tale is told. His ashes rest in peace. His cher-

ished virtues live in the recollection of surviving friends, who still venerate the memory of Dr. Silas Ames.

The execution of this notice has been delayed in consequence of inability to communicate speedily with relatives at a distance, on whom we relied for items of information. We trust that some abler hand, with more ample material will, at some future day, write a biographical sketch worthy of the dead.

AMERICAN MEDICAL ASSOCIATION.

The twelfth annual meeting of the American Medical Association will be held in Louisville, Ky., on the 3d of May, 1859. We think from all appearances that there is every promise of a large and interesting convocation, and sincerely trust that its next volume of transactions may be worthier of its name and the country than those which have been published during the past few years.

According to a resolution passed at the last meeting in Washington, delegates from the various Medical Colleges throughout the country are earnestly solicited and expected to assemble in Louisville on the first Tuesday in May, to take into consideration the present and prospective status of medical education among us, and recommend, if possible, to the consideration of the schools, such measures as they may think feasible for elevating the standard of professional acquirement, at least so far as their future graduates are concerned.

In the last report made to the Association the following recommendations were offered: that the schools should be encouraged, and those who have no leisure or facilities for instructing pupils should not receive them into their offices; every school should have at least seven professors; all the schools in the country should commence their sessions in October, have but one term of six months annually, and four

lectures only should be delivered daily; candidates for graduation should have received a liberal education and attend some hospital regularly.

The report has omitted some of the most essential conditions of a thorough reform, among which we may mention, in passing the necessity of having a board of competent examiners for graduates, unconnected with and independent of the College offering the diploma. We do not wish to offer any evil augury, or make ill-omened prophecies, every one must certainly be convinced of the wisdom, if not necessity of some change in the present management of the colleges, but it requires no great wisdom or sagacity to foresee, that, from a multiplicity of causes, no agreement among the representatives of the several schools is probable, and that the whole matter will be indefinitely postponed if not finally dropped. We doubt, in fact, if a respectable convention of such delegates will so much as assemble. However this may be we urge all who are able, to attend the next meeting, and sustain its character and objects by the influence of whatever ability or position they may possess.

PAST DURATION OF THE HUMAN RACE.

Prof. Owen in his Inaugural Address before the British Association, offers the following remarks upon the distribution and past duration of man upon the earth:

Man alone, with one of the beasts which he has domesticated—the dog—is cosmopolitan. The human species is represented by a few well marked varieties; and there is a certain amount of correspondence between their localities and general zoological provinces; thus the Australian variety of man is as well marked and circumscribed as the Australian fauna generally; the Papuans of New Guinea present the same difference from, with degree of affinity to, the Australians, as we find in comparing the respective faunæ of

Papua and Australia. But with regard to the alleged conformity between the geographical distribution of man and animals, which has of late been systematically enunciated, and made the basis of deductions as to the origin and distinction of the human varieties, I would submit the following remarks as affecting the system referred to. Using Blumenbach's term in the sense of the latter terms "Indo-European" and "Arian," we find the "Caucasian" race extended from Iceland to the mouth of the Ganges. There is no corresponding distinction in the animals and plants of the Europæo-Asiatic continent, which is bisected by the oblique line dividing the Mongolian from the Caucasian varieties of mankind. The Persian fauna extends into Tartary; the Himalayan into Thibet. As two primary varieties of mankind exist in one great zoological province in the old world, so a third great variety extends over at least too zoological provinces in the new world. All authors divide the north American or "Nearctic" from the south American or "Neotropic" region, whatever class of organic life they may treat of geographically; but the red or copper-colored American is the same, physically and linguistically, to the extent of the characteristics of a primary race, from the 60th degree of north latitude to the 53d degree of south latitude. The Lapps of Arctic Europe differ linguistically and physically, as a race, from the Norwegians and Swedes; the zoological province is essentially one. As such it extends over the same parallels of latitude in America, where the Mongolian Esquimaux and the American Chippawas inhabit. The Hottentots and Kaffirs are more distinct, linguistically and physically, than the former are from equatorial negroes, or the latter from the Nubians; yet they both inhabit one well-marked zoological province, south Africa. Two varieties of mankind—the Papuan and Malayan—inhabit Borneo and other islands at the eastern part of the Indian Archipelago—these islands forming one and the same zoological and botanical province. Not less than twenty colors have been found requisite to indicate in a map of the British islands the different varieties and sub-varieties of the human race that have

contributed to its miscellaneous population. Other facts of the same kind might be cited, affecting the conformity of the distribution of man with that of the lower animals and plants, as absolutely enunciated in some recent works. Nor can we be surprised to find that the migratory instincts of the human species, with the peculiar endowment of adaptiveness to all climates, should have produced modifications in geographical distribution to which the lower forms of living nature have not been subject. It is only since man began to exercise his privilege and power that the geographical laws in regard to the lower animals of existing species have begun to be blotted out. Ethnology is a wide and fertile subject, and I should be led far beyond the limits of an inaugural discourse were I to indulge in an historical sketch of its progress. But I may advert to the uniform testimony of different witnesses—to the concurrence of distinct species of evidence—as to the much higher antiquity of the human race than has been assigned it in historical and geneaological records. Mr. Leonard Horner sagaciously discerned the value of the phenomena of the annual sedimentary deposits of the Nile in Egypt as a test of the lapse of time during which that most recent and still operating geological dynamic had been in progress. In two memoirs communicated to the Royal society in 1855 and 1848, the results of ninety-five vertical borings through the alluvium thus formed are recorded. In the excavations near the colossus of Rameses II at Memphis, there were 9 ft. 4 inches of Nile sediment, between 8 inches below the present surface of the ground and the lowest part of the platform on which the statue had stood. Supposing the platform to have been laid in the middle of the reign of that king, viz: 1361, B. C.—such date added to A. D. 1854 gives 3215 years, during which time the above sediment was accumulated, or a mean rate of increase of three and a half inches in a century. Below the platform there were 32 feet of the total depth penetrated; but the lowest two feet consisted of sand, below which it is possible there may be no true Nile sediment in this locality, thus leaving 30 feet of the latter. If that amount has been deposited at the same rate

of 3½ inches in a century, it gives for the lowest part deposited an age of 10,285 years before the middle of the reign of Rameses II, and 13,500 years before A. D. 1854. The Nile sediment at the lowest depth reached is very similar in comparison to that of the present day. In the lowest part of the boring of the sediment at the colossal statue in Memphis, at a depth of 39 feet from the surface of the ground, the instrument is reported to have brought up a piece of pottery. This, therefore, Mr. Horner infers to be a record of the existence of man 13,371 years before A. D. 1854—"of man moreover, in a state of civilization, so far, at least, as to be able to fashion clay into vessels, and to know how to harden them by the action of a strong heat." Professor Max Muller has opened out a similar vista into the remote past of the history of the human race by the perception and application of analogies in the formation of modern and ancient, of living and dead languages.

BURIAL PITS OF MARSTON MOOR.

In these days of sanitary works, it is well to remember that the *debris* of animal, and especially human, bodies may be subterraneously interred and hermetically sealed from the air for centuries, and then finally let loose upon the world, "as from Pandora's box," by necessary agricultural and sanitary excavation. A curious confirmation of this is afforded by a narrative lately published, of excavations made with the view of driving a large leading drain, at considerable depth, right through Marston-moor. It was on this field that the fierce tide of war rolled, and that a decisive battle was fought between Cromwell and his Roundheads, and Rupert with his Cavaliers. Beneath lie the burial pits of the slain. Instructions were given to avoid not only the tumuli, as indicated in the old maps of the district, but also the legendary localities of sepulture. However, long before the completion

of the work, the navvies came upon the Golgotha of the battle-field. There was nothing for it but to go straight ahead; to make a *detour* was impossible. At one place, there were found about twenty or twenty-five bodies, laid one over another, in all directions and postures; the forms of many were left in the clay. At this place, there was much of a sort of deposit that looked like soot,—not slime, but damp; the smell at first was intollerable, and could be perceived at some distance; it was so bad that the men could only work short spells. The skulls had preserved their shape, but crumbled when exposed to air. One poor fellow's passport to eternity was picked up; there was a bullet in his skull, which drooped out wheu the skull fell to pieces. The bones, especially the large ones, did not crumble away, but were very brittle when touched with a spade. The teeth retained their hardness and normal characters; but the carious state of those in many gave unmistakeable evidence that toothache was in the ascendant, and dental surgery at a discount, in those days.

THE PASSING WIND OF BULLETS.

A Russian *savant*, M. Pelikan, has made some sufficiently curious observations upon the supposed contusions produced by the "wind" of passing balls. The existence of this kind of lesion has been admitted by some surgeons, and denied by many others. M. Pelikan addressed himself to the Committee on Artillery of St. Petersburg, and having obtained some pieces of large calibre, had a machine constructed for measuring the force exerted by the wind of balls passing at various distances. The results obtained were constantly the same. At a distance of three inches, a passing ball produced not the slightest effect. The conclusions deduced by M. Pelikan are: 1. A projectile passing very close to any object exercises only an insignificant influence upon it. 2.

That what is called the wind of the ball, even with a full charge of powder, has so trifling a force as to be incapable of determining any lesion.

DEATH OF THOMAS D. MUTTER, M. D.

Dr. Thomas D. Mutter, Emeritus Professor of Surgery in Jefferson Medical College of Philadelphia, died in this city on the 16th day of March, in the sixtieth year of his age.

Thus, after a long-sustained struggle with disease, and much suffering, has closed the earthly career of one who early attained, and to the last preserved, the reputation of being one of the most brilliant, as he certainly was, the most popular, lecturer on surgery in our country. This announcement will be received with sincere regret, we are convinced, by his numerous pupils who were wont to listen to him with admiration and reverence, in their student days, and with no less profound sorrow by his surviving colleagues, who cannot have failed to perceive how much the prosperity of their institution has been indebted to his enthusiastic and ardent labors in its behalf.

His name will long be honorably connected with this magnificent donation to the Philadelphia College of Physicians; a gift to the securing of whose permanent value and increased munificence, his last thoughts and energies were almost wholly directed.

RETURN OF DEATHS IN THE CITY OF CHARLESTON

FOR FEBRUARY, 1859.

WHITES.		BLACKS AND COLORED.	
Apoplexy,	1	Apoplexy,	1
Birth, Premature	1	Brain, Softening, of	1
Bowels, Congestion of	1	Consumption,	5
Bronchitis,	2	Convulsions,	4
Consumption,	6	Croup,	1
Convulsions,	1	Dropsy,	2
Constipation,	1	Dysentery,	1
Colic,	1	Epistaxis,	1
Cystitis,	1	Fever, Typhus,	1
Drowned,	1	Fever, Typhoid	1
Dropsy,	3	Gangrene of foot,	1
Enteritis,	1	Gastritis,	1
Erysipelas,	1	Heart, Hypertrophy of	1
Fever, Typhus	1	Hemorrhage,	1
Fever, Typhoid	1	Lungs, Congestion of	1
Gangrene,	1	Marasmus,	1
Intemperance,	2	Old Age,	3
Lungs, Congestion of	2	Paralysis,	1
Old Age,	1	Pleurisy,	1
Pneumonia,	1	Pneumonia,	8
Sore Throat, Membranous	1	Stomatitis,	1
Trismus Nascentium,	2	Suffocation,	1
Want of Vitality,	1	Tetanus,	1
		Trismus Nascentium,	1
Males, 24; Females, 10; Total,	34	Want of Vitality,	3
		Males, 23; Females, 21; Total,	44
Ages.—Under 1 year,	8		
1 to 5,	4	Ages.—Under 1 year,	9
5 to 10,	1	1 to 5,	2
10 to 20,	2	5 to 10,	4
20 to 30,	3	10 to 20,	4
30 to 40,	7	20 to 30,	4
40 to 50,	2	30 to 40,	5
50 to 60,	2	40 to 50,	6
60 to 70,	3	60 to 70,	6
70 to 80,	2	70 to 80,	2
		80 to 90,	1
Total,	34	90 to100,	1
Natives of Charleston,	22	Total,	44
Natives of United States,	5		
Foreigners,	7	Natives of Charleston,	28
		Natives of the United States,	14
Total,	34	Foreigners,	2
		Total,	44

J. L. DAWSON, M.D., *City Registrar.*

RETURN OF DEATHS IN THE CITY OF CHARLESTON
FOR MARCH, 1859.

WHITES.		BLACKS AND COLORED.	
Apoplexy,	1	Accident,	1
Cancer Uteri,	1	Brain, Disease of	2
Convulsions,	1	Burn,	1
Consumption,	3	Convulsions,	3
Croup Membranous,	1	Consumption,	7
Constipation,	1	Dropsy,	2
Executed,	1	Enteritis,	1
Erysipelas,	1	Fever, Typhoid	1
Fever, Typhoid,	1	Fever, Scarlet	1
Fever, Scarlet	3	Heart, Disease of	2
Gastro Enteritis,	1	Hæmoptisis,	1
Gout,	1	Hydrothorax,	1
Insanity,	1	Intussusception,	1
Intemperance,	2	Lungs, Congestion of	1
Laryngitis,	1	Marasmus,	2
Lungs, Congestion of	2	Old Age,	1
Old Age,	1	Peritonitis,	1
Paralysis,	1	Phrenitis,	1
Pneumonia,	4	Pneumonia,	3
Trismus Nascentium,	1	Scrofula,	1
Unknown,	2	Teething,	1
		Tetanus,	2
Males 16; Females 15. Total,	31	Want of Vitality,	2
		Males, 20; Females, 19. Total,	39

Ages.—Under 1 year,	7	Ages.—Under 1 year,	8
1 to 5,	4	1 to 5,	7
5 to 10,	2	5 to 10,	2
20 to 30,	4	10 to 20,	4
30 to 40,	2	20 to 30,	3
40 to 50,	7	30 to 40,	5
50 to 60,	2	40 to 50,	5
60 to 70,	1	50 to 60,	0
80 to 90,	1	60 to 70,	2
90 to 100,	1	70 to 80,	0
		80 to 90,	1
	31	90 to100,	1
		Total,	39
Natives of Charleston,	17		
Natives of United States,	7		
Foreigners,	7	Natives of Charleston,	26
		Natives of the United States,	11
Total,	31	Foreigners,	2
		Total,	39

J. L. DAWSON, M.D., *City Registrar.*

METEOROLOGICAL ABSTRACT FOR FEBRUARY, 1859.

BAROMETER.

Maximum, 30.520, - - Minimum, 29.608, - - - Range, 0.912

THERMOMETER ATTACHED.

Mean.

7 A. M., 53.92, - - - 2 P. M., 60.92, - - - 9 P. M., 58.28

THERMOMETER DETACHED.

Mean.

7 A. M., 50.07 - - - 2 P. M., 61.64, - - - 9 P M., 55.35

Highest Degree.

7 A. M., 62, - - - 2 P. M., 77. - - - - 9 P. M., 64

Lowest Degree.

7 A. M., 37, - - - 2 P. M., 52, - - - - 9 P. M., 46

REGISTER THERMOMETER.

Mean.

Night, 48.78, - - - - - - - - - - - Day, 63.12

DEW POINT.

Mean, - - - - 47.64

WINDS.

Sun Rise—N.E. 7 days; N.W. 5; S.E. 4; S.W. 8; E. 3; W1. Prevailing S.W.
4 *P. M.*—N.E. 6 days; N.W. 3; N.E. 3; S.W. 11; E. 4; W. 1. Prevailing S.W.

WEATHER.

Fair, 19 days—Cloudy, 2 days—Rain, 7 days

QUANTITY OF RAIN.

Inches, - - - 3 34.

JOHN L. DAWSON, M.D.

METEOROLOGICAL ABSTRACT FOR MARCH, 1859.

BAROMETER.

Maximum, 30.460, - Minimum, 29,634, - - - Range, 0.726

THERMOMETER ATTACHED.

Mean.

7 A. M., 61.66, - - 2 P. M., 65.77, - - - 9 P. M., 63.22

THERMOMETER DETACHED.

Mean.

7 A. M., 58.00, - - 2 P. M.,66.25 , - - - 9 P. M,, 60·67

Highest Degree.

7 A. M., 69, - - - 2 P. M., 81, - - - - 9 P. M., 71

Lowest Degree.

7 A. M., 44, - - - - 2 P. M., 51, - - - 9 P. M., 52

REGISTER THERMOMETER.

Mean.

Night, 55.22, - - - - - - - - - - - Day, 68.12

DEW POINT.

Mean, - - - 54.38.

WINDS.

Sun Rise—N.E. 7 days; N.W. 4; S.E. 9; S.W. 7; E. 2; W. 2. Prevailing S.E.
4 *P.M.*—N.E. 2 day; N.W. 5; S.E. 10, S.W. 7; E. 3, . W. 4. Prevailing S.E.

WEATHER.

Fair, 16 days—Cloudy, 3 days—Rain 12 days—Prevailing—Fair.

QUANTITY OF RAIN.

Inches, - - - - 4.56.

JOHN L. DAWSON, M.D., *City Registrar.*

ALABAMA.

Dr. H. D. Capers, Auburn, - - - in full for 1858
" J. R. Ruskin, Tallasse, - - - - " " '59
" J. T. Sankey, Pintlala, - - - " " '60

ARKANSAS.

Dr. J. B. Payne, Magnolia, - - - - $3 for '59

FLORIDA.

Dr. T. P. Gary, Brooksville, - - - in full for '59
" C. A. Gee, Quincy, - - - " " '58
" H. M. Holmes, Silver Springs, - - " "
" M. Nicholson, Quincy, - - - - " " '59
" J. M. Valentine, Newnansville, - - - " "
" J. P. Wall, Brooksville, - - - " " '58

GEORGIA.

Dr. J. S. Coleman, Henderson, - - - in full for '59
" W. L. Felder, Augusta, - - - " "
" T. Galloway, Monroe, - - - - " "
" J. LeHardy, Savannah, - - - " "
" S. W. Leland, Cartersville, - - - " "
" John LeSneur, Knoxville, - - " "
" W. P. Mathews, Prattsburg, - - " "

LOUISIANA.

Dr. D. W. Brickell, New Orleans, - - in full for '58
" J. S. Scott, Pine Flats, - - - " " '60

MISSISSIPPI.

Dr. J. M. Burgess, McLean's, - - - " " '59
" Fant and Yates, Macon, - - - " "
" E. Foster, Louisville, - - - " "
" G. J. D. Funchess, Westville, - - " "
" F. R. Shuford, Holly Springs, - - - " "
" J. T. Suggs, Harrisburg, - - - " "
" E. L. Taggart, Byhalia, - - - - " "

MICHIGAN.

University of Michigan, Ann Harbour, - in full for '59

MISSOURI.

Dr. J. A. Boyd, Bloomfield, - - - - in full for '59

NORTH CAROLINA.

Dr. W. W. Ward, Plymouth, - - - in full for '59

OREGON TERRITORY.

Dr. A. M. Loryea, Portland, - - - in full for '59

PENNSYLVANIA.

Dr. G. B. Wood, Philadelphia, - - - in full for '59

SOUTH CAROLINA.

Dr. S. E. Babcock, Chester, - - - - $2 for '60
" L. Bellinger, Dutch Branch, - - in full for '59
" J. R. Bratton, Yorkville, - - - " "
" M. E. Carriere, Charleston, - - - " "
" D. E. Connor, Reevesville, - - - " "
" T. Y. Diseker, Hopkins, - - - - " "

Dr. J. Y. Dupre, Lenud's Ferry,		in full for	'59
" J. D. Dudley, Marion,		" "	
" T. J. Dubose, Flowerton,		" "	
" F. Fraser, Walterboro,		" "	
" E. T. Edgerton, Bamberg,		" "	'58
" E. J. Frederick, Speedwell,		" "	'59
" P. G. Gourdin, Pineville,		" "	
" T. H. Gregorie, Grahamville,		" "	
" J. W. Grierson, Charleston,		" "	
" R. B. Hanahan, Edisto,		" "	
" J. Hill, Cross Keys,		" "	
" J. P. Jervey, Charleston,		" "	
" L. B. Johnson, Walhalla,		" "	
" A. W. Kennedy, Columbia,		" "	
" J. Kirk, Bluffton,		" "	
" G. B. Lartigue, Blackville,		" "	
" B. L. Lucas, Tillers Ferry,		" "	
" W. T. Mayo, Pierceville,		" "	
" J. Moultrie, Charleston,		" "	
" G. W. Morall, Grahamville,		" "	
" G. J. Odom, Blackville,		" "	
" T. Ogier, Charleston,		" "	'58
" P. P. Palmer, Pineville,		" "	'59
" G. S. Pelzer, Charleston,		" "	'58
" T. Prioleau, Jr., Charleston,		" "	'59
" T. F. McDow, Liberty Hill,		" "	
Capt. J. D. McIlwain, Dry Creek,		" "	
Dr. S. W. B. McClarkin, Halseville,		" "	
" H. Ravenel, Black Oak,		" "	
" W. C. Ravenel, Charleston,		" "	'58
" W. A. Ramsay, Manchester,		" "	'59
" T. T. Robertson, Winnsboro',		" "	
" W. Rook, Martin's Depot,		" "	
" L. B. Sams, Beaufort,		" "	
" L. S. Smith, Leesville,		" "	
" T. Smith, Society Hill,		" "	
" H. M. Stuart, Beaufort,		" "	
" W. Snowden, Charleston,	$2	" "	'60
" S. Taylor, Wateree,		" "	'59
" E. S. Tennent, Charleston,		" "	
" J. W. Wyman, Whippy Swamp,		" "	
" C. Witsell, Ashepoo,		" "	
" C. Williams, Georgetown,		" "	

TENNESSEE.

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" T. C. Miller, Kimsey's Store,		" "	

VIRGINIA.

Dr. C. M. Hubbard, Burnt Ordinary,		in full for	'59

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CHARLESTON

MEDICAL JOURNAL AND REVIEW.

VOL. XIV.] CHARLESTON, S. C., JULY, 1859. [No. 4

ART. I.—*An Inaugural Dissertation upon the Avulsion of the Arm and Scapula.* Presented to the Faculty of the Medical College of the State of South Carolina. By H. B. HORLBECK, of Charleston, S. C.*

This case, which occurred in the practice of my father, Dr. Elias Horlbeck, of Charleston, S. C., and has no parallel, so far as I am aware, in the annals of American Surgery, is besides notable for presenting all the peculiarities which attend injuries of this kind, in a very marked degree. These circumstances determined me to present a *resumè* of the present state of our knowledge in relation to the injury under consideration, and must serve as my apology for recalling the incidents of an event which have impressed themselves very vividly on my memory.

I have not been fortunate enough to meet with any monograph on this subject. The only one bearing any relation to it is a memoir on "*plaies par arrachement,*" contained in the second volume of the *Memoires de Chirurgie.* This, with some notices in the regular works on Surgery, the record of the case of Samuel Wood, Phil. Trans. No. 449, and two other cases, collected in Eve's Surgical Cases, are all the matter on the subject to which I have had access.

* To this Thesis the annual prize, offered by the Faculty of the College, was awarded.—(ED.)

It shall be my endeavor, in the succeeding pages, to collect in one view the phenomena attendant on such lesions, which will bring to mind many curious and interesting provisions of nature for the restoration of injured members. According to the expressive definition laid down in the text books, "wounds are recent solutions of continuity, produced by incision, rupture, or fracture of a part." They all have some common characteristics and some peculiarities, occasioned by their causes, the manner in which the wound has been effected, and the nature and extent of the organs implicated. Those wounds, which are the subject of the present observations, belong to that class occasioned by the forcible tearing away of one part of the body from another.

They are characterized by many notable peculiarities. The individual, at first, is usually not even aware of the loss of the limb; the effect on the system seems entirely disproportioned to injury sustained; the patient appears hardly to experience any pain, nor does the severity or suddenness of the accident usually disturb the ordinary train of the associations going on in the mind.

In the case of the boy reported by Benomont in the Memoirs of the Academy of Surgery, Vol. ii., who had his leg torn off at the knee joint by a carriage in rapid motion, the patient was more occupied by the fear of the punishment he expected to receive at home for disobedience than by the pain of his wound, or even the loss of so valuable a member as his leg. The almost complete freedom from pain, and the acknowledged fact that it is seldom or never experienced in the progress of the case is, then, a distinguishing peculiarity of the injury. This is illustrated by all the recorded instances, when quite the reverse would have been supposed likely to occur, from the extent of surface exposed, the importance of the portion drawn off, and the nature of the parts torn across,—such as large nerves, blood-vessels, muscles, tendons, etc. However large the artery which supplies the limb, there is entire absence or sudden cessation of arterial hæmorrhage. To sustain this it will only be necessary to refer to the case of Cæsar, who received a

large, gaping wound, caused by the loss of nearly one-fifth of his body, and tearing through one of the largest arteries of the system, which was seen pulsating, throbbing, in the depth of the wound, and not as much blood lost as in an ordinary case of amputation, with all precautions taken against it. This symptom was entirely unexplained until the minute structure of the arteries was thoroughly studied out by modern anatomists and surgeons, and made known to the world mainly through the discoveries and writings of Jones.

We leave the explanation of this subject for the present, reserving its consideration for a future page.

Again: the wound does not partake at all of the nature of those produced by contusion, and exhibits no tendency to sloughing. There is also a remarkable absence of all the alarming symptoms which supervene usually on such accidents. The pain, swelling, fever, and constitutional symptoms, which it would be natural to expect from so large a separation of important tissues, are seldom excessive, and yield readily to a moderate antiphlogistic treatment. Suppuration is soon set up on the surface of the wound. Granulations spring up as readily and speedily as they would from an open incised wound, whose edges have not been approximated, and proceed naturally towards a cure.

Before offering the observations I propose in explanation of the phenomena peculiar to wounds of this nature, I shall proceed to detail the particulars of the case, to which I have referred, in which the right arm and scapula were forcibly and suddenly torn away from the body, by becoming engaged between a twelve inch band and the drum of a steam engine in rapid motion. The peculiarities of wounds of this nature are so characteristically set forth in the history of this case, that I cannot convey a better idea of them than by giving its details.

On the 11th of December, 1857, my father, Dr. Elias Horlbeck, was called to visit Cæsar, (belonging to Mr. T. B. Lucas,) at West Point Rice Mills, who was reported to have been grievously wounded in some machinery at the Mill. He arrived very soon after the accident, and found the patient to be a muscular negro, from thirty-five to forty years

of age, who, while putting pounded resin upon a twelve inch leather band, (to keep it from running off,) in revolution round a wheel in motion, had his hand engaged between the band and wheel. His efforts to disengage it were unsuccessful, and the hand, fore-arm, arm, etc., passed rapidly between the wheel and band. It was carried round until the shoulder came to a large beam which stood up and down alongside of the wheel, about one and a half inches from it. His body stopped, of course, as soon as it met with the resistance of the timber, and his arm, carried on in the revolution, was torn from the trunk at the shoulder. He fell to the ground from the platform on which he was standing, about three or four feet high, got up, unassisted, and walked to the outside of the door of the mill, a distance of about twenty or thirty yards. Here he became faint, and fell to the ground; he was immediately taken up by some of the workers of the mill, and removed to the Hospital on the premises, where I found him a short time afterwards, having arrived there a few minutes after my father. On examination an irregular, oval wound disclosed itself, the greater diameter of which was from above downwards. It extended from the humeral end of the clavicle (which was covered by skin,) to the lower border of the axillary space, for about six inches, and from side to side about three inches, in the site of the axilla. The surface of the wound was covered by a thin coagulum of blood, and the extremities of three or four large nerves of the axillary plexus, were lying bare, like whip-cords over it. The depth or centre of the wound was filled up by a rather thicker clot, which could be distinctly seen moving up and down with each pulsation of the axillary artery. The patient lay on his left or sound side with his clothes on, as when he fell. He complained of some pain from the soreness of the wound, but said that he suffered no pain at the time of the accident, and only discovered his loss as he saw his arm fall out from the wheel when brought around by the revolution of the band. His pulse was beating naturally, seventy-six strokes to the minute, regular, and of good firmness. He had lost but little blood, the quantity

could, of course, not be exactly measured, but there was a coagulum on the floor of the mill, where he first fell, amounting to about a pint or two. He had on a cotton shirt and an under one of close-fitting flannel. These were both torn through circularly, at the shoulder joint, and were more or less stained by blood, but not by any means saturated. Without disturbing them, farther than to cut away enough to expose the entire extent of the wound, a soft, folded linen cloth, dipped in cold water, was applied, and he was made as comfortable as possible.

The question now came up for considerstion whether it would be advisable to disturb the parts and attempt to take up the main artery, or to submit the case to the efforts of nature. The patient being quiet, and no immediate indication for interference presenting itself, the latter course was decided on. The reasons for so doing were the fact that on examining the artery on the separated arm, it was found to be divided low down, where the axillary becomes humeral, leaving considerable interval up to the next collateral branch, amply sufficient for the formation of a clot, and the effusion of plastic lymph for its permanent closure. The portion, too, on the arm was not gaping, as when an artery is cut across, but was closed by the external cellular coat being drawn out to a finely closed point, and twisted, closing its extremity as if by torsion. A similar condition, it was expected, existed at the extremity within the wound. These circumstances, with the recollection that in the celebrated case of Samuel Wood, (known as the Cheselden case,) analogous to the present, no after bleeding had taken place, induced us to leave this large artery untied.

The care of the case was confided to me, with instructions to restrain any hæmorrhage which might arise, until assistance could be obtained. The circulation continued during the afternoon, natural. The patient had a good, firm pulse, with normal warmth of skin, no shock was manifest, and he was very quiet, complaining only of soreness, and moaning now and then. He was directed to have a full dose of Tinct. Opii given him, and to be kept in the greatest quiet.

I was detailed, in company with my fellow students, Messrs. Palmer and Cain, to sit up with him, in the event of accident, being provided with all the means and appurtenances for arresting hæmorrhage. We watched the case with great interest and some anxiety, but happily the necessity did not occur, and we were not troubled on that score. He slept most of the night, with little or no restlessness. The cold cloths were removed as soon as they became dry, and fresh ones substituted; and towards morning thirty drops more of laudanum were given him. The weather was very cold.

Dec. 12th. On examination to-day, the surface of the wound presented a similar appearance to that of yesterday. The superficial coagulum still covering the wound, and the arterial pulsation very apparent, but the extremity of the artery was completely covered by the clot, and could not itself be seen. In the situation of the vacancy left by the abstraction of the scapula, behind, a large tumefaction had taken place, occasioned by effused blood in a coagulated condition. Anteriorly, in the situation of the pectoral muscles, the swelling was slight, there being no effusion here in consequence of no cavity being left by tearing out of the muscular substance. General condition of the patient very satisfactory, no constitutional or febrile disturbance having been set up. The pulse was rather stronger than natural, not frequent. There existed no thirst, and he experienced some desire for food. In the evening thirty drops of Tinct. Opii again administered, to ensure quiet. The cold water applications constantly applied.

Dec. 13th. To-day there occurred no circumstance requiring interference, except the removal of his bloody clothes, which had become dry and stiff, and which had not been changed from fear of disturbing him. No pain or inconvenience from the wound. He complained of a lapsided feeling of uneasiness, caused by his having to lie principally on his left side and back. Some serous exudations occurred to-day from beneath the skin on the back. Gruel, and bread, and tea, for nourishment, were all that was given him. His bowels were moved naturally, without assistance. There is no

necessity to pursue the daily report of his case, as no symptom occurred worthy of being detailed.

Dec. 16th. The whole of the external coagula had by this time separated. Suppuration from the region of the scapula commenced, and coagula of blood, mixed with blood, could be pressed out. No discharge from the region in front. The tumefaction in the region of the pectoral muscle subsiding. The pulsations of the artery were by this time not so marked. The great apprehension of secondary hæmorrhage slowly diminished, and we began to indulge a confident hope that we should escape so disastrous an event.

Up to this time I had been with him a great deal, sharing the nursing with my fellow students, and no drop of fresh blood had issued from the wound. Our patient was seated up a short while in bed, and was as cheerful as if laboring under an ordinary and trifling wound. From this time long adhesive straps were laid across the wound, approximating the edges, and far enough posteriorly to exert some pressure upon the cavity left by the torn out scapula.

Dec. 20th. Discharge diminishing from the sinus on the back; whenever it was dressed the purulent contents were pressed upwards, then outwards. From this time there was a gradual contraction of the sinus.

Dec. 29th. The skin had entirely adhered to the parts beneath, and only the space left by the loss of the arm in front to be healed. This was filled with healthy granulations, and rapidly progressing to cure.

Jan. 20th. Six weeks after the accident the parts were entirely healed over, and the patient was walking about attending to business.

AUTOPSY OF THE AVULSED ARM.

Upon inspection, the limb presented the appearance of having been subjected to much pressure. None of the bones of the arm or fore-arm, however, were fractured. The scapula on the contrary, was broken in a good many places, probably

on account of its irregular shape, and from the amount of pressure which it had been subjected to in passing between the wheel and band. The upper part of the arm was entirely denuded of skin, as though it had been separated to form a flap for the stump; it was torn off below the shoulder joint in an oblique direction, the greatest deficiency being on the inner side of the humerus, leaving the cellular sheath of the vessels bare. On separating it, the extremity of the artery was found free from cellular connections for about one-fourth of an inch. The mouth of it was not open and gaping, as though a clean cut had been made, and the end presented a cone-shaped appearance, closed by a slight twisting, as if by torsion. The external coat and sheath being drawn to a fine point, an inch was cut off and put in spirits. The entire plexus of nerves were separated below the level of the head of the humerus, accounting for the length of the nerves remaining on the trunk.

Muscles left entire on the Avulsed Arm.—Infra spinatus; teres, minor and major; all of the deltoid, with the exception of a few fibres at its insertion into the clavicle; supra spinatus, very much torn, but all brought away; about three inches of the latissimus dorsi came away with the arm; same quantity of the great pectoral; no traces of the lesser pectoral, the coracoid process being bare at its point of insertion.

Neither could any part of the rhomboidei be seen. Three inches in width of the whole insertion of the serratus magnus was torn off with the arm. The teres muscles, although both very much lacerated, came away entire.

The coracoid process of the scapula was broken off at its base, but kept in position by the short head of the biceps. The infra spinal fossa was very much broken up, the spiculæ running parallel with the outer border of the scapula. Posterior portion of the scapular border, one and a half inches, detached from the body of the bone, but connected by muscular tissue.

The posterior superior angle, corresponding to the attachment of the levator anguli scapulæ, cannot be found, and we

suppose it to be the only portion of the scapula which may have been left behind.

The entire clavicle left on the patient separated at its articulation with the acromion.

There are three cases of a similar nature to the one just described, reported in the annals of Medicine, but as the incidents of Cæsar's accident furnish us with every circumstance relating to a proper knowledge and appreciation of the injury, and the means by which a cure is effected, it will not be necessary to record them at length, but only to make some general observations respecting them.

The case, known as Cheselden's celebrated case, of a miller having his arm and scapula torn off in a mill, and so noted in a recent collection of Surgical cases by Paul F. Eve, did not in reality occur to Cheselden. It is described in the 449 No. of the Phil. Trans. for the year 1737, by Mr. John Belchier, F. R. S., then Surgeon to Guy's Hospital, and was treated in the wards of Mr. Fern, of St. Thomas' Hospital. The subject was Samuel Wood, aged twenty-six years. "He went to fetch a sack of corn from the further part of the mill in order to convey it up into the hopper. He carelessly took with him a rope, at the end of which was a slip knot, which he had round his wrist, and passing by one of the large wheels the cogs of it caught hold of the rope, and he not being able to disengage his hand immediately, was drawn towards the wheel, and raised off the ground till his body, being checked by a beam which supports the axis of the wheel, his arm, with the shoulder blade, was separated from it." He was not sensible of pain at the time; felt only a tingling about the wound, and being a good deal surprised, did not know his arm was torn off till he saw it in the wheel. He descended by a narrow ladder to the floor, and walked near one hundred yards distant, towards the house, when he fainted away from the great less of blood. The wound was covered with loaf sugar by the attendants. When the Surgeon arrived, there being no effusion of blood, the fleshy parts were brought together by a needle and suture bandages, etc. The patient recovered in about two months.

The second case is described by S. F. Scarnell, of Essex, England, in the Lancet, 1832, Vol. xxii, as occurring in a boy aged thirteen. It is there mentioned that the vessels were secured, but no detail of the quantity of blood lost, nor of the necessity of recurring to the operation. Some sloughing occurred, in consequence of the severe bruise of the integument which covered the top of the shoulder between it and the tiller of the mill. The portion of the clavicle which was exposed, as far as the deltoid had its origin, and which projected like a finger, was removed, and covered with that part of the integument which remained.

In the third case, by John Braithwaite, London Medical Gazette, 1833, the subject was a boy, twelve years old; and it occurred while working at a carding machine. He fell, as supposed, in an epileptic fit, to which he was subject, and was caught by the right arm by a revolving strap, which carried him up to the ceiling, and tore off the arm and scapula. In this case also, from apprehension of the inefficiency of the powers of nature, although there was no very profuse hæmorrhage, an attempt was made to secure the artery, which, failing at first, caused a violent gush of blood. It was, however, afterwards taken up by a tenaculum, and the vein was also ligatured, and a small artery. The edges of the wound were then approximated by two ligatures; in consequence a large cavity was left over the site of the scapula. A seton was passed through this to excite the growth of granulations, without effect; but adhesion of the entire flap was subsequently effected by injecting a solution of alum.

In the two last cases the cure was not left to the unaided efforts of nature, ligatures having been applied to the main arteries. Of their necessity I shall not express a positive opinion; according to the record there did not appear to be an absolute need for so doing, inasmuch as the ligature was applied some time after the receipt of the accident, when there was no hæmorrhage going on, and in all probability there would have been none, had they been left to nature

alone. The patients would have bled to death long before the Surgeon arrived, had it been necessary.

In the case of Samuel Wood alone the parallel exists with that of Cæsar, since they both had their arms torn off in a similar manner. Both were unconscious of their loss at the time; both walked a considerable distance after having received their injury; both recovered without a ligature having been used to secure the vessels; and both were adults. And here a question presents itself for solution; why is it that in these cases the arm has never been separated from the body at the humero-scapular articulation? The solution of this problem is to be sought in the anatomical relation of the parts. For the performance of the various movements of the arm, the scapula is, except at its small connection with the clavicle, (which presents no great resistance) not connected with the body by articular ligaments, but entirely by the intermedium of muscles, two of the largest in the system, the pectoralis major, anteriorly, and latissimus dorsi, posteriorly, being inserted on each side of the groove in the humerus, for lodging the long head of the biceps. When these are torn through, the scapulo-humeral joint is protected by those larger fleshy muscles which arise from the scapula and are inserted into the humerus, and with the strong, unyielding tendinous and ligamentous fixtures of the joint itself, oppose a more powerful resistance than the other muscles, arising from the trunk, and inserted into the different parts of the scapula. Hence it will be seen that all the muscles originating from the scapula and inserted in the humerus have escaped unhurt, (or only injured by mechanical pressure,) along with the scapula; whilst the tearing through has been effected in the latissimus dorsi and scapular muscles. It is easier to separate a limb from the body at any other articulation, than the scapulo-humeral.

We propose, in the next place, to consider the various symptoms which have characterized our case. The first in order is the very slight pain which the patient suffered at the time of the accident. The most plausible supposition which presents itself is that this arises from the extreme sud-

denness and rapidity with which the rupture is effected. The brain has not time to perceive the sensation whilst the injury is taking place. Universal experience on this subject is that at the time of the accident the patient is entirely unconscious of real pain, the only feeling approximating it being described as a shudder.

Upon the rapidity and unexpectedness, then, with which the lesion is effected, we base our explanation of absence of pain.

Among the peculiar symptoms characteristic of the affection, we noticed the almost entire absence of nervous shock, the harmony of the great functions of life being almost undisturbed. We are here much embarrassed to account for this curious fact. According to our text books, we should look for shock particularly, in affections of this description, as, they say, the more sudden and unexpected the accident, the more shock will usually occur. This is conspicuous in some gunshot wounds, and certainly cannot be attributed to loss of blood. How is it that these injuries do not partake of the nature of contused wounds? This is explained by the fact, that all the bruised and contused tissues have been entirely removed, and only those of a sound and healthy nature left, which are soon brought under treatment.

We now take the next division of our subject, which will be a consideration of the mode of separation of the various tissues taking them in the order in which they are ruptured, their physical peculiarities, and the reason that they are so affected. This will necessarily imply a notice of their minute anatomical structure, which we hope will not prove entirely uninteresting. When extension is made upon the arm, the body holding the relation of a counter extending power, the ligaments, muscles, tendons, vessels, nerves and skin necessarily experience great tension. In proportion as these tissues are susceptible of elongation, or according to their elasticity, is the force exerted upon them. Besides the force does not act on them in the same degree. The parts yield one after another; the most tense yielding first, and the most distensible last. It is on this account that

the great strain is first experienced by the unyielding ligaments of the articulation. Those ligaments become tense, and if the power applied be greater than the resistance, they must be either torn through, or out with the part of the bony tissue to which they are attached, but, as in the instance of the shoulder joint, the compound influences of tendons and ligaments present a greater resistance than the muscles, and instead of the limb being separated at the joint, the amount of force of traction is communicated to the muscles, which hold the scapula in its position. These, being less resisting than the ligamentous tissues, yield and are perhaps the first torn through. In cases like the foregoing, then the muscles are always the first torn through. No sooner are the extending influences applied, than they are stimulated to contraction. This is remarkably displayed in the attempt to reduce a luxation by the pulley. As soon as the ropes are tightened, they become tense and corded, and it is only after they have become fatigued by a long application of the power, that the extending and counter extending forces have the opportunity of acting on the ligaments of the joints. Modern surgery has been able to overcome this opposition, by placing the patient under the influence of chloroform, or diverting his attention to some other portions of the muscular system, when the muscles taken at a disadvantage do not oppose their resistance, but the head of the bone may slip into its place by manipulation alone. Again, in the recorded history of the punishment of Ravaillac, the murderer of Henry IV., who was sentenced to be torn asunder, by having a powerful horse attached to each of his four limbs, we are informed that it was necessary for the executioner to divide the muscles of the arm and thigh before the powerful force exerted by the horses could succeed in directly acting on their ligaments.

Let us enquire what portion of the muscle gives way when torn through by such a force as we have been considering. Each muscle is surrounded by a layer of cellular tissue, and some by an aponeurosis, which serves to maintain them in proper position, and to furnish points of attachment;

each of the fasciculi which compose it, is equally surrounded by a cellular sheath. The thickness of the layers diminish successively as it penetrates into the interstices of the fasciculi, the envelop of which furnishes interior prolongations which unite the fibres between themselves. As to the minute anatomy of the fibres and fibrillæ of muscles, whether their envelop is membranous or formed of a gelatinous matter, which glues or covers externally the fibrillae without impairing the movements, as Dugès in his Traitè de Physiologie Comparè thinks, does not concern us here. It is only necessary for us to remember that the extremity of the muscular fibres are directly continuous with a ligamentous tissue (known to us as tendon) through whose intermedium their action is transmitted. Example of rupture of individual muscles are sufficiently numerous. Some authors would have us believe that the tendinous structure alone is capable of rupture. Again, there are others who think that rupture occurs only at the continuation of the muscular with the tendinous structure. Bichat, who advocates the rupture of the tendon, in his *Anatomie General*, vol. 8, page 153, tries to explain the phenomena by saying, that when it happens, the fleshy fibres are in contraction, while the tendinous fibres are as it were passive, and the contracted muscle acquires a hardness and density greater than that of its tendon. However that may be explained, I am by no means sure that rupture takes place oftenest through the tendon. I am inclined to think the muscular tissue most frequently gives way. Mons. Sedillot in twenty-one cases where the seat of lesion is indicated, reports that in thirteen cases it took place at the point of insertion of fleshy fibres into the tendons, and eight times in the body of the muscle itself. In eight cases reported in Memoires de l'Academie de Chirurgie, vol 2, p. 79, under the article "plusieurs observations sur des membres arraches," where the fingers were torn off with tendons attached, the rupture in most of these, took place at the junction of the tendon and muscle, and in several, some of the fibres of the muscle came away with the tendon. The rupture of the tendon seems to take place generally

when it is subjected to great traction, as in sudden, unexpected movements, where it is necessary to hinder a fall. Such is said to have been the case with John Hunter, who ruptured his tendo achillis while dancing, and it is said to be no very unfrequent occurrence to have the tendon of the long plantar of the foot ruptured. In the case which I have reported, the rupture took place in every instance *through the muscular tissue.*

The junction of fleshy fibres with tendons has been clearly demonstrated by Peaslee, in his recent work on Histology, to be a direct continuation of one tissue into the other.

The ligaments will next engross our attention. When avulsion takes place through a joint, this tissue will always be the first part torn through; the reason is obvious and readily explicable. The ligaments are composed of white fibrous tissue. Two of its chief peculiarities are its inextensibility and entire want of elasticity. Not being disposed to yield, it first is subjected to the force of the traction, and if that is great enough, it is ruptured. In the case which I have reported, none of the ligamentous tissues were torn through, except those of the acromio-clavicular articulation.

In the sequence of parts which yielded, the arteries and veins may next come up for discussion. Preparatory to enquiring why little or no hæmorrhage takes place, and into the process instituted by nature for its arrest, it will be necessary to take up the minute anatomy of the arteries. In a general way we say they are composed of three coats: internal, external and middle, which are in their turn more or less divisible.

We consider first the internal or epithelial layer, composed of oval epithelial scales, the long axis of them being parallel with that of the artery. These scales make up a fine transparent colorless membrane, smooth and polished, it serves as the lining along which the blood flows with great facility, it is elastic, but quite brittle aud readily torn through. Todd and Bowman, among the most reliable of modern observers, believe that the internal coat is nourished by the blood flowing through the artery, and not by the vasa vasorum.

The middle coat is made up of yellow elastic fibrous tissue, which is arranged in a series of concentric layers around the arterial tube; it is by means of this coat we have the greater part of the wall of the artery formed. It is much more elastic than the internal coat, and it is through this property we see the cut artery kept open in the dead subject. This coat is readily broken through by a ligature, as it is possessed of but few longitudinal fibres, and the circular ones are loosely connected together. Modern observers divide the middle tunic into two separate coats: the circular fibrous, and the longitudinal fibrous.

According to the authority of Todd and Bowman, "we have no evidence that the blood vessels penetrate through the external coat further than to a slight depth into the fibrous tunic. It is probable, therefore, that they are destined to nourish the external tunic, and a portion (chiefly the muscular element) of the fibrous tunic, leaving the remainder of the arterial wall to imbibe its nutrition directly from the blood itself."

Lastly, the external coat, which is composed almost entirely of condensed cellular tissue. This is the stronghold, if I may so say, of the artery, and it is a tissue of much importance, and exceeding interest to the surgeon. Upon its physical properties of extensibility, toughness, and great power of resistance depend all his chance of success with the ligature. It serves as a support to the other tissues, and as we said above, in some measure as a medium for their nutrition, as through its walls are transmitted the vasa vasorum, nourishing vessels of the arteries, together with the nerves, veins and absorbents. As arteries have a similar organization in these respects with other tissues of the body, it is of the utmost importance to the arterial tube, and there is no coat from the loss of which an artery suffers so much, as from this. Upon it depends all the reparative changes necessary for the arrest of hæmorrhage in operations. The experiments and writings of Jones most satisfactorily explain the process of the suppression of hæmorrhage. "It is not," he says, "a simple effort, but a process performed by the con-

current and successive operations of many causes. These may be briefly stated to consist in the retraction and contraction of the artery, the formation of a coagulum of blood at its mouth, the inflammation and consolidation of its extremity by an effusion of coagulable lymph, within its canals between its tunics, and in the cellular substance surrounding it." This is the summary which Jones gives of the process of arresting hæmorrhage by the efforts of nature.

Beclard compares the end of a vessel thus plunged into coagula, to the neck of a bottle obstructed both by cork and sealing wax, which covers it. To complete the progress of the cure there is now set up a process by which the obliteration of the artery becomes definite; an organizable matter is thrown out by the divided vascular ends of the internal coat, (Jones) which adheres strongly to the cellular membrane, and when fully organized, makes a firm cul de sac. If the internal coats are *not vascular*, as Todd and Bowman would have us believe, it would seem that the coagulable lymph is thrown out, not by them, but by the cellular coat alone, and the explanation given by Jones may have to be altered. The same organizable matter is thrown out around the artery in the surrounding cellular tissue. Granulations spring up and the whole becomes merged, by the general reparative process of granulations and cicatrization. These effects taking place in vessels of considerable size, they will be very much more easy on those of smaller calibre.

When a ligature is applied to an artery the two inner coats are divided, while the external resists. The ligature supplies the place of the coagulum of blood at the mouth of the vessel. Effusion of coagulable lymph is thrown out, which glues up the extremity of the vessel, before the ligature cuts through the external tunic, uniting the parts permanently. Mr. Jones, the Surgical authority, in explanation of the principle of the ligature, and the modes by which nature succeeds in arresting hæmorrhage, does not seem to have considered the condition of a divided artery in a torn off limb. By investigating the mode of injury to the vessel in such a wound, we shall find a beautiful analogical illustration of the principle

of the ligature. As traction is made upon an artery, the inner and middle coats, which have no great degree of resistance lengthwise, and are susceptible of distension only to a limited extent, give way, when the extending force has surpassed their capacity of resistance. Both ends immediately contract and retract. The force being still applied to the external coat it now yields, and, after having been drawn out, is in its turn, torn across. From the observation of the specimen of the artery attached to the limb, it would seem that the attenuated extremity of the vessel receives a spiral twist, much in the same way as if slight torsion had been resorted to for closing it. The difference between an incised wound, cutting through an artery, is that after syncope an external clot first stops the flow, whilst in the torn artery a mechanical obstacle is presented, by a solid and closed canal, formed in consequence of the manner in which it is divided. Under these conditions a coagulum of blood forms within the vessel, up to the next collateral branch. Jones thinks that this internal clot contributes very little to the arrest of hæmorrhage.

En resumé, I would say that the principal reasons why so little hæmorrhage take place, are the irregular manner in which the internal coats are torn through; then after these give way, the force is expended in drawing out the external coat to its utmost capacity, portions of it yielding until the inelastic cellular structure is drawn out to a fine point, comparably, as has been said, to a glass tube under the action of the blow-pipe. This, giving way at length, retains its form, and by some mechanical power, not satisfactorily explained, receives the turn or twist, which, as far as I know, has been first taken notice of in the autopsy of Cæsar's arm.

Having now explained my views, *tant bien que mal*, of the reason why so little blood is lost by the arteries of an avulsed limb, it only remains to say that venous hæmorrhage is arrested, after a very short time, by the valvular arrangement of those vessels, and as no blood returns to them, they could only bleed by a kind of regurgitation, which does not and cannot occur, on account of the aforesaid valves.

ART. II.—*Description of a new Hysterotome, for the cure of Dysmenorrhœa.* Invented by Dr. O. A. WHITE, of Charleston, S. C.

Dysmenorrhœa is certainly one of the most painful and intractable disorders we can be called upon to treat. It is of very frequent occurrence, and is the fruitful source of much ill health to females. Commencing, as it has often been known to do, even with menstrual life, it may proceed unchecked throughout successive years, and yield at last only to time and the natural cessation of the discharge. If unrelieved, a trifling case is liable to become worse and of longer duration, until finally, the afflicted patient may be only beginning to recover from the shock of one period when the dreaded approach of the next is near at hand. It is, therefore, not at all improbable that many unfortunate women have fallen victims to consumption and other terrible maladies, in consequence of these periodical sufferings.

Under such circumstances, the attention of accoucheurs must ever have been directed to its relief. And such, indeed, has been the case, though it is mortifying to reflect that until a comparatively recent date the blindest empiricism appeared to direct the treatment. Incorrect views respecting its pathology inevitably led the way to notable errors in practice, and the wretched applicant for relief was too often subjected to struggle not only with the original malady itself, but also against the evil effects of injudicious means used to remove it. Thus, at one time, obstetricians erroneously attributed all the phenomena of this disease to plethora, when cups and lancets were indiscriminately applied. At another, debility was the prevailing doctrine, and tonics became the fashion. At another, spasm was obscurely hinted at as the cause, and opium was the only specific. At a still later period we hear of nothing but its nervous and neuralgic origin; this was the day of active temporising,—main reliance being placed upon inunctions with belladonna, acconite, and the like; whilst, behold, at

another, the very identity of the disease was called in question, and ovarian irritation and inflammation imputed the source and origin of all the manifold suffering that was certainly uterine in its seat.

Such were many of the vague notions which prevailed respecting dysmenorrhœa, anterior to the year 1823. About this period Dr. Mackintosh, of Edinburgh, first suggested the employment of mechanical means for its relief. Being led to investigate this subject by accident, he forthwith commenced a series of examinations with experiments, which resulted in convincing him that preternatural narrowing of the uterine canal was most commonly concerned in the production of this complaint. This point being established, he next proceeds to account for the alarming symptoms so frequently met with, in the following manner: "The menstrual discharge, after it is secreted in the uterus, cannot readily escape, in consequence of the small size of its orifice; distension of the organ is the result, which, by exciting the contraction of its fibres, produces uneasiness and pain in the pelvic region. When the os uteri is very small and the secretion viscid, or mixed with coagulated blood, shreds of membrane, or organized masses, then the distension becomes more considerable, and stronger contractions are excited. Sometimes the action of the abdominal muscles is called into play, and bearing down pains are produced, resembling in every particular the pains of labour, and continue until the expulsion takes place."

These observations, originally entertained and expressed so many years ago by Dr. Mackintosh, have since become generally adopted; and though improvements in the management of these cases are still being made, no one can fail to observe that the doctrines promulgated by that illustrious practitioner continue to influence surgical opinions and practice even to the present day.

Dysmenorrhœa, therefore, being proved to result most commonly from mechanical causes, surgical remedies for its control have largely superseded the vaunted resources of the pharmacopœa. But though surgical appliances

are oftenest available in the management of this disease, we must not lose sight of the fact that there are also a certain proportion of cases which require constitutional measures alone for their cure. Let me not, therefore, be thought to accord to surgical interference too great supremacy, experience having long since assured me that by far the largest number are amenable only to a careful and judicious institution of both plans of treatment combined. If, therefore, we adopt one of these modes of relief, and attempt to practice it to the exclusion of the other, we shall too often be baffled in our best directed efforts, and find the disorder liable to return, even after its apparent removal.

The method advocated by Dr. Mackintosh consists, as will be remembered, of careful and repeated dilatations of the os and cervix uteri, with bougies of various sizes. But the same evil consequences which often eventuate during the management of ordinary urethral stricture, by dilatation, were so liable to arise during this course of treatment here, that surgeons were soon compelled to abandon this proceedure and look to measures more expeditious, and against which no such objections could be alledged.

Accordingly, about twenty years after, Prof. Simpson announced to the profession another mode of treating this obstinate complaint, by an experiment at once novel and brilliant. This was by incisions skillfully made within the cervix uteri with appropriate instruments, and keeping the parts afterwards dilated with sponge tents until the cure was complete.

In behalf of this safe and simple operation it is contended that it at once places the parts in somewhat the same condition as subsequent to parturition or a miscarriage. For it is a notable fact that many cases of otherwise intractable dysmenorrhœa have been known to abate or cease altogether after one or other of these occurrences have transpired. As to the efficacy of this mode of operating, it would be needless here to recount all the high testimony recently added in its favor. Suffice it to remark the practice has

since been adopted by many of the most eminent accoucheurs and surgeons in this country and elsewhere.

Promising such certain and expeditious relief as this procedure certainly does, the facility with which the several steps of the operation may be performed cannot fail to interest surgeons. Already, therefore, have instruments of different construction been suggested, each having in view that important design. That invented by Prof. Simpson has, thus far, very justly commanded the greatest favor. This, it will be remarked, however, is but a modification of the *Lithotome Caché*, which, though a contribution of considerable value in its proper place, to the armamenta of Surgery, is certainly liable here to objections which readily suggest themselves to all who attempt its use.

FIG. 1.

Having been compelled, upon several occasions, to resort to the practice and the instrument of this eminent master, for the relief of obstructive dysmenorrhœa, the peculiar dangers which embarrass its use compelled me to lay it aside and employ one invented by myself, of the following description.

Annexed is an engraving, which, though considerably reduced from the original, will assist, I hope, in giving an adequate idea of this instrument. It consists, as will be seen, of a long and delicate staff or stem affixed to a handle of unusual form. The staff is about the calibre, and has somewhat the same curve as a No. 3 small catheter. It is full nine inches in length, and is so plainly notched and numbered off upon its posterior surface, from the point to the handle, that these measurements may easily be distinguished by the touch alone.

At a moderate distance from the point of the instrument may be noticed two small blades, jutting out from the staff on either side. Each of these blades is capable of being thrust out about a quarter of an inch, or concealed by being withdrawn again within its sheath in the circumference of the staff, by means of a certain manipulation at the handle, which I shall take occasion presently to describe.

The standard depth of the womb from the os to the fundus uteri has already been ascertained to be just two inches and a half. To render, therefore, the application of this instrument safe, and in order to bring its utility within the range of possible cases of even smaller uterine dimensions, a point about an inch and a half from the apex of the stem has been selected, from which the knives are made to protude when needed.

The projection and closure of these blades is effected with great facility, by means of a female screw, *(a)* which plays upon a thread *(b)* situated at the very butt of the handle. This screw is hexagonal in shape, each side of which is made to bear a number ranging from one to six. The instrument being closed, No. 1 corresponds exactly to the convex surface of the handle, but progressively, as the succeeding numbers are brought upwards, by turning the screw, the blades are forced out to the extent desired; No. 6, of course, denoting the maximum. The value of this little contrivance will be obvious, it being by this means alone that the operator can learn at a glance to what degree the blades have been opened or shut, after that portion of the stem which contains them has been introduced, to perform its duty within the uterine orifice.

FIG. 2.

The second figure represents an anterior view of that section of the stem which holds the blades, the upper surface being removed in order to display the mechanical principles upon which they act. The stem, therefore, is seen to be

hollow, and the two blades, lying back to back, are held together, and fastened by means of a single pivot to a small steel wire, which is made to pass quite through the centre of the instrument, from *c* to *b*, fig. 1. When closed the blades, of course, overlap each other, to economise space, but obedient to the screw, (*a*) they are forced to part, one on each side of the stem, in consequence of the steady resistance opposed to the inclined plane of their backs, in the shape of a wedge.

The handle is made of some light material; is intentionally flattened upon its posterior surface and convex upon its anterior. The convexity of the handle corresponds to the curvature of the stem, and by referring, therefore, to this distinction, the operator is constantly reminded of the direction of the point of the instrument, even while hid to the handle within the vaginal passage.

To incise the neck of the womb with this instrument the patient is made to lie upon her back, in a convenient position. The forefinger of the right hand is inserted into the vagina, and its extremity held in contact with the os tincæ, to act as a guide. The blades being closed, the instrument is next held in the left hand, and its point carefully slipped along the palmer surface of the finger, until it is finally directed, in this manner, through the cavity of the cervix and within the os internum. It is then maintained in this position until with the right hand, the blades are forced out to the extent desired; when, by simply withdrawing the instrument, in the same direction as introduced, the operation is fully accomplished. To complete the cure, all that remains now to be done is to prevent re-union of the cut surfaces, and this is effected by means of a sponge tent, or a permanent metallic bougie, similar to that recommended by Prof. Simpson. Whichever is selected should be worn within the parts for several days, and only changed as often as cleanliness requires.

My main objects in communicating this paper have been not merely to record my approval of Prof. Simpson's mode of treating certain cases of dysmenorrhœa by incision, but to

introduce to the profession a new surgical instrument for effecting this purpose. I trust that its credit rests not so much upon its originality and comparative cheapness as upon its especial adaptation to the relief of all classes of strictures in which internal cutting would be admissible.

The instrument in my possession is one which was neatly and successfully fabricated at the factory of Mr. George Tieman, No. 63 Chatham St., New York. The skill of this veteran instrument maker is already too well known to need encomium.

In conclusion, I would simply add that this instrument needs but a single application to effect in these cases what it requires at leas two or more attempts with any other to perform. It offers, consequently, an easier, more expeditious, safer, and, upon the whole, less painful mode of operating than any other I know of. I therefore recommend its use to my professional brethren, hoping that it may be found of as much service in their hands as it has so far proved in my own.

ART. III.—*An Essay on the Importability and Diffusion of Yellow Fever.* By the late Dr. P. C. GAILLARD, Professor of Practice of Medicine in the Medical College of the State of South Carolina.

The following article, written by the late Dr. P. C. Gaillard, was intended as the introductory chapter to a work on Yellow Fever. The scope and objects of the work will be best explained in his own words. He says: "It is my design in the following pages, to give some account of the epidemics of yellow fever which I have observed in Charleston; and I offer this volume only as a contribution, on my part, to the history of that disease in our city. I shall, therefore, in all I have to say, rely upon facts observed by myself or furnished me by my friends, and recorded as soon after their occurrence as possible. It has been my habit, for many

years past, to preserve in my note book every thing of interest which I learned in relation to yellow fever, so that my records will, I trust, make amends in truth and accuracy for whatever they may lack in interest."

His death, in the midst of his labors, prevented his completing even this initial chapter, the latter half of which has been put together since his death, from materials selected and arranged by himself. A subsequent article will contain the facts concerning the origin and progress of the yellow fever epidemics of this city in 1849, '54, '56 and '58.

The importance of these facts to the history of those epidemics, and the care and accuracy with which they have been collected are sufficient reasons for their being placed upon record.

In the history of diseases which, at times, become epidemic in places where they do not usually exist, or where they only occasionally occur, nothing is more important in determining their etiology, than a careful study of the earlier cases, with all the antecedent circumstances connected with them. This, simple as it may at first sight appear, is not in reality an easy matter. Independently of the time and trouble which a minute examination of the first cases of an epidemic require, of the difficulties so often encountered in tracing the antecedents of the unsettled class of persons who are usually its first subjects, we are often met by the doubt whether cases we have carefully traced out really belong to the disease we are investigating, or whether we may not have overlooked true cases, which had occurred anteriorly. Hence the necessity for having fixed rules of diagnosis, if such are possible. But even if such cases are established, first cases may occur to those who are indifferent to their importance or careless in their observation, and they may thus be passed over unnoticed or unrecognised. Or some observers may look for too great an aggregate of symptoms, or require the extremest,—as collapse in cholera, or black vomit in yellow fever,—before they will admit the existence

of the disease; while others, more facile, allow all cases bearing resemblance to it,—as diarrhœa to cholera, ephemera to yellow fever,—to be cases of the disease in question. Between these extremes it is very hard to strike the right mean. Others, again, from various motives, the chief of which is the fear of damaging the reputation for health, and injuring the trade of the place they live in, conceal well-marked cases which fall under their observation. With the moral involved in this course, however reprehensible I may think it, I have now nothing to do, but it adds another to the difficulties already enumerated in establishing the exact dates of the occurrence of the early cases of epidemics; and often, when we think that this has been done with great accuracy, the ghosts of doubtful or concealed cases are raised, to refute the most carefully recorded facts.

There are, however, difficulties inherent in the subject itself,—difficulties of diagnosis,—which may in some cases perplex, or even mislead, the most careful and conscientious. Certain cases, of so doubtful a character as to have earned, apparently by common consent, the name of "suspicious." are not unfrequently observed before the outbreak of epidemics. In these the diagnosis is really for some time doubtful, and it is necessary to watch carefully the progress, and, it may be, even the termination of the case, before it can be decided. This has repeatedly occurred with regard to yellow fever, from its earliest history, and has been the cause of endless controversies as to the genuineness of certain cases at the commencement of many epidemics. On the decision of this point often depends the solution of the entire question of the disease, viz: whether it arises from domestic sources or from the importation of foreign germs,—a question of the last importance in public hygiene, on which the prosperity, the health, and the lives of entire communities depend. It should, therefore, be carefully considered, and not be decided hastily or unadvisedly.

In Charleston the study of the earlier cases of yellow fever had long been neglected, until the publication of Dr. Strobel's book, in which he had attempted to prove that the epidemic

of 1839 was caused by the arrival of an infected ship and a sick crew, and to show from this that the disease is transmissible, i. e., is capable of extension, after introduction into a previously healthy place; one, at least, which had been previously exempt from it. The book was published in 1840. At that time, so great was the unanimity of sentiment here against the idea of yellow fever being of foreign origin, capable of importation and subsequent extension, that, seeing the turn things have since taken, we are almost disposed to agree with Lord Bacon in the opinion "that there is no worse augury in intellectual matters than that derived from unanimity." The profession, with scarcely more than a single exception, and the community generally, were so well convinced that yellow fever belonged to our geographical position, that they little thought of endeavoring to prevent its occurrence, by shutting it out. Its violence was to be mitigated by keeping the city clean, but after all it was thought to be a necessary evil, to be borne as best it might. Quarantine became, in consequence, almost a dead letter; and it is not surprising, with this state of professional and public opinion, that Dr. Strobel's book was received with very little favor, which was further lessened by great defects in the work itself. But its publication had this good effect: it called attention to the subject, and gave point and purpose to the doubts which had begun to spring up in relation to the origin of the disease. It also revived among us a doctrine which had been almost forgotten, viz: that of contingent contagion, as it has been called, being that modification of the doctrine of pure contagion, which requires, in addition to the contagious matter, favouring surrounding circumstances, or contingencies, to give it activity and life. This property in yellow fever Dr. Strobel called transmissibility, and he likened the local state of atmosphere to the magazine, and the imported germs to the spark, which would explode only when brought together, the spark being inoperative and the magazine safe without the contingency which might bring them into contact; and this, I do not hesitate to say, is, with very slight modification, the belief I entertain

of the origin and mode of extension of yellow fever, when it spreads beyond its usual abode. But, although fully convinced of the truth of this, I cannot expect so entirely to surmount the difficulties enumerated above, nor do I believe it always possible, even in the case of acknowledged contagion, so clearly to follow out the chain of cases as to make the proof amount to demonstration. Neither do I expect to cause those around me to abandon their long cherished views on this subject, based, as they think, on firmer foundations than those I offer. But I wish to record the deliberate opinion of one who has seen much of yellow fever, and who has given it no small degree of study and attention. If the work is well done, and its records are deemed sufficiently valuable to save it from early oblivion, it may have some influence in forming the opinions of those who are undecided, and of those who are entering on the study of the subject.

Although I believe the disease to be of foreign origin, and an exotic here in Charleston, yet, as already remarked, I do not think that the proof of importation each year that we have an epidemic amounts to demonstration. Neither do I believe that proof of this positive nature can be furnished in every case of epidemics of measles, scarlet fever, and even of small pox; yet these diseases are undoubtedly contagious, and are transmitted from individual to individual. But after they are introduced into a place it is impossible to trace cases to any near connection, or to communication of any kind with a previously existing case; and, on the other hand, we sometimes find that after introduction they die out without extending, or after having extended to very few. It is sometimes also impossible to trace their mode of introduction, or to do more than speculate on their origin. I repeat these facts, which may appear trite, because in the discussions on yellow fever they seem to have been forgotten, and because, when the latter disease behaves in a similar manner, the conclusion is at once drawn that it possesses no reproductive properties. In relation to small pox I shall have something more to say in this connexion further on. I would here merely recall the very judicious remarks of Sir

Henry Holland on the Method of Inquiry as to Contagion. If we consider carefully all the conditions which determine and modify the communication of disease from one person to another,—the condition of the person giving the infection; the state of the person receiving it; and the condition of the medium through which the transference is made,—we will, I think, agree with him in the conclusion that "they will be found to show adequate cause for all the strange and perplexing appearances of contagious diseases. So far from it being difficult to explain why a given disorder should occasionally appear infectious, at other times not,—why it should spread rapidly and virulently in some localities, and not at all in others,—why it should affect some persons and leave others free,—why the cases should be violent at one period, mild at another,—it is rather, perhaps, matter of wonder that circumstances are not still more varied and irregular than we find them."*

But, although I do not think the proof of importation in yellow fever always conclusive, it is sometimes so clear that I conceive it to be impossible for one reasoning logically and inductively not to admit this source of origin of the disease. This was, I think, the case in our city in 1849 and in 1854; and I shall presently adduce proofs from recent epidemics elsewhere too clear to admit of either cavil or doubt. If, then, this can be shown; if numerous instances can be adduced in which yellow fever broke out and prevailed in places where it did not previously exist, or even where it had never before been known, immediately after the introduction of cases of the disease, or after the arrival of infected vessels, or the landing and exposure of their cargoes, or of the clothes, bedding, &c., of those who had been sick, or which had come from an infected locality; and if the cases next occurring were within the immediate influence of either one or the other of these, are we not, in truth and candor, justified in saying that the disease was imported and spread by some property inherent to itself, rather than refer it to

* Medical Notes and Reflections. Second Edition. London. Page 279.

local causes which had for years been innocuous, and to meteorological states, which had, year after year, been inoperative? How apposite and how true is the following remark: "When I compare," says Dr. George See, of Rio Janeiro, "the present state of Rio with what it was twenty-eight years ago, and contrast its former salubrity, while reeking with filth and impurities of every conceivable kind, with its sickly condition, scarcely ever free from some epidemic disease, although greatly improved in cleanliness, well paved, well supplied with water, well lit at night with gas, and various other hygienic measures adopted, *I feel myself forced to look beyond the local condition of the city for the origin of yellow fever and cholera, which have lately come among us.*"*

Further: when we come to look into the local circumstances and meteorological conditions to which the origin of yellow fever has been ascribed, we find the greatest doubt and uncertainty prevailing among the advocates of this view, as to their nature and character. For instance, Bancroft and many others after him, regard yellow fever as only a variety of remittent fever, and as produced by the same causes; but this opinion is no longer maintained by those best acquainted with the disease, here and elsewhere. Dr. Barton, of New Orleans, again, refers its origin to an epidemic condition of the atmosphere; by which he does not intend any specific state, but merely a peculiar combination or coincidence of certain meteorological and terrene conditions; the former, probably forming the "pre-disponent," is more or less present every summer, but is innocuous without the other, the terrene condition, the efficient cause of all the New Orleans epidemics, and which consists mainly in "*extensive disturbances or upturnings and exposure of the orginal soil of the country.*"† Dr. La Roche, the latest, most erudite and pains-

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* Proceedings of Epidemiological Society, May 4th, 1857, in London Med. Times and Gazette, July 11th, 1857, P. 47.

† Report of New Orleans Sanitary Commission, P. 448. The idea of the upturning of the soil causing yellow fever was signally disproved in Charleston in 1857. Extensive excavations, to depths averaging 12 feet, and of an aggre-

taking of the writers who look solely to local causes for the origin of the disease, differs from both these writers. He regards yellow feqer as *quite distinct* from ordinary remittent fever, and he classes the explanation as to its production, offered by Dr. Barton, among those which he styles "purely hypothetical."* In regard to meteorological states he says: "They are to be viewed, for the most part, in the light of predisposing and exciting causes; and though some of them, as heat and humidity,—and, perhaps, to a certain extent electricity, light and certain winds,—may very deservedly be regarded as contributing, under peculiar circumstances, to the development and diffusion of the efficient cause, we are justified in the conclusion that, whether singly or combined, they cannot be admitted to possess the power in question."† Let us then see where he finds the efficient cause of the disease.

So varied are the circumstances under which yellow fever has prevailed, so diverse the conditions of the localities where it has appeared, that Dr. La Roche, after a careful examination of the whole subject, is induced to set aside all the local causes to which it has been attributed with a single exception, viz: the decomposition of ligneous material. After noting the greater prevalence of the fever where there had been extensive conflagrations, inundations, and upturnings of the soil; after pointing to its preference for the newly made ground, and for the low, damp, filthy and crowded portions of cities; after showing that decaying wood or ligneous vegetable matter is often found in localities where yellow fever broke out or prevailed, he arrives at the following conclusion in regard to its efficient cause, viz: that

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gate length of 13,892 feet, for laying new sewers, were made during the winter, spring, and early summer of that year in Spring, Meeting and Calhoun Streets. All sorts of material were turned up: old filling, original soil, &c., yet of the dozen cases of fever which occurred this year, not one is reported in either of the streets mentioned, through which the excavations were made.

* Vol. 2, P. 625. † Vol. 2, P. 189.

yellow fever is produced by an infection, the offspring of local causes, similar to, but not identical with, that of other malarial fevers, the sources of the effluvia being somewhat different, but both springing from the decomposition of vegetable material,—the one, yellow fever, from that of its ligneous constituent; the others, palludal fever, from that of its herbaceous portions. That I may not do injustice to Dr. La Roche, I will quote in full the two paragraphs in which he announces his views.

"The history of our yellow fever affords us," says he, "numerous examples of the disease breaking out and prevailing in localities where there was no vegetation at all, or where there could be none, and where, therefore, there could not exist anything capable of eliminating the principle in question,—the principle of vegetation. If the latter is supposed to proceed from the earth, we cannot explain the production of fever in barren soils, and in such localities as are usually visited by yellow fever,—in ships for example, for in these there is no vegetation, or the possibility of it; and it is impossible to suppose that nature would place the principle of vegetation and set it in operation in places where she has withheld or removed the elements required for the development of the process; while, on that supposition, all places would be liable to fever."

"To affirm, as has just been done, that yellow and palludal fevers, generally, are all the products of organic, and especially vegetable decomposition, and at the same time to disbelieve the identity of the former with the latter, and to regard them as the offspring of different causes, would seem contradictory, did we not bear in mind the *probability* that the decomposition of different vegetable material *must* give rise to products of different character, and that these, when applied to the human system, *must* produce different, though kindred, morbid effects. Many facts, indeed, lend support to this belief, and justify the conclusion *that yellow fever, properly so called, is due to the influence of ligneous decomposition,* aided in many places, though not necessarily, by peculiar characters of soil; *while remittent and intermittent fevers are*

ascribable to the decomposition of herbaceous matter, the ordinary component of marshes and palludal localities."*

But even this, favoured as it may be by the requisite meteorological conditions, cannot, Dr. La Roche confesses, explain the epidemic diffusion of the disease, and hence an epidemic constitution of the atmosphere is *assumed* to exist at such times,—but of what nature, or how produced, and in what it differs from states of the atmosphere in other epidemics, and by what influence it becomes a yellow fever state, is not explained. Now, epidemic states of the atmosphere should not be called in to explain the *origin* of any such disease as this. Certain states of atmosphere are known to favour the extension of diseases which at times become epidemic, but the peculiarity of atmosphere, after the diseases have extended sufficiently to be called epidemic, is given in such cases by the disease itself. Thus a case of small pox introduced into a previously healthy place, extends first to those near the patient, and from these to others, until a sufficient number of cases are disseminated throughout the place to vitiate its atmosphere, and to give rise to the disease in persons who had not been in contact with or near the sick. Here, although a favouring state of atmosphere might have been necessary to give great activity to the germs of the disease and to enable them to increase rapidly, yet it cannot be said that this forms an "epidemic constitution of atmosphere;" the epidemic constitution or peculiarity is given by the disease itself, and its germs are the cause of infection, whether acting through limited portions of air, as in near approach and contact, or diffused through masses of atmosphere, as in epidemics. No state of the atmosphere can render a true endemic epidemic. In certain seasons endemic diseases may be unusually prevalent, but no epidemic constitution of the atmosphere can transport them from their places of origin and cause them to infect places the most salubrious. If, therefore, yellow fever be produced

* Op. Cit., Vol. 2, P. 618. The italics in the quotation are ours, not Dr. La Roche.

by ligneous material in a state of decomposition, and if it possess, within itself, no element of reproduction, it cannot extend beyond its efficient cause, whatever may be the state of the atmosphere. It must die where it springs into life; it can extend no farther than the wind can waft it,—at most but a very limited distance. It becomes in this view a true endemic, a local disease, such as remittent and intermittent fevers. But if this be so, how comes it that yellow fever so often appears in unusual localities; how comes it that it prevailed so lately as 1856 along the shores of New York Bay? Its whole history, indeed, disproves this idea, for there is scarcely any disease more erratic than it; and this consideration alone is enough to cause us to reject the theory of its efficient cause advocated by Dr. La Roche. There may be a peculiar state of the atmosphere "which facilitates the diffusion, or increases the severity of a local cause or contagious virus," and I believe such to be the case; but I know of no state of the atmosphere which will transport a true endemic, possessing in itself no power of increase, to remote regions, and there cause it to extend. There is no such record in the whole history of remittent and intermittent fevers, which are true endemics. They have never been known to extend to the previously healthy inhabitants of barren pine-land settlements and salubrious sea-girt islands, immediately after the introduction of cases of their kind from abroad, or after the arrival of vessels from regions where they prevail. Yet this has occurred so often in the history of yellow fever that it can no longer be regarded as a mere coincidence; it can no longer admit of doubt, explain it as we may.

But to return to the point more immediately before us, viz: the origin of the efficient cause of the disease. Between the two paragraphs quoted above, and which immediately follow each other in Dr. La Roche's work, there seems to me to be a remarkable discrepancy. The first states that yellow fever frequently, we might even from the text say usually, prevails where there is no vegetation, as in ships and barren soils. Yet the next tells us that yellow fever is due to

ligneous decomposition. Now, ligneous decomposition is undoubtedly going on in the timbers of ships, but it must be a very, very slow process, while in barren spots and soils there is usually no ligneous matter at all! Surely, these are not the localities that one would select as places where ligneous decomposition is unusually active. Quite the contrary. It, therefore, seems strange that ligneous decomposition should be assigned as the efficient cause of a disease which usually prevails in localities where this is at minimum rather than in excess.

Let us, however, look a little more closely into the matter. Has there been observed any constant, or even very frequent relation between the prevalence of yellow fever and the existence of unusual ligneous decomposition. We have searched Dr. La Roche's book in vain for any such. Instances in which there have been an outbreak of fever and the simultaneous existence of this, are numerous, but nothing like a constant or even ordinary co-existence has been made out, while the disease has, in many of the instances recorded by Dr. La Roche, appeared and prevailed extensively and severely without any such coincidence being noted. In New Orleans no such coincidence is observed, for Dr. Barton connects the appearance of yellow fever with upturnings of the *original soil.* In Charleston the disease has never once first appeared in the neighborhood of the saw mills, where there is most ligneous matter in all stages of decay. I do not think it necessary to pursue this inquiry further, for the theory we are considering cannot stand for an instant before the general question, "Does yellow fever usually *first* appear, and does it prevail most extensively, in localities where there is most ligneous decomposition?" The answer to this question, among those experienced in the disease, must unhesitatingly be in the negative,—emphatically, No! The damp, low, crowded localities of cities, where the disease is usually most rife, abound most in animal effluvia. They no more, in themselves, give rise to yellow fever than they do to cholera, or to scarlatina, or to small pox. They are, from their bad hygienic state, merely the localizers of these diseases. By

furnishing numerous subjects, more or less crowded together, with a ventilation too imperfect to carry off the impurities generated there, or the effluvia which arise from the bodies and excretions of the sick and well; with an atmosphere so impure as to lower very materially the standard health of their inhabitants, it is not surprising that they should be peculiarly liable to visitations of all prevailing diseases, and that epidemics should be both more common in their outbreaks and more virulent in their nature there. But it is not the locality which gives the peculiar, the specific character to the disease—it is the foreign element, the extraneous germ, that does this.

Yellow fever although delighting in such localities, like other similar diseases, is by no means exclusively confined to them. It prevails at times on the most healthly spots in the pine barrens of Alabama and Mississippi, on the sea-girt shores of Staten Island and Long Island; on the barren, sandy Islands of Ascention and Boa-Vista, and on that dot in mid ocean, Bermuda, when its germs are carried there. It has extended to the hardy inhabitants on a rocky promontory in the Bay of Biscay—to the crews of healthy ships in open sea—to patients in the wards of hospitals—to nurses, to friends and visitors of the sick. Facts such as these seem entirely forgotten by many of those who deny absolutely that Yellow Fever may be transported from one place to another, and may, after such transportation, extend or reproduce itself. By others they are explained away, and as already remarked, some local cause, often insignificant in itself, and whose existence for years had not been noticed from its very harmlessness, is made to bear all the blame and is triumphantly pointed at to account for the origin of the most fearful disease which afflicts our race. Others again crush them under a weight of testimony on the other side, dismiss them with a sneer at the incapacity or credulity of the observer, and, I regret to add, even question the veracity of most honorable and intelligent men of our common profession, when the fact is too strong to be denied. This is not the mode in which truth is to be reached. The facts must have their

due weight, and are entitled to calm consideration and impartial examination. The reported coincidence between importation and outbreak or extension must be allowed its proper significance. The spread of the disease after the introduction of cases, in localities remote from its usual seats, must show it to possess an inherent power of self propagation, and if this has occurred sufficiently often to make it clear that no mistake has been committed, it must be admitted,—it cannot either be ignored or crushed out—it matters not how many cases of a negative nature are shown on the other side.

Again in the cases of the occurrence of Yellow Fever on ship-board, on which so much stress has been laid, if all the cases of outbreak which Dr. La Roche has collected with so much care and with such commendable industry, be carefully examined, it will be found, with very few exceptions, that the vessels *had either recently come from the West Indies or Coast of Africa, the Atlantic Coast of the United States, the Carribean Sea or the Gulf of Mexico*, or were cruising in those waters. Not one case of outbreak is recorded on the coast of Brazil,* not one in the Levant, none in the waters of the Indian Ocean, the China Seas, or the Pacific. Now how happens it, if the decomposition of chips and shaving, or filth in the holds of ships, or the decay of the timbers of ships, form the efficient cause of yellow fever, that this does not act except in those seas and on those shores where yellow fever had previously prevailed. How happens it that yellow fever is not developed, as well in the Bay of Bengal, all the so called cases being in existence there, as in the Carribean Sea? It is simply because something more than ligneous decomposition and foul holds is required to produce yellow fever. The filthy state of the ship, to my mind, no more gives rise to the peculiar disease in question, than the crowded, filthy parts of towns—they are mere localizers of all forms of zymotic disease.

I have said above that the vessels in which yellow fever declared itself, were from yellow fever regions, in all the

* I here speak of the record in Dr. LaRoche's Book—or up to about 1852.

instances recorded by Dr. La Roche, with few exceptions. Yet even in these exceptional cases, the vessels were in the seas already indicated as those in which the fever prevails. In four of them* the vessels were approaching the West Indies when the disease appeared. In another, the Columbia, the disease broke out during a voyage from Rhode Island, U. S., to Marseilles in 1802—on this we may remark, that the fever had existed at Newport, R. I., and in New Bedford the year previous† and prevailed in this year in Boston,‡ whence possibly, and I only suggest the possibility, some of the seamen might have come with closed chests of clothes, &c. In another, the Hibbert, the fever broke out in New York harbour in July, 1803. The ship had came last from Portsmouth, England, but had been employed previously as a transport for troops from the West Indies to Nova Scotia and to England. Finally yellow fever appeared in a French transport flotilla, during a voyage across the Atlantic, from the coast of Spain, in 1802. Fever had prevailed in Carthagena, from which the Flotilla finally sailed, the year previous, and vessels were employed as transports taken from the harbour of Carthagena. Here again the same strong, significant fact looms up, viz: that the vessels were in yellow fever seas, or had been in ports where the disease had already and recently prevailed.

As throwing further light on this point, I would call attention to what occurs on the British West India Steamers almost every voyage they make. These Steamers sail from Portsmouth, England, and take in coal at St. Thomas's, W. I., the latter a very unhealthy spot and very much scourged with yellow fever. On the out voyage no one ever dreams of the occurrence of fever, but on the home voyage they scarce ever escape, deaths to the number of twenty or thirty among passengers and crew have more than once occurred, and cases have continued to appear, up to the arrival in England. In the case of the La Plata,† in November, 1852,

* Gen. Green—1799—Souverain—Fabricius 1818.

† London Lancet, February, March, May and June, 1853.

not only did the cases continue during the whole voyage, but one occurred after her arrival at Southampton in a person from the shore. Here it surely could not have been the foul state of the ships, which invariably produced the disease in going in one direction, and was efficient in their outward voyages. Common sense will every where say that the disease was contracted in St. Thomas,* and afterwards extended among those on board, let refining reasoners call the mode of extension, contagion or infection, as may suit their fancy. The visit to the sickly place was the cause of the disease. More recently the Brilliant, man-of-war,† on her *return voyage* from the West Indies lost one hundred and thirty men in her transit, and of these fourteen died in British waters, viz: in going round from Portsmouth to Chatham. At the moment even that I write (April, 1858,) the United States Steamship, Susquehanna, from Central America, where she has been crusing for six months, is lying pest stricken in New York Bay, having had one hundred and fifty-five cases in all, up to the time of her arrival, April 15th 1858, since the 19th of March. Were the outbreaks in all these cases due to the decay of the ligneous portion of vegetable material? No, no; something more must be required, the cause must have some element of self-perpetuation that commits such ravages and is so persistant. Let me again call attention to the case of Rio Janeiro. I have already quoted the remark of Dr. George Lee of Rio, relative to the recent increased sickness of the place and the frequent outbreaks of epidemics, in spite of increased attention to hygienic

* "Yellow fever has appeared with great severity, in at least three Steamers of the West India Mail Company. The Island of St. Thomas's appears to be the chief seat of the disease. The Oronoco left St. Thomas's on 27th June, with a crew of 106, of whom 75 were attacked with fever, and 28 died before her arrival in England. ******* St. Thomas's is the great central coaling depot of the Company's vessels; and every one, at least within the tropics, knows that there is not one of the Company's vessels which has coaled there but has been attacked with the disease, and *also that every vessel that has not gone there, is still free from it.*" Extract from a letter of the chaplain of the Oronoco to "Times,"—in Lond. Med. Times and Gaz., July, 1857, p. 91.

† Lond. Med. Times and Gaz., Nov. 21st, 1857.

measures, improved cleanliness, &c.; and I have also quoted his opinion that we must look beyond the local condition of the city for the origin of Cholera and yellow fever. In the concluding remark, I fully concur. Rio had been settled for three hundred years before yellow fever appeared there, and it was only so recently as 1850, that the disease showed itself there. As is usual there are some who look to the local state of the town to explain its origin. But we have the assurance of Dr. Lee, that although a state of complete cleanliness has not been obtained, the city had been decidedly improved in this respect before the recent outbreak. He, therefore, with many others, looks beyond the local condition of the city for the origin of the disease, which has been attributed to importation. I will not attempt to discuss the question of its origin here; I will only remark that it cannot in my opinion, be attributed to the filthy state of the town, for this had never produced it in the three hundred years preceding, and it had been improved, instead of having deteriorated within late years, when the disease has appeared there. Having once been introduced, it has now become fixed there. Year after year, the crews of ships resorting to its magnificent harbour are seized with the disease and Rio has already become, as Havanna has long been, a focus of yellow fever. As one of the evidences of this, we have had during the past year an outbreak of the disease in Lisbon, believed to have been imported by the Tamar,* from Brazil, by which upwards of three thousand persons were carried off in two months, (to Nov. 15th, 1858,) the employees of the custom house, who opened some very dirty baggage from that vessel, having all died of the disease. That the vessel was infected, is shown by two of the passengers having died during the voyage, which was an unusually short one.

Let me not be misunderstood. In what I have written so far, I have aimed to show, from a general view of the subject, that local causes are altogether inadequate to account for the very great diffusion of yellow fever, and that none of those

* Lond. Med. Times and Gaz., Oct. 31st, 1857 and December 26th, 1857.

indicated by authors, go even a small way to explain its occurrence in unusual localities. I have also from the same general survey deduced the belief which I entertain, that it must possess powers of self multiplication.

In support of this position a large array of acknowledged facts have been already collected, and to this mass the experience of every day brings fresh accessions. In some instances however,—so varied and complex, are the conditions affecting the suject—it is impossible to analyse, or even detect the obscure sources of origin, or satisfactorily trace the subsequent diffusion of yellow fever in a certain locality. Moreover examples are continually occuring, which, though they afford valuable corroborative and supplementary testimony, are readily thrust aside by those who, chosing to consider them as isolated and independent, apply to each case a method of special pleading, by which, as is well known, the strongest evidence may be shaken. These casuist ignore entirely the presumptive-force of deductions, drawn from a large number of related phenomena, and withhold their assent from inferences whose validity is assured by the impossibility of accounting for the known result on any other than the assumed hypothesis. Such reasoners, it is evident, are not prepared either to examine or argue a question of probabilities, though as jurists are aware, these are by no means the most perplexing or uncertain. The object of the following quotations, therefore, is to bring together a number of indisputable cases of importation and extension, sufficiently strong to silence carpers at least, though they may fail to convince opponents.

H. M. S. Bann, left Sierra Leone on 27th March, 1823, with fever on board of a malignant type. The arrival at Ascension on the 25th of April, by which time the fever had attacked almost the entire ship's company, only sixteen escaping out of a complement of one hundred and fourteen. The Garrison at Ascension consisted at this time of thirty-five officers and men, and several women and children, all in good health. The sick of the Bann were sent on shore, subject to no restrictions. On the 28th April, one of the

garrison was attacked with fever resembling that which had scourged the Bann, he was convalescent on the 7th May, and recovered. On the 11th May, a boy was attacked and died, soon afterwards six men, two women and seven children sickened in rapid succession. At this time the fever had nearly ceased on board the Bann, after causing a mortality of thirty-four out of ninety-eight. It continued to spread, however, through the Island, and attacked twenty-eight officers and men, of whom 15 died; six women, of whom four died; two boys who both died, and seven children who all recovered. While the Bann was suffering from fever, H. M. S. Driver arrived from the African Coast, with her crew in perfect health. Four of her men visited the Bann, three of whom, were immediately prostrated with fever; two of these were sent on shore, and one died on board ship. The Driver put instantly out to sea, and the disease ceased here. An example of contagion so palpable as this, scarcely needs a word of comment. If a typical case had been selected for purposes of trial it could not have more perfectly included all the conditions of irresistible proof than this accidental occurrence. An Island in mid-ocean, sparsely inhabited, containing a Garrison, all of whose inmates are in perfect health, is visited by an infected ship, whose crew are sick and dying of a malignant fever; some, if not all of these are sent on shore, and are brought into constant contact with the healthy garrison; one of the latter is attacked with the same fever, three days after the arrival of the vessel, and the disease gradually infects the whole garrison. Nay, more, to disprove any alledged *coincidence* merely, a healthy ship from a distant quarter, furls her sails in this harbour during the height of the epidemic, and of the four men who visit the plague-stricken Bann, three are instantaneously seized with the fever then raging on the latter. These are at once sent on shore, the vessel withdraws from the source of infection, and no other case subsequently occurs among her crew.

On the 16th May, 1795, the Hussar frigate captured the French ship Raison. The Raison had been employed at

Gaudeloupe as a prison ship, and when captured her crew were suffering from yellow fever. In shifting the prisoners great care was taken to receive on board the Hussar only those in perfect health; notwithstanding this precaution, the same fever soon after broke out among the crew of the Hussar. The immense value of this evidence, if correct, is at once apparent, because there are no modifying or doubtful circumstances. The disease was conveyed, far out at sea, from one ship to another; the hold of the Hussar was clean, and her general hygiene excellent, as the previous health of her crew prove, and the only rational mode, therefore, of accounting for the outbreak of fever on the latter, is on the hypothesis that it was introduced by the crew of the prize. Of fourteen men sent from the Hussar to navigate the Raison, nine died in twelve days.

A precisely parallel circumstance to the latter is related by Caillot. A French ship-of-war, infected with yellow fever, captured a merchantman, and sent some of her crew on board to navigate her. Soon after the French sailors were transferred yellow fever broke out in the prize, and nearly all the crew perished of it. Without multiplying quotations we may refer, in support of the above, to the case of the Scout, sloop-of-war, admitted by Sir William Burnett, as proving the contagious nature of the fever; and to that of La Pique and the Majestic, recorded by Gillespie, in the British and Foreign Medico-Chirurgical Review, Vol. I., 1848.

In proof of the importability of yellow fever from infected cities, and its extension in previously healthy pine-barren settlements, we offer the following *resumé* of Dr. J. C. Nott's cases, recorded in the New Orleans Report on the Epidemic of 1853:

Spring Hill is a part of a sandy, pine hill region, west of Mobile; one hundred and fifty feet above tide water, and six miles distant from the wharves of the city; it has been a summer retreat for many years; is watered by excellent springs, and had always been considered exempt from yellow fever, or any form of malarious disease. The settlement

covers about three-fourths of a mile square, with the virgin pine forest still standing, and contains about thirty families, in addition to St. Joseph College, where there are about two hundred resident pupils. On the 12th August, just about the time the yellow fever began to assume the epidemic form in Mobile, and one month after the first imported case, Dr. Nott was called to see a young gentleman, Mr. Alfred Murray, with a well-marked attack of the disease, at a boarding house in Mobile, on St. Louis street, near St. Joseph, and on the 14th had him removed to the house of his father-in-law, Mr. Wheeler, on Spring Hill, about the centre of the settlement. He recovered, and twenty days after he entered the house, 5th September, two of Mr. Wheeler's children were attacked with the epidemic, and about two weeks after two other children were attacked; three had the black vomit, and two died. Mr. Greer moved, with his family, to the house of Mr. John B. Toulmin on the 29th of August, from town, carrying a daughter convalescent from yellow fever. Another daughter sickened on the 8th; three of Mrs. Flemming's children in the same house on the 10th; Mrs. John Greer on the next day; Mrs. Flemming on the 15th, and John H. Greer two or three days after. This house is about three hundred yards northwest from Mr. Wheeler's. Col. Deas, Dr. Nott's father-in-law, lives on a lot about one hundred yards north of the last named house, and his household, white and black, consisted of sixty persons. On the 7th Septempber one of his negro women was attacked, on an adjoining lot; on the 8th his daughter-in-law, Mrs. John Deas, and on the 9th Mrs. Brown, his daughter; each being in a different enclosure, and one hundred yards from each other. The disease then spread rapidly through the families of the three adjoining premises, attacking white and blacks indiscriminately. Fifty-four were attacked out of the sixty, and in fourteen days the whole tale was told. Five whites, two mulattoes and one black were dead with black vomit, and the rest were convalescent; one-half the whites attacked died. This occurred among a class of people possessed of

every comfort, living in well kept and well ventilated houses, on a high, barren sand hill, nearly six miles from the city.

The disease afterwards spread throughout the entire settlement, though the mode of diffusion and immediate connection was, of course, not then readily apparent or easily traced. The great majority of the subjects on Spring Hill had no communication with the city for many weeks, and it is worthy of note that the disease had attacked most of the country between the hill and town before it reached the hill; though some neighborhoods, as the Nunnery and around it, escaped.

Citronelle is the name of a village of rapid growth, thirty-three miles from Mobile, at the present terminus of the Mobile and Ohio Rail Road. It is situated on a beautiful plateau of pine-land, about four hundred feet above tide water, and has been considered, like all the pine hills at the South, perfectly healthy.

The local population of Citronelle is two hundred and fifty, estimating the boarders at the hotels and different public houses at one hundred, the population may be set down as about three hundred and fifty. This does not include over one hundred negro hands in the immediate vicinity of Citronelle.

A promising and estimable young physician, (Dr. James S. Gaines,) who unhappily fell a victim to the disease himself, treated from the 16th August to the 4th of October, fifty-three cases of yellow fever; thirteen out of this number died. The first case, which he was satisfied originated there, occurred on the 11th September, from which time it continued steadily to increase. Some of the cases being of a very malignant type, Dr. Gaines, in a letter published in the Mobile Advertiser, 6th of October, 1853, says: "I have no idea that the disease could have originated here, had it not been for the frequent communication between this point and Mobile; and it is not singular that it should have done so, when we reflect that the baggage-cars are almost air-tight when closed, running from Mobile to this point in two hours." According to Dr. Gaines' statement, there was just a month's

interval between the first case imported into Mobile and the first at Citronelle.

The Dog River Cotton Factory is situated southwest of Mobile, about five miles, and has within its enclosure of some twenty or thirty acres, about three hundred operatives, including their families. The houses are built in a hollow square, and form a complete village. Mr. Charles Whattleworth, one of the most efficient and intelligent officers of the establishment, under date of November 22st, in reply to certain queries, says:

"The first case we had here was a man that had been to New Orleans; he was taken ill on the 8th August, two days after his return, and died on 22d; a man that waited on him died about the same time. The next cases that occurred were about the 1st September; they were six in number, and the parties had been in the habit of going frequently to town. The first cases that appeared here among persons who had no connection with the city, or with the sick, occurred on the 9th October; there were five cases on the evening of that day, and about the 13th there were five cases more. Others broke out from this date."

In the seventh volume of the Transactions of the American Medical Association, Dr. Fenner has made an excellent report upon the epidemic above spoken of, and its importation into New Orleans from without is clearly apparent, from the statements contained in this record.

On the 17th of May ship Augusta arrived from Bremen direct, with about two hundred and thirty emigrants, out fifty-two days, and took position at the foot of Josephine street. The Augusta was towed from the mouth of the river by the same boat that brought up the Camboden Castle, a British vessel, direct from Kingston, Jamaica. During the passage up the river there was free communication between the two ships, across the tow boat. The Camboden Castle had been in port at Kingston, Jamaica, six or eight weeks; there was much yellow fever among the shipping at that time, and while there she lost seven men with fever. On the 2d of May she sailed for New Orleans, with

no fever on board after she left Kingston, and took position about a half mile from the Augusta, nearly opposite Robin street.

May 27th, James McGuigan, sick four days, entered Dr. Fenner's wards, Charity Hospital; he died on the 28th, with yellow fever. McGuigan was from Liverpool, had been only a week in the city, and was taken sick the same day that the first case occurred on the ship Augusta, the two vessels being not more than a hundred yards apart.

John Allen, Scotchman, was admitted on 4th June, into Charity Hospital, with yellow fever; recovered. He lived in the city, but had been for some days previous to his illness employed in loading the ship Harvest Queen, which lay very near the Camboden Castle.

Michael Mahony, Irish laborer, recently from Liverpool in ship Saxon, entered Hospital on 6th June, with yellow fever, and died on 7th. Mahony slept on board the Saxon every night since his arrival, very near the Camboden Castle.

The first case of yellow fever which occurred at Algiers (across the river from New Orleans,) was that of an Irish laborer, (Anthony Howe,) who, for fifteen days prior to his attack, had been engaged in unloading the ship James Titcomb, from Liverpool. On the 6th June, Howe crossed over to Jefferson City and walked through the district, where the first cases of fever had occurred; on the 17th he was attacked. Shortly after his wife and child had the disease. On the 26th June, a Swede living in the next house to Howe died of the fever. Very soon afterwards a Mrs. Nelson, living in the next house, died, and three of her children were seized with fever. From that point the disease spread through the city.

The first case of fever at Woodville, in 1852, occurred at the factory, a mile south of the town—one of the laborers (Lee,) left the Factory late in July, and visited New Orleans, he returned to the factory on the 9th August, and died a few days after, with black vomit on the 25th August; the next cases occurred on the 28th, 29th and 30th, several others followed, and during the first week in September, the disease

became epidemic in the factory, nearly all of those taken had been in the room and about Lee, during his illness. On the 26th August, a boy belonging to Dr. Henderson was attacked; on 31st Dr. H. sickened, and in ten days his whole household, nine in number, passed through attacks. About the same time Major Feltus was taken ill; as soon as he could be removed, he was carried into the country with his family; one of his children had an attack soon after. In the cases of Major F. and Dr. H's. boy, there had been no personal communication with the factory, but Dr. H. had been attending some of the earlier cases there, and the residences of both of these gentlemen, were on the southern boundary of Woodville, and nearest to the factory.

Early in September, 1853, William McDermott, arrived at Mrs. Smith's house, from Arkansas, having landed at Vicksburg where fever was prevailing, and remained one night. From thence he went to Bayou Sara in a boat on which there had been deaths from fever. He spent a night in Bayou Sara, fever prevailing there also, and the next day reached Mrs. Smith's house, ten miles in the country, with fever on him. He died five days after, throwing up a large quantity of black vomit. In a few days, Miss W., an inmate of the family, was taken; about the third day of her illness, Mrs. McCausland, a visitor at the house, went frequently into Miss W's. room. Several members of Mrs. Smith's family afterwards took the disease. Mrs. McClausland resided nine miles from Woodville, she had been in Miss W's. room on the 20th September, and on the 26th she became ill and died, throwing up black vomit. Twenty days after the death of Mrs. McC., her daughter contracted the fever; during her illness three servants who had aided in nursing her mother were successively prostrated, and a few days later, three other members of her family shared the common fate which had befallen the others.

Fever appeared again in Woodville, in September, 1855. John Blacker from Cincinnati, reached Woodville on 15th August, having spent a few days on his way down at Vicksburg, at which place yellow fever was prevailing. As Wood-

ville was under quarantine regulations, he was sent on the 27th to Hastings House, (a mile and a half southwest of Woodville,) where he died on 2nd September, of fever. On 28th August, Mrs. Blacker and daughter, Mrs. Craige and children, and Mrs. Brown, all residents of Woodville, and friends and relatives of Blacker, visited him at Hastings. On the 5th September, Mr. Craige was taken down, on the 6th Miss Woodbridge and a woman of Bengoner's who died on the 14th with black vomit, on the 7th Wm. Beach, living in the same house with Miss W., on the 8th Mrs. Blacker and her daughter, on the 9th two of Mrs. Craige's children, on the 11th a negro boy at Mr. Butterworth's, on the 13th Mrs. Brown, on the 18th Henry Blacker, on the 21st fever was epidemic. Blacker and Craige reside in the northwestern portion of the village, Butterworth in the southwestern, about a half mile apart. Mr. Beach's and Bengoner's houses are on a line between the two, and one-third the distance from Blacker's to Butterworth's. Hastings is a half mile beyond Butterworth's, on the same road.

From Dr. Fenner's report of the Epidemic of 1855, contained in the 9th volume of the transactions, we learn that the same year in which the fever appeared in Woodville; Mr. Wall was taken sick on the 26th of August, on board the steamer Princess, returning from Cowper's Well; he was carried to the plantation of Mr. F. A. Evans, fourteen miles from the river, where he had a serious attack of yellow fever. Acclimated nurses were procured to attend upon him, and in the two weeks succeeding Mr. Wall's recovery, fourteen negroes, constituting all Mr. E's. house servant's and stable boy's were attacked. Three weeks later the disease appeared in the negro quarter, a half mile from the residence. Mr. Evans' plantation is a healthy locality, in a region never before visited by yellow fever. A few weeks after Mr. Wall's recovery, he visited the plantation of Mr. Williams, near Fort Adams, accompanied by Mrs. Wall, and her servant who had recently recovered from yellow fever; six days after their arrival, the woman in whose room the servant of Mrs. W. had been lodged, was taken ill. Mr. Williams left the place

and went to Dr. Sheppard's in Pinckneyville, became unwell the next day and had a severe attack of yellow fever. Before he recovered, two servants of Dr. Sheppard's were attacked; Dr. S. and his wife had the fever in 1853. Mr. Wall immediately separated the negroes on the plantation, removing five or six who were known to have been in the room with the first case, and by keeping them apart from the others, the disease was arrested. The Barque Tivoli, sailed from New Orleans towards the close of August, 1854, and reached Last Island, where on account of sickness on board, she anchored, and remained until September 5th. In the mean while, two men died and whilst the Captain was sick, on the 5th September, the Tivoli was towed in from Last Island, by Judge Baker's steam boat Picayune to his plantation. She was there overhauled and cleaned within a hundred yards of half a dozen negro cabins. The hands of the Picayune, (negroes and mulatoes) were the first attacked with fever. Those who had been on board the Barque, and cleansed and smoked her hold were taken sick within ten days after; they all had fever. The negroes in the cabins near the Tivoli were next attacked by the disease, from these it spread to the remaining negroes. On the 17th October, Mr. Anthony Baker and his consin were taken down and both died with black vomit.

M. Boudin, in the second volume of his erudite and valuable work on Medical Geography and Statistics, offers the following personal testimony and opinion, in relation to the importation and transmission of yellow fever in the West Indies.

"Observation has proved that the yellow fever which prevailed at Martinique, in 1851, and at Guadeloupe, in 1852, was the result of importation through infected vessels. The invasion of Martinique, which was in September, was generally attributable to a spontaneous generation. I alone perhaps, was struck with the fact, that the first cases occurred soon after the arrival of certain vessels at Port de France, which came from Cayenne, and San Domingo, where the disease then prevailed. As an epidemic it was slowly extend-

ed over the Island, for it was long confined to the port at which the vessels had arrived, and did not reach Saint-Pierre, where I was, until July, 1852, after two introductions; the first in November, 1851, at the Seminary and the Bishop's residence, the inhabitant's of which, had frequent intercourse with Port de France, and the other at the hospital, in 1852, where the Sibylle sent her sick sailors, after her arrival from infected port. These facts leave no doubt on my mind relative to the importation, but they are not sufficiently established by adequate proof.

The second invasion of the same colony occurred in September, 1855, under the following circumstances. The corvette Recherche, from Brest, arrived at Cayenne, on the 24th of August, where she remained for eighteen days. Her crew and passengers communicated freely with the infected town through the infected steam boat Gardien. Leaving Cayenne on the 12th September, she anchored at Port de France on the 18th, which had been healthy since the begining of 1853. Three days of quarantine was imposed upon her in consequence of coming from an infected port. On the 22nd, one day after her liberation, a lieutenant of marines was taken sick, on the 23rd he was sent to the hospital and died of yellow fever on the 25th. From the 23rd to the 30th, six more men were sent to the hospital with the same disease. The Recherche sailed for Europe on the 30th, and carried with her the germs of disease, which did not cease their action, until she reached the Banks of Newfoundland. Let us now return to Martinique and record what occurred. An Artillerist, a passenger in the Recherche, landed on the 22nd and took up his lodging in the barracks with his company, and on the 24th he was carried to the hospital with yellow fever. The disease did not immediately extend, but on the 3rd of October, it broke out with such violence in the barracks that from the 3rd to the 9th, seventeen soldiers were attacked. The barracks were evacuated and a part of the company went to fort Desaix, where they carried the disease and communicated it to the 18th company of marine infantry, which inhabited the ground floor of the same barracks. Other portions of

the same company went to Marin and to Trinity where they also carried the disease, and infected the companies at these ports. Afterwards it extended throughout the whole colony. It is obvious that if the importation of the first epidemic was not sufficiently established, the importation of the second requires no additional proof.

At Guadeloupe the epidemic of yellow fever, appeared in 1852, and in the following manner. In July the Gaston and Genie arrived from Martinique, lay in the road and sent patients to the hospital of Pointe a Pitre. In August, the frigate Armide arrived at the Port de France, Martinique, where she remained only sixty-six hours, and sent a boat ashore on the 6th, where it passed the day in obtaining provisions. On the 7th she left for Pointe a Pitre, Guadaloupe. On the 9th, one of the boatmen, who had gone ashore at Port de France, exhibited the symptoms of yellow fever, and was sent to the hospital, where he died on the 13th. After discharging her passengers, the Armide sailed for France, on the 14th, and on the 18th, the first case of epidemic yellow fever was observed among the crew, which continued for twenty-eight days, attacking one hundred and twenty-two men, out of a crew of one hundred and sixty, of which fifty-four died. In July a few cases were reported at Pointe a Pitre, and after the departure of the Armide, other cases appeared both at the hospital and barracks, and shortly after the epidemic became general. At Basse Terre, the first case was a gendarme, who arrived in the Armide soon after the gendermerie barracks, where this man resided, was infected. From thence the disease reached the houses of the Champ d'Arband, where the Europeans resided, and soon after, it extended to the barracks of the infantry and artillery. The progress of this epidemic was easy to prove, and the importation by the gendarme, from the Armide, was never questioned. This was the commencement of the epidemic of 1853, which subsequently extended throughout the colony.

The frigate Sphigenie, left Port de France in December, 1855, with the germs of the epidemic on board, which were not dissipated by an anchorage of one month, at Saintes,

where she landed the greater portion of her crew, and sent her sick to the hospital. On the 1st of January, 1856, she came to Basse Terre, lay in the harbor, and sent five yellow fever cases to the hospital, three of whom died, other patients were placed in the surgical wards. The epidemic continuing on board of the frigate, she ordered her sick to be encamped in the field and towards the end of the month, she left Basse Terre without being relieved. During the same month, the Chimere which accompanied the Sphigenie, had some cases, two of which were sent to the hospital. The frigate Eregone coming from the Port de France, in February, also sent an artillerist, taken in at that port, who died at the hospital at Basse Terre. The yellow fever had ceased at Basse Terre, from the 4th July, 1855, and throughout the colony for more than a year. Towards the end of March a manifest epidemic influence was revived, and localized at Basse Terre, where these several imported vessels had arrived, proceeding from the hospital, where the patients had been treated. This was not an epidemic constitution, which was generalized, but the limited influence of importation.

The transmission of the disease from a sick man to a sound one, who approaches him, has often been verified. Thus, in 1854, the frigate Armide, leaving Gaudeloupe for France, carried certain artillerists, who had served their time in the colonies; among these there was one who concealed his disease for fear of being left at the hospital. On entering the ship he went to bed, and on the third day after he died. Four days after, four other men, who slept near him, were attacked with yellow fever successively, but were cured. This ended the limited extension of the disease. During the same year, at Point-a-Pitre, as at Basse-Terre, the following circumstance was observed. An orderly, lodging with an officer, fell sick and died at the hospital. His place was supplied by another, who occupied the same room, and used the same bed; he also was sent to the hospital; and also a third, who supplied his place, was likewise attacked with yellow fever. Thus the three were sacrificed under the same circumstances, and then the office was

abolished. In the month of December, when the epidemic was ceasing, a soldier of the infantry, attacked with yellow fever, was placed in a ward where there existed no other case of the disease; he died in three days, having black vomit. Two days after, three artillerists, one of whom was near, were attacked, and all died. In June, 1856, the artillery barracks being invaded by the epidemic, imported into Basse-Terre by the infected vessels, all the soldiers who had never had the disease were sent to Camp Jacob in hopes of preservation. Three of these soldiers, tainted by the infection previous to departure, entered the hospital of the camp, and there died. The sister who nursed them, who had been in the colony only five months, fell sick five days after the last death, and presented the symptoms of the severest kind of yellow fever. No case had previously existed before the arrival of the artillerists at Camp Jacob, nor did any case follow the illness of the sister.

In 1833, the brig Meleagre arrived in the West Indies and visited successively Martinique, Cuba, Vera Cruz, New Orleans and Pensacola, remaining at each port but a short period. She suffered neither from cholera nor from yellow fever, which prevailed at several of these places. But on a second visit to Havana, a cabin boy, who had been left in the hospital for two months, returned on board, and was attacked with yellow fever. Notwithstanding her departure for Vera Cruz, in fifteen days the epidemic was declared, and continued to prevail. During five weeks twenty-three cases and five deaths occurred, and of these seven were officers. M. Delaport explains this enormous disproportion by the location of the officers near the store rooms.

The first person attacked with yellow fever, at Marie Galante in 1853, says Mr. Brette, was M. C., a sub-lieutenant of the 23d Company of the 1st Regiment of Marine Infantry. This officer had gone to pass some days, about the end of December, 1852, at Point-a-Pitre, where the fever prevailed, and where he contracted the disease at the moment of his return to Marie Galante. The second case was observed a few days after, in one employed about the custom house, who

was carried to the hospital, where he threw up black vomit. This man frequently visited the barracks, and was intimate with M. C., who had already fallen sick. Both of them died. These two cases having occurred, the disease extended in the barracks and in the hospital, in one of the wards of which it fixed its position, for every soldier who entered the hospital, either with a simple wound, intermittent fever, or any other disease, contracted yellow fever. It was then decided to evacuate this ward for some time, and remove the patients to another house, hired by the administration. The fever continued, it is true, but the cases were less violent and less numerous. The precedence of the first case, and the relation which it bore to those following, establishes the fact of importation into Marie Galante, and its transmission from centres of infection developed around the first cases of the disease."

A writer in the Annales D'Hygiene Publique, p. 25–27, remarks that "paludal fevers are the most numerous and severe endemic diseases of Guana, even during the prevalence of epidemic yellow fever. They form three-fourths of the whole number of cases which occur in ordinary years.

Yellow fever does not appear to be a disease of the climate of Guana. It has made but few visitations. We had forgotten the epidemic described by Camper, and that at the commencemement of the present century, when it made its appearance at the end of 1850, one year before it was made the recipient of French criminals, which, consequently, could not have been the determining cause; but surely it will be the perpetual aliment to support it, as it has done since the appearance of the fever, in 1855."

In the same work, p. 206, we find it stated that during the summer of 1842 two passengers, coming from New Orleans, where the fever prevailed, arrived at Panama, where they embarked for Guayaquil, in the ship Reyna Victoria. On the day of embarkation they sickened, and died eight days after, of yellow fever. Many other passengers, of whom some died, contracted the same disease. After the arrival of the vessel at Guayaquil, in August, when

the most perfect health prevailed, some repairs were necessary, and several carpenters were employed on board, some of whom contracted the yellow fever, and communicated it to their families. From these it spread throughout the city, and continued to prevail until the end of 1845.

According to the same author, the yellow fever was imported into Lima, in January, 1852, by sixteen German passengers, who came from Rio Janeiro, where the disease prevailed. This epidemic disappeared under the influence of winter, (July) and did not re-appear until March, 1853. This year, also, the yellow fever disappeared on the advent of cold weather, (July) and did not re-appear until the summer of 1845. From this period it spread over a great part of the coast of Peru. In 1856 it reached Valparaiso, and also Sant Yago. (See Smith on the climate of Peru, Brit. Rev., 1856.)

At Bahia the yellow fever was imported in November, 1849, by the American brig Brazil, coming from New Orleans, and touching at Havana. On 18th of December following, the disease appeared at Rio Janeiro, where the first victims belonged to the crew of the bark Navarre, which left Bahia on the 22d November.

The Times correspondent, writing October 11th, 1857, says, in speaking of the interesting epidemic, which ravaged Lisbon that year, "Little or no amelioration can yet be noted in the course of the fever. Business continues almost paralysed, and the transactions in the shops, as well as in the counting houses of the merchants are almost a nullity. The lower part of the town, which is decidedly the business quarter, is almost deserted, and very many of the shops are closed. Every second person you meet converses about death, cases, symptoms or remedies. If this state of things continues, there may be a probability of a crisis in the commercial community. The exchange and the custom house are nearly deserted, and the traffic with the provinces has almost ceased, people being afraid to come to town. Among other things I hear on good authority that the guards round the custom house are doubled, and the lower quarter of the

city is thoroughly guarded—there being some fear that the mob who look upon it as the focus of infection may attempt to burn it down. Besides this, I am told the public are highly dissatisfied with the want of energy displayed by the government in meeting this emergency; and the authorities fear an outbreak. In the deserted streets, I believe robberies have taken place, the thieves not scrupling to enter houses where the fever has raged. A strong belief exists that this fever was imported in the Tamar, which arrived here from Brazil on the 2nd ultimo, after an unprecedentedly short passage. Certain it is, some very dirty baggage brought by her was opened at the custom house, and all the *employe's* who were present, have since died of the fever. I can vouch for this. More over, the Tamar had the fever on board, for two of her passengers died. In consequence of the prevalence of fever in Lisbon, free pratique is given to vessels from ports in Brazil declared suspected; from those called infected a new regulation is adopted—the passengers may at once land, but the baggage and goods must go to the Lazaretto and the ships on being washed with some disinfecting liquid may go to the Quadro."

We might fill an indefinite number of pages with facts similar to these, having the same purport and all tending unequivocally to establish the same point; but content ourselves, in conclusion, with merely calling attention to the epidemic with which Staten Island, Fort Hamilton and the Long Island shore were visited, during the summer of 1856. Dr. Harris, the Physician of the Marine Hospital at New York, has most indusputably made out its importation to the harbour and neighboring shores, through the crews of the Hancock and Gliddon, both coming from infected ports, with yellow fever on board. The most important portions of his Report, presented to the Legislature have been transcribed in a notice of the pamphlet published in the September number, 1857, of this Journal, to which we may refer those who have not access to the original paper.

It is interesting however, to remark, what has often before been observed, that while this disease is an annual visitor to

the salubrious shores of Staten Island, a granite rock, swept by the pure sea breezes, cleanly, well drained and presenting in every aspect the most favorable hygienic condition, its foul and reeking neighbour, dirty, ill ventilated, overcrowded and pestilential New York, should almost invariably escape the fatal plague, and present an immunity nearly perfect from its terrible presence.

Believing then, that yellow fever is a plant of exotic growth, transported by human agencies to our shores on which it has unhappily found a too congenial soil and climate, but where it has not yet, at least, become fairly naturalized, requiring the successive introduction of new germs to bear fresh harvests, "we cannot," in the language of Prof. Dickson, "or ought not to wonder—assuming for a moment, that it is a specific disease, depending upon a specific *Materies Morbi*, capable of persistent retention of its qualities, and among them self-multiplication and self-diffusion—that its dispersed and widely scattered germs, shall under favoring circumstances be developed, and produce the special poisonous effects for which they are characteristically adapted. This must rationally be expected to occur rarely, but it is enough to show that it is not impossible. Thus, and thus only, arise sporadic and apparently spontaneous cases. No other origin of yellow fever has yet been shown to be possible."

REVIEWS.

ART. IV.—*The Transactions of the American Medical Association.* Instituted 1847. Vol. XI. Philadelphia, 1858. 8vo., pp. 1027.

In our review of the last volume of Transactions emanating from this body, we took occasion to speak of its objects, the means adopted for securing the success of these, and the good which had already been accomplished by its efforts. As we then remarked, this Association is inevitably regarded abroad as the representative of American medical character and progress, and we cannot evade the responsibility attached to this supposititious position. In estimating the value of its publications the above fact should be kept constantly in mind, and the standard of criticism must therefore be, to those who have a proper appreciation of our true character and worth as a scientific body, a very elevated one. It cannot be supposed that this can be fairly applied without the risk of appearing censorious, or of giving offence to individuals, who will necessarily regard their productions as harshly treated; but it is due to ourselves to pursue this method of examination, without fear or favor, and if properly conducted, the result must prove, in the end, a valuable and desirable one to all who are interested in our own advancement, and the respect and esteem with which we are regarded elsewhere.

At the last meeting of the Association, held in Louisville, the number of regular and special reports submitted were unusually meagre, and the whole proceedings singularly devoid of interest. The Committee on Prize Essays acted with most commendable wisdom in rejecting the papers

handed into them, if their grounds were tenable, viz: that they exhibited no originality, and did not possess the species of merit which entitle any of their authors to the Association award; but it is unfortunate, to say the least, that this should be the case, and the fact certainly exhibits a lack of interest in the body, and respect for its adjudication. Perhaps, if the same degree of caution had been earlier exercised, we should not now have to chronicle this painful decision. Throughout the career of the Association, however, and never more markedly than at their last convocation, the most striking defectiveness of organization or constitution has been displayed in the number of regular committees failing to report upon the special topics submitted to them, and asking further time. This is the more to be censured, as the persons constituting these have usually been volunteers, and have enjoyed, without compensation, whatever benefit may accrue from a publication of their names and intentions in the annual reports. We are pleased to see that the attention of the Association has been called to this matter, and trust that ere long the mode of selecting such committees will be totally changed, and appointments made of those who are not merely acquainted with the literature and progress of the special departments assigned to them for investigation, but possessed of the capacity and taste for original inquiry.

The address of Prof. Paul F. Eve, the retiring President, with which the volume proper opens, is in every way worthy of its author's reputation. The thesis he has chosen, the past performance and future promise of the Association, is well and briefly illustrated. He is of opinion,—and his estimate is supported by some of our most distinguished trans-Atlantic brethren,—that the Reports already published contain a large amount of highly valuable information, and that the ethical success and moral power of the body have far exceeded the most sanguine expectations, while the community of interest, the emulation and concert of action, which it has fostered among physicians, have been productive of the happiest results, and created a confidence of

success never so well assured as in the present day. It is true that the radical and extensive plans at first proposed have not yet been fully carried out; the work assumed will consume time and labor far greater than has yet been bestowed; but it has advanced medical knowledge by enlisting talents of a diversified and high order in its ranks, has called into being, and given publicity to much that is eminently useful, and cultivated a spirit of harmony and generous rivalry among its members which cannot fail to bring forth fruit in due season.

The address closes with an eloquent tribute to the benevolence, self-sacrifice, and lofty sense of duty which have, without exception, characterized the lives and labors of our entire profession, and an admonition of the high interests, hopes and purposes which are concentrated in the aims and action of the Association.

Of the Reports on Medical Topography and Epidemic Diseases the first in order is by W. L. Sutton, M.D., of Georgetown, Kentucky. After a brief account of the geological formation of this State, Dr. Sutton, in a digression more interesting than pertinent, describes the dangers and hardships attending the settlement of Kentucky, and follows up this with a graphic though hasty sketch of the manners, customs and diet of the early immigrants. Meteorological tables, and quantitative and qualitative analyses of mineral waters come next in order, and the reporter proceeds to a consideration of the second division of his subject,—epidemics,—only two of which are noticed as having come under observation.

The *Jerks*, or, as it has been called, *Epidemic Epilepsy*, is certainly, as the author says, the most remarkable disease which has ever visited the State, for the extent of country overrun, the number of persons affected, and the strangeness of the phenomena exhibited. This affection, called at the time "bodily exercise," attended a great religious excitement which prevailed from 1799 to 1805. An account of it has been given in a work by the Rev. Robert Davidson, entitled the *History of the Presbyterian Church of Kentucky*.

As a matter of psychological curiosity and interest it is, we think, second to none of those manifestations of disordered will and sympathetic excitement which prevailed so extensively during the middle ages, either in its history, etiology, or individual details.

It seized all classes and character of persons, respecting neither age, nor sex, nor color, nor opinion, the incredulous and scoffing as well as the "true believer," and manifested itself in the strangest and most contrasted distortions of the limbs, the wildest and most vehement movements, howls, trances and visions. In its farcical, yet melancholy exaggeration, it furnishes an impressive warning against the tendencies common among certain classes of persons, to regard these and other not less absurd disorders of an imitative class as evidences of the solemn and awful presence of the Almighty among communities and peoples.

For the account of "Milk Sickness" Dr. Sutton is partially indebted to Dr. J. H. Barbour, of Falmouth, Ky., who furnishes an interesting history of the epidemic which prevailed at Falmouth in 1854 and '56. Through the writings of the ever-to-be-lamented Drake most of our readers are familiar with the symptoms and progress of this disease. The patient first complains of pain in the limbs and head. These may be followed by a slight chill and fever. There is loss of appetite, a sense of oppression at the stomach, succeeded by nausea and vomiting, with burning at the epigastrium, and intense thirst; fluids drunk are usually rejected; the tongue is coated with a heavy fur, is sometimes red at the tip and edges, and becomes black or brown in the progress of the disease; the pulse is small, and may be depressed or increased in frequency; respiration is hurried and sighing. The drowsiness of the first stage may pass into profound coma, the pupils dilated. In all cases there is restlessness independent of the stupor. The face is red or dusky, and bloated; the trunk warm; the extremities cold; at the same time the patient complains of burning.

When the case proceeds to a fatal termination, the irritability of stomach becomes intense, the least motion or con-

versation bringing on vomiting, which sometimes resembles the black vomit of yellow fever; the constipation continues; there is no peritoneal soreness; the sighing is deeper, and hiccough supervenes; the pulse sinks in frequency and force; the heart is convulsively agitated; the patient is restless, though comatose; there is seldom any delirium, and death occurs from this gradual prostration.

Dr. Sutton adds little to our knowledge of the pathology or therapeutics of this disease. He regards it as malarial in origin, and discusses the arguments of Drs. Drake, Seaton and Owen, who believe it to be caused by the animals affected eating the Rhus toxicodendron, the vegetable productions of soils abounding in astringent salts, and by constipation, respectively. He further is of opinion that there is no relation of cause and effect between the "trembles" in animals and "milk sickness" in man, for the following reasons: 1. Many persons do not take the disease, who eat the flesh and drink the milk of cows so affected. 2. Persons die of the disease who abstain from beef and milk. 3. Though children partake largely of milk, they are confessedly less liable to the disease than adults. 4. The period of the year when the disease prevails corresponds with that for bilious fever. 5. It appears in families in which malarious fevers prevail contemporaneously with them. 6. The symptoms are often the same. 7. As observed by Dr. Drake, in a few instances, the complaint has recurred for two or three years in the season in which it began, without any known application of the remote cause. 8. The effects of remedies.

The report concludes with a number of laboriously compiled and valuable tables, showing the population, area, and proportional number of deaths of each county, the proportional mortality from certain diseases in the lime stone regions and coal fields of Kentucky; the color, age and sex of those dying of cholera, croup, dysentery, continued fever, scarlet fever, and hooping cough, for the five years, 1852–56.

Dr. Lyndon A. Smith is the author of the second report, on New Jersey. Owing to the recent good health which this State has enjoyed, and the failure of physicians through-

out the State to respond to the author's appeals for information, the paper is very meagre, and presents few facts of either interest or value. Scarlatina, intermittent, remittent, and typhoid fevers, dysentery, measles, erysipelas and whooping cough have been epidemic in one or more sections of the State; the same remark is true of mumps, rheumatism, diarrhœa, influenza and cholera infantum, which have all visited some portions of the State, though not extensively.

Small pox had been more than usually prevalent, but the faith of the profession in the efficacy of vaccination had not been weakened. The author of the report, from a long experience, advances the following opinions with great confidence: That a *perfect vaccination* affords *perfect protection;* that some subjects require re-vaccination to destroy their susceptibility to small pox, this being necessary as long as an impression can be made; that vaccination, if done in time to pervade the system, will save the life and mitigate the distress of a patient sick with small pox; that a small pustule, which passes through the regular stages, is more perfect, usually, than one which produces great pain, inflammation and swelling; that if the virus be taken from healthy subjects, as should always be done, there is no fear of propagating other diseases; that eruptions often occur after the most careful vaccinations, which are not attributable to the virus; that a small scar, with pits round the border, is better evidence of perfectness than a large, smooth and glossy one; in vaccinating a patient with a good scar, if the virus produces redness and itching for a few days, the first vaccination was *perfect;* if it produces no other effect than that of a common puncture, another attempt should be made, with new virus.

With the above conclusions most physicians now very generally agree, and even in regard to the first, which has been the subject of warm dispute and considerable investigation, the number of converts is daily increasing.

The committee appointed in 1855 to prepare an account of the epidemics of Ohio, after three years grace, present a paper of as many pages only. It appears that no en-

treaty could induce the profession of the State to commit itself on paper; and as no communications bearing upon the subject could be procured, the committee have considered their duty performed in calling attention to the increased prevalence of intermittent fever during the summer of 1855, when "market vegetables decayed very rapidly," so that cabbages "became scarce long before the usual time of their disappearance," and to the value of sulphate of cinchonia as an anti-periodic. Most interesting information!

The Committee on Medical Literature have performed their office well, and presented a carefully prepared and useful paper upon this important topic. The critical portion of the essays is divided into two sections. The first takes up the subject of Medical Journalism in our country, and is a discriminative and sagacious discussion of its strength and defects, its weaknesses, short-comings and accomplishment. We cannot subscribe, however, to the opinion here expressed, that there are advantages in the large number and diffused localization of these journals which more than compensate for such defects in their average quality as the greater number may induce. It is true that inexperienced and modest writers are more likely to contribute to small journals in their own neighborhood, and that thus the *quantity* of our medical literature is increased; but as *quality* is more important, as the very large majority of these nurselings perish early, and the few who have something to do and say, with the capacity for both, are certain to make themselves known through better recognized channels, it is to be wished, we think, that the profession would lend a more general support to those journals whose position, influence and capabilities render them the most valuable exponents of medical opinion and progress. The country practitioner, subscribing to the little monthly published at the neighboring village or town, flatters himself that he has contributed his quota towards the support of American Medical Literature, and neglects to aid or be benefitted by the more mature, elaborate and useful work which, at longer intervals, and with nicer preparation, is

issued in a distant city. In consequence of this the latter are not adequately supported, they cannot afford to remunerate their contributors, and are, therefore, compelled to be satisfied with a standard far below that which is felt to be easily attainable under different and more encouraging auspices.

Dr. A. B. Palmer, the author of the report, has spoken very plainly and pertinently upon all the questions affecting the status, progress and promise of our periodicals; he has done this, moreover, with the tact, knowledge and authority which a long experience of editorial trials and difficulties could alone bestow, and we agree most heartily with him in his honorable estimate of the good already achieved, and the certain advancement in scholarship, originality and practical value which each year will bring forth.

The profession has every right, we think, to be proud of the gratifying improvement steadily going on in this department of scientific labor, and we trust that the day has gone by when sensible and reflecting men will indulge in the cant of disparaging our national contributions to medical knowledge and progress.

In the second division of the report, original American medical publications are noticed. Of these works, as a whole, though possessing various degrees of merit, we need not be ashamed, and considering the embarrassments under which we still labor, they are of an order of excellence and perceptible elevation of subjects and treatment which merit no little praise, and indicate a comparative advancement on past efforts sincerely to be admired. No analysis of these has been attempted,—indeed, this would not have been admissible,—but some of them have been passed over with a mere recognition, where more extended comment and eulogy would have been fitting. Dr. John Watson's discourse before the Academy of Medicine, on the Medical Profession in Ancient Times, for example, is slurred over as merely "thorough in research and correct and forcible in style," when the highest English authorities have pronounced it the

most classical, critical and original contribution offered to the History of Medicine in the last half century, by any pen.

Lastly, Dr. Palmer discusses the "measures deemed advisable for encouraging and maintaining a national literature." However extensively this theme may have been elaborated by this and preceding committees, we cannot regard it as likely to lead to very valuable results. The bulk of this division is occupied in a consideration of the benefits and justice of an international copyright. The committee have arrived at conclusions which we cannot wholly commend, but space will not permit their discussion. We have always contended and still believe that by every law of morals and justice "the laborer is worthy of his hire," and that the author has the same exclusive right to the products of his brain labor as the mechanic or merchant, who enjoy a protection which the oppressed scholar is denied.

We had wished to speak in this connection of the comparative and contrasted condition of our own and European communities and individuals in respect to the opportunities, means and facilities for original investigation, and exhibit the social and economic barriers which are opposed to our rapid progress in certain directions, as well as our increasing capacity and efforts to surmount these, but must offer in stead the following *resumè* of the Committee's views, which are, in the main, judicious, and worthy of serious consideration by the profession at large.

The number of original American Medical Works is increasing, and their character is improving, and in some respects, particularly in practical utility, they will not suffer in comparison with those of Europe; yet serious imperfections exist, and decided improvements are demanded. Great and permanent improvements in medical as in general literature, must be gradual, depending more upon the advancement of education, of taste and intelligence, than upon any specific measures which may be adopted. Still, various particular measures, such as the frequent writing of medical theses during professional pupilage, and keeping systematic records of cases when in practice, would do very much in hastening on improvements. But for the greatest

perfection of our literature we must wait the fuller development of our country, and for those changes of time and circumstances which shall produce larger numbers of devoted savans and scholars, placing them in situations where a variety of absorbing pursuits shall not prevent the concentration of great talents upon a comparatively limited range of subjects.

The report of Dr. James R. Wood, on behalf of the "Special Committee on Medical Education," possesses, we confess, little interest for us. We have always held and maintained the opinion, that the Association is utterly impotent to effect any changes in this quarter. It must be remembered, that the Schools represent, as nearly as may be, *le creme de le creme* of the profession; that the Professorial Chairs are occupied by the best talent, and sustained by the highest influence in the land. If the laity choose to array themselves in opposition to these, they must inevitably succumb in the unequal contest and the colleges could safely—backed by their prestige and power—defy the united efforts of any body, made up of so heterogeneous material as our Association. They will possess, if united, all the concentrativeness of an orderly, well organized and well equipped staff, while the opposition will be composed of the most discordant elements, and will lack all the moral force which unity of purpose and action bestow.

The revolution therefore must commence from within, no agitation without can ever accelerate it; and just so long as emolument and patronage are convertable terms, it connot be hoped that any faculty will be found generous enough to relinguish pay for propagandism. Large profits will still be sought after, diplomas will not be difficult to procure, nor the requirements for graduation hard to satisfy, so long as college faculties are contented with the present status of college classes,

That little unanimity is to be expected from the colleges the history of the last convention unequivocally proves. In the face of the unanimous recommendation of the Association and its special resolutions, the "Teacher's Convention" was

any thing but a success. Many colleges were not represented, and the delegates from the others could arrive at no agreement, and decided on no measures of reform worthy of adoption. It adjourned. we believe, after moving a day of reassembling, whose proceedings will doubtless be as profitable as the first.

The committee—to return—presented the following conclusion on the several propositions submitted to their consideration:

First, "Primary Medical Schools should be encouraged; but as office instruction will continue to be sought by students, practitioners should either give them necessary advantages of demonstrations, illustrations and recitations, or if not prepared to do so, they should refer them to such primary schools, or medical men, as will give them proper instructions.

Second, The number of professorship's should not be less than seven, viz.: a Professor of Anatomy and Microscopy, Physiology and Pathology, Chemistry, Surgery, Practical Medicine, Obstetrics and Materia Medica.

Third, There should be but one term annually, which should commence about the first of October, and close with the March following, thus lengthening the term to six months. The commencement of the term in October, should be uniform in all the colleges throughout the country. During the session there should never be more than four lectures given daily.

Fourth, The qualifications for graduation, in addition to those now required by the schools, should be a liberal primary education, and attendance upon a course of clinical instruction, in a regularly organized hospital."

The succeeding report, on Spontaneous Umbilical Hæmorrhage of the Newly-Born, is by J. Foster Jenkins, M. D., of New York, and is statistically curious as presenting a large number of carefully arranged tables of this rather rare accident. One hundred and seventy eight cases have been collected by the industrious author, from foreign and home journals and through the courtesy of friends. The tables themselves are so drawn out, as to present at a glance, the autho-

rity and reference for the case, sex of child, hereditary history, character of labor, condition at birth, date after birth when cord was separated, when hæmorrhage began, the symptoms, treatment, result and autopsy.

It appears from an examination of the above tables, that there are two varieties of the form of hæmorrhage under discussion; first and most common, that depending on a depraved condition of the blood; second, that arising from an unusual patency in the umbilical vessels. These two conditions are often associated, but the second may sometimes exist independently of the first.

Any influence which affects the plasticity of the blood may act as a predisposing cause of hæmorrhage. This is of course more likely to occur when the mechanical obstacles usually impeding it cease to exist. Any influence which tends to prevent the formation of a coagulum in the outer extremity of the umbilical arteries, or which displaces it, will be likely to produce hæmorrhage when the healthy constitution of the blood is deteriorated, some of these influences are; 1st malformation of the liver and its deranged function, ending in jaundice; 2nd Polydipsia. A spanæmic condition of the blood the author thinks is often induced by excessive drinking of diluent fluids. We are inclined to regard this however as merely a symptom of an atonic state of the vessels, giving rise to dropsies, and hæmorrhagic effusions, depending on an antecedent vice of nutrition, whose etiology requires further investigation. 3rd, Scrofula or Syphilis. 4th, excessive use of alkalies by pregnant women, referable, also, in our opinion to a similar cause as the second. 5th, insufficient food, depressing causes of every kind, physical and mental. 6th, arrest of the fœtal development of the liver. 7th, a diseased condition of the umbilical vessels.

Umbilical Hæmorrhage usually takes place after the separation of the cord, and in the majority of cases on the 8th, day. It has been preceded some times by colicky pains or vomiting, oftener by signs of an unnatural condition of the liver, as clay colored stools, jaundice, &c., not unfrequently by purpura; generally there are no premonitory indications.

The hæmorrhage may be rapid, in gushes, or slow by oozing or by drops. The average duration in eighty-two fatal cases was three and a half days. Death has occurred within three hours, while life has been prolonged twenty-eight days. Coma and convulsions, complicated six cases.

The diagnosis is not difficult, if a careful examination is made, and is much assisted by a consideration of the prodromes. The prognosis is fearfully grave; five sixths of those attacked succumb to the disease. The cases most likely to recover are those of the healthy children of healthy parents, in whom hæmorrhage has only come on after separation of the cord,

To be efficient the treatment of umbilical hæmorrhage must be undertaken early. Compression, styptics or caustics cannot be trusted. Churchill's method of filling the umbilical depression with fluid plaster of paris has been successful, but is not to be relied on. The author recommends as the best resource, the adoption of the ligature *en masse*, as practised by Paul Dubois, which may be effected by the figure of 8 suture, passed around two needles transfixing the integuments at right angles to each other, horizontally; also the internal use of mineral acids, and the preparations of Iron. Anodynes are sometimes useful in allaying the spasmodic mucular action, which occasionally aggravates the bleeding.

The laborious report of Dr. S. M. Bemiss of Kentucky, on Influence of Marriages of Consanguinity upon offspring, comes next in order. Considering the short time allowed the author, only one year, he has performed his work well, and however imperfect as affording conclusive results the paper may be, it will furnish an admirable basis for more extensive inquiries in this direction hereafter. To the physiologist, the philanthropist, and society generally, the subject is one of surpassing interest, affecting, as it does, so many and such important interests. But although passing allusions to the evil consequences of marriages of consanguinity, are to be found in a number of authors, and although popular attention has always been strongly attracted to it, Dr. Bemiss has been the first—so far as we are aware, to publish an extended

series of examples, with an object to throw light upon this engaging but obscure topic. The question is one to be settled by comparison only, but although the author has been remarkably successful in collecting cases of marriages of consanguinity, with their effects on the offspring, he has not been so fortunate in becoming possessed of observations on marriages between parties neither themselves related, nor the offspring of blood relations. His tables of the first class exhibit results of near nine hundred such marriages, while the second comprise only one hundred and twenty-five. Unfortunately, there are no researches so far as he has been able to learn, which establish the average fecundity and vital statistics of marriages in the United States.

Eight hundred and seventy-three observations of marriages of consanguinity, from twenty-five States, have been arranged upon the tables, in classes corresponding to their estimated grades of relationship; exhibiting a statement of temperament, health, and other circumstances relating to the parents and the date of marriage; then the number, sex and condition of children, the number of dead and causes of death. An effort has also been made to ascertain the proportion of deaf, dumb, blind, idiotic and insane, in one asylum, who are the decendents of blood relations, and although this has not been very successful, Dr. Bemiss feels authorized in asserting, that over ten per cent. of the deaf and dumb, over five per cent of the blind, and near fifteen per cent of the idiotic in one State institution, and throughout the country at large, are the offspring of marriages between blood relations.

We are not able to transcribe the results, from the tables of the first class, which are affected by conditions essential to be known in order to arrive at correct conclusions, and for which we have not space. We must content ourselves with offering the following opinion of the author, which he thinks is fairly supported by the observations he has collected.

The increase and diminution of calamities to offspring correspond so closely with the increase and decease of relationship, as to fix the conviction firmly in the mind of the reporter

that multiplication of the same blood by in and in marrying does incontestibly lead in the aggregate to the physical and mental depravation of the offspring. And if we admit the above statement, which the observations here presented abundantly prove—that defects of offspring multiply precisely as we multiply the same blood—and to this admission join the fact that all those contingent circumstances of parental health, habits, proclivities of constitution, sexual incongruity, &c., are as liable to affect one class as another, we cannot rationally assign these effects to any other influence than that of consanguinity.

Dr. E. Andrews of Chicago, Ill., is the Author of the succeeding report, on the "Functions of the Cerebellum."

The author finds reason to differ from the view of Flourens who first advocated the co-ordinating function of the cerebellum, and whose experiments have since been repeated and corroborated by Hertwig, Longet, Magendie, Bouilland and others. Dr. Andrews, regards the cerebellum as an organ, connected with motor functions, and thinks that the lateral lobes, which are properly the posterior, preside over the muscles of the posterior group, and that the median lobe exerts its influence upon those of the anterior group; in support of this theory, he presents the following propositions:

1st. In the warm blooded animals, the median lobe, or vermiform process of the cerebellum varies in size directly as the bulk and power of the anterior group of muscles.

2nd. The lateral lobes vary in like manner, as the power of the posterior group of muscles; subject however to certain variations.

These latter are the complexity and size of the cerebral hemispheres and relative power of the posterior muscles, which conditions affect directly the size of the cerebellum; the latter being proportionate to the quantity and power of muscular fibres to be moved, with no regard to the simplicity or complexity of their combinations. While however it may be true that the mind through the cerebellum co-ordinates motion, it does not do so because it possesses a specific function of co-ordination, but because its action is directly excito-

motor, and the mind though it can select any muscle, or set of muscles, it may choose for action. These conclusions, are not offered as decisive, without further conformation, but merely as indicating a new *direction* for further research.

We next meet with a report on the Treatment best Adapted to each Variety of Cataract, by Dr. Mark Stephenson, Surgeon to the New York Ophthalmic Hospital. The paper is an extremely short one for an essay carefully and deliberately prepared upon so important, embarrassing and interesting a question. It is moreover utterly devoid of originality, and can not be considered as in any way adding to the literature of the subject; the author offering but two cases from personal observation, in illustration of his particular views, neither of which is happily chosen.

In case the first, the author operated by the process for solution and absorption, on a child of three or four years of age, who had traumatic cataract. It is intended to prove that frequent repetition of the operation is objectionable. At the expiration of eighteen months, the child was removed to California, and has never been heard of since; yet the Doctor has no doubts as to the final result. From the appearance of the eye, presented in a drawing taken just before the child disappeared, we cannot coincide entirely with the sanguine expectation of the operator, and we are afraid that a second operation will have to be performed, if it has not been done already, with not very satisfactory success.

In case the second, a young woman who was suffering from soft cataract in one eye, its fellow being sound, had the same anterior operation for solution performed.

Eight weeks after, the cataract was entirely removed and she could see with one eye as well as with the other. Dr. Stephenson admits this to be strange, but accounts for it on the supposition that a new lens was reproduced; a palingenesia, which he believes possible in man generally, and in this particular case with such perfection as to enable the woman to sew the finest cambric muslin, without the aid of glasses, by the use of that eye alone.

We find in the volume before us two very interesting and clever reports, on the kindred department of the same subject; the first by Dr. C. B. Coventry, on the Medical Jurisprudence of Insanity, the second, by Dr. D. Meredith Reese, on Moral Insanity in its Relations to Medical Jurisprudence. The first of these is noticeable for its condensation, its simple straight-forward style, its freedom from all confusion of ideas and argument, and the varied knowledge which the author exhibits of the subject he is discussing. The Pathology, classification and tests of insanity are briefly and well described. Next the plea of insanity in criminal cases is considered, and the legal relations and responsibilities of the insane so far as they affect personal liberty, control of property, general business capacity, amenability to punishment for crime, are concisely presented. The report closes with a consideration of the testimony of skilled witnesses in courts of justice, and the directions and advice here given are worthy of the serious study and consideration of any physician who expects to be examined as an expert upon this subject for the first time, and who has little familiarity with the ingenious tortures with which witty or irritable lawyers delight to rack medical witnesses, for the edification and amusement of bystanders.

The other report referred to above, on Moral Insanity, is possessed of equal merit with the first. We regret that space forbids our making an analysis of either, but we must content ourselves with merely calling attention to their merit as well prepared digests of most intricate and engaging topics. The practical conclusion which Dr. Reese arrives at from his examination of the question, are the following, which we heartily commend:

1st. Insanity is a *mental* phenomenon, symptomatic of a *physical* disease, having its seat in the *brain*, hence, can be neither moral nor intellectual, *exclusively*.

2nd. No alledged insanity, not preceded or accompanied by *positive and demonstrable disease of the brain* should justify the judgment either of *incapacity* or *irresponsibility*.

3rd. When any disease of the brain is proved to exist with

coincident perversion of the moral or intellectual functions, of which the subject is unconscious, for his own safety as well as the protection of society, he should be committed to some asylum, until his restoration is satisfactorily established.

4th. Alledged "Moral Insanity," if unaccompanied by proof of disease of the brain, should be regarded as *moral depravity*, and the party held accountable to the law.

5th. No theory of loss of "moral freedom," should be countenanced by the profession in the absence of evidence of physical disease of the brain.

6th. Neither "delusion," "hallucination," "knowledge of good from evil," "hereditary taint," &c., can be relied on in deciding the question of responsibility; the presence or absence of disease of the *brain* being the only true criterion of sanity or *insanity*.

7th. In all cases of acquittal from crime on proof of insanity, perpetual restraint upon liberty during life, or until perfect recovery, should be secured by *statute law*, and insane convicts should be confined in distinct institutions.

8th. Medical psychology, in its relation to juridical inquiries on the subject of insanity, should be made an integral part of medical education.

9th. No physician who has not special qualifications in this department, should consent to give testimony in cases of alledged insanity, unless after consultation with and the concurrence of an acknowledged expert.

The author of the voluminous report, on the Nervous system in Febrile diseases, Prof. H. F. Campbell, of the Medical College of Georgia, needs no introduction. He will be generally recognized as one of our most indefatigable and pains-taking workers, who already achieved a reputation in a difficult and most interesting field of inquiry. In the last volume of the Association reports, certain deductions on the subject of the Nervous System in Febrile diseases, were presented by him as the result of investigations in this department of medicine. Each of the positions there assumed, are here examined separately, with most commendable distinct-

ness, comparative brevity, and great care and industry. It would be impossible to do more than state in very general terms our opinion of his essay, which covers a space of over one hundred and seventy pages, and without detailing either the argument or illustrations, we cordially refer the reader to his admirable and ingenious elucidation of the following propositions, which form the thesis of his paper.

As all the moral phenomena of living organisms occur under the influence of, and are controlled by the nervous system, it is rational to regard all morbid actions, as influenced in their manifestations by aberated nerve force.

As in the nervous system, we recognize two grand departments, viz: The cerebro-spinal system, all the normal actions of which, are subject to cessation and interruption, and 2nd, the ganglionic system whose actions are of a continuous character, so in the manifestation of febrile diseases, we distinctly recognize two grand distinguishing characteristics, a character of *paroxysm* obtaining in certain cases, while a character of *continuousness* as plainly marks the others.

As the action of the cerebro-spinal system, moreover, pertains to sensation and motion primarily and chiefly, while the *ganglionic system* presides over nutrition and secretion, so in *paroxysmal fevers* we find intense pain, modified sensation, and symptoms which ally them to neuralgic and convulsive diseases very prominent; while in *continued fevers* modified and altered secretion are the marked characteristics.

Dr. Campbell, has also, for the sake of convenience and for facilitating progress in this study, drawn out the *sketch of a plan* for the tabulation of the Neuroses, "that portion," he says, "which refers to *febrile* diseases is more complete though not perfect." We doubt not, if his life and health are preserved, that he will in future communications develope further and more thoroughly his views upon this subject, to which he has long and meritoriously devoted his time and talents.

Preceding this report is one by Dr. Edward Jarvis of

Dorchester, Mass., on the Law of Registration of Births, Marriages and Deaths. In connection with the State Returns, we have repeatedly urged the importance of such statistics, and we believe that the profession as a body, and the majority of communities are now sufficiently alive to their value. In Massachusetts the system is better organized and the reports more full and reliable, than any elsewhere published in this country; but some ten or twelve other states are now energetically directing their attention to the subject, and the time is opportune, by such concert of action as the Association may induce, to procure a general uniformity of method and result.

To the report are appended two nosological tables, one prepared for the use of the census department of the United States, the other that of Dr. William Farr, reported to the World's Statistical Congress, held at Brussels, and adopted, in Great Britain; it is used by the registration officers of England, Scotland and Massachusetts.

Dr. D. L. McGugin, is the author of a brief Report on Stomatitis Materna. This disease known as *thrush, nursing sore mouth*, &c., has of late years attracted considerable attention, though in many portions of our country, it has not so far been met with, and some of our best authorities, residing in populous cities, have only described it from the observations of others. The author follows the commonly entertained view of its pathology, and considers it an error of nutrition, peculiar to women during pregnancy and lactation, though it may exist when neither condition obtains, as a continuation of a previous invasion during one or other of these states.

The most marked predisposing cause is the tubercular or strumous diathesis, but he looks upon the disease as distinct from either chronic follicular stomatitis or land scurvy.

There is nothing new or worthy of note in the detail of symptoms, the argument for its etiology or pathology, nor in the therapeutic recommended. The latter adopted by the author is the iron tonics, alteratives, fresh air and abundant exercise, good wholesome food, warm bathing, and removing

the child from the breast as a last resource; even this will not always arrest the progress of the disease, when it has become chronic and has continued through several pregnancies.

Dr. J. B. Flint's Report on the True Position and Value of Operative Surgery, as a Therapeutic Agent, is a manly, fearless and scathing rebuke of that unscientific spirit, and despicable love for notoriety by which many are seduced from the paths of wisdom and honor, to their own shame and the hurt of others. It exhibits, in glowing and most praiseworthy terms, the exalted office of him who feeling it to be his duty and highest reward to heal the sick and bind up their wounds, is continually striving, how he may restore and preserve, not mutilate, the members and organs of the unhappy sufferers who appeal to his art for relief. It moreover fairly shows that while few of the "major operations," as they are called, are entitled to a place among the legitimate resources of the healing art, "no such doubt or reservation applies to that great class of professional services, unfortunately demonstrated, *minor surgery*." To all young surgeons who are aiming to become wise and conscientious practitioners, we would most heartily commend the conservative and judicious opinions of the author upon this momentous topic.

Passing by a "Method for Preserving Membranous Specimens," by sprinkling them with arsenic, and varnishing them, which Dr. R. D. Arnold, would inform the Association how to do, in the very gratifying space of three quarters of a page, and a letter from Dr. Fenner to Dr. Eve, which our readers would not be interested in, we close our task and this voluminous book with a passing mention of the two Prize Essays.

The first, on the Clinical Study of Heart Disease, is from the pen of Austin Flint, M.D.; it fully sustains the author's excellent reputation, and justifies the award of the committee. In our next number we will present our readers with an analysis of its contents; a work which however, grateful at this time, would occupy more space than we could possibly bestow.

The second is an Essay on Vision and some of its Anomalies, as Revealed by the Ophthalmoscope, by Montrose A. Pallen, M.D., of St. Louis. It is a clever critical and literary performance, exhibiting proofs of scholarship, labor and research in its preparation, but adding nothing to our stock of knowledge, and barren alike of original facts and suggestions. However creditable it may be to the author as the product of a well trained and well stored mind, we cannot but think that it is devoid of that kind of merit which should characterize a "prize essay," and that the committee has made a mistake, and done injustice to the body it represents, in bestowing upon it the high encomium of special and deliberate approval.

If we had space for the criticism, we might point out several defects which detract from its character as a mere summary, even, of our knowledge of the physiology of vision, and by consequence, of the organic and functional lesions of its organ. For example, although the author notices, in speaking of the ocelli of the lower invertebrates, the absence of a retinal, and the universal presence of a pigmentary coat, as well as the peculiar sensitiveness of the "yellow spot of Sœmmering," which is uncovered by any of the expanded fibres of the optic nerve, he speaks of the function of the choroid as one of absorption of light merely, without even adverting to the plausible, if not demonstrated view, lately advanced by one of our ablest physiologists, and assented to by many distinguished philosophers, that it is the true receiving screen of the eye.

The plates which accompany the essay are, with two exceptions, copied from Jäger, and, like the text, are familiar to all who have any acquaintance with the literature of the subject.

BIBLIOGRAPHICAL NOTICES.

ART. V.—*Notes on the Surgery of War in the Crimea, with Remarks on the Treatment of Gunshot Wounds.* By GEORGE H. B. MACLEOD, M.D., F. R. C. S., formerly Surgeon to the Civil Hospital at Smyrna, and to the General Hospital in Camp before Sebastopol; Lecturer on Military Surgery in Anderson's University, Glasgow, &c. London: John Churchill, New Burlington Street, 1859, 12mo., pp. 439.

We welcome, with no little interest, this first English contribution of the surgical experience gained in that great campaign whose thunders have scarcely yet died away along the shores of the stormy Euxine. It comes from the same hand which contributed some excellent papers from the seat of war to the Edinburg Medical Journal, in 1855–56, and bears the stamp of a thorough master of his art, who draws upon large stores of knowledge, gained by a minute and intelligent personal observation.

The Crimean war will long possess an interest sad and terrible, and in some respects peculiarly its own, from many circumstances connected with its origin and history. To the modern military surgeon its lessons will be most valuable, not merely as exhibiting the effects of injuries produced by our new engines of war, but by showing him how extraneous elements, "diseases of circumstance," to be guarded against and averted by every possible means, may prove more fatal than the bristling onset or scathing fire of the foe. To British officials it should teach a wholesome if melancholy lesson of the importance of forethought, self-reliance and energetic dispatch in those who are entrusted

with the safety of large masses of men. To governments it conveys a warning of the value, if not necessity, of placing the surgical staff of armies upon an independent basis, free at once from the officious and arrogant interference of conceited drill-sergeants, and the imperturbable self-sufficiency of routinist agents and commissioners; and to the world it presents another example of that sublime heroism, that unconscious martyrdom, that completest abandon of self-sacrifice and devotion to duty which have distinguished the surgeon, military or civil, from an immemorial date to the present hour.

In a former number of this Journal we took occasion, while the subject was warm with interest, to review, at considerable length, the medical history of the Crimean war, both in camp and hospital, and may therefore be pardoned for passing over the interesting portion of the volume before us, which traverses similar ground.

The legendary and classical fame of the Tauric Chersonese, its physical configuration and character, climate, geology, and seasonal changes, the steppe lands of the interior, the vegetation and resources of the country, and a description of the natives, with an account of their diseases, form the subject of the introductory chapter. Next follows an account of the drainage of the camp, which seems to have been as nearly perfect as the character of the ground permitted; the supply of water, which was generally good; the site and construction of the latrines; the food, cooking, fuel and clothing, which during the first winter were wretched enough, defective both in quantity and quality; the bad housing and intolerably severe and unremitting labor of the troops; the pernicious effects of these upon the health of the troops; the arrangement of the hospitals, distribution of the sick, nursing, male and female, and transportation of the wounded, which were for the most part as badly provided for and arranged as is conceivable.

The author does not omit to pay a passing but earnest tribute to that "noble type of good, heroic womanhood," whose name is so touchingly and intimately identified with

the history of the campaign, and to the invaluable services of properly trained and paid female nurses—not *ladies*—in military hospitals, if selected with discretion and employed with caution.

In chapter the third, the disastrous and badly managed campaign in Bulgaria, and its effects on the troops, the unfortunate location of the camp, by which the splendidly equipped army which landed at Varna was decimated from disease, and transported to the seat of war in a condition better fitted to take the hospital than the field, are briefly and effectively described; and having thus rapidly sketched the progress of the expedition until it reached Sebastopol, and indicated the leading circumstances which exercised an influence on the health of the troops, Dr. Macleod concisely reviews the ravages of the diseases which resulted from these and previously mentioned influences, whose result was the destruction of ten thousand men in seven months, and which delayed the fall of the fortress for more than a year.

Scurvy, as might be inferred from the above facts, was the most destructive agent against which they were called to contend, both in its immediate and secondary effects; and among the other diseases, depending on the unfavorable circumstances as to food, shelter and duty in which the troops were placed, diarrhœa, dysentery and typhus fever were the most formidable in their character and fatal in their results.

These remarks allude, it must be remembered, to the early period of the war only. During the second winter a marvellous change was effected; the organization of every department became the most perfect and efficient, every luxury prevailed in the English hospitals, and the army lived as perhaps no army ever before fared in the annals of war. The change which took place in this respect was in marked contrast with the continued suffering of the French troops, and occasioned the handsome compliment from the French Medical Inspector, in his review of the campaign: "Quand on compare les conditions oú se trouverent les Anglais au debut de la guerre qui les prenait au depourvu,

et celles ou ils s'etaitent placés en 1856, on est forcé de reconnaitre la grandeur de la nation Britannique."

A rather curious affection of the hands and feet was common both in the English and French hospitals during the first winter and spring, which Dr. Tholozan was inclined to look upon as allied to a peculiar disease, termed acrodynia, which appeared epidemically in France between the years 1828 and 1832. Dr. Macleod was persuaded that it was the product of cold and scurvy, a union of rheumatism or the early stage of frost bite with a weak circulation and scorbutic taint. In India a similar affection, called by writers "burning feet," is mentioned as being a distressing sequala of rheumatism among the Sepoys. The disease known as beriberi has, in some of its slighter forms, a resemblance to it, and there are traces of its occurrence during the Peninsular war.

This affection showed itself chiefly in the pulpy parts of the hands and feet. Its earliest symptom was a prickly sensation, experienced when the patient stood upon the foot. There were lancinating pains in the calves of the legs, which parts felt hard and brawny, and were sometimes swollen and discolored. There were weakness in the limbs and distressing heat in the feet, most marked at night. Occasionally the parts affected were numbed, and an erethematous redness was observed along the edges of the feet and hands. "It was often localized in small patches, and not always accompanied by other scorbutic symptoms. It was often combined, too, with low fever or dysentery, and not unfrequently followed by desquamation of the epidermis, and sometimes by local gangrene."

Having prefaced thus much, it remains, in noticing such a volume as this, only to point out, and that concisely, the modifications of surgical practice introduced in consequence of new conditions of lesion, or progress in the art itself.

Among the first of these elements of modern field surgery the most important arise from the use of the new rifle, with its conical ball, and the great momentum, extent and accuracy of range which have been given to round-shot and

shells, from late improvements in the guns carrying these projectiles. The advance in the latter, however, has been far from parallel with the signal change effected in the first, which is at present by far the most effective arm of service.

The rifle used by the Russians was little inferior in range or force to the Minie, while its conical, deep cupped ball was much heavier. The same fact is noticed by Larrey in his Surgical Memoirs of the march to Moscow, and speaks well for the skill and military foresight of the Sclav. The severity of primary action, the immense comminution of bone, and the amount of laceration of the soft parts, occasioned by these balls, requires amputation to be more frequently resorted to, and the distance from the trunk at which that operation has to be performed being diminished by the same cause, has greatly increased the resulting mortality.

The characteristics heretofore insisted on as distinguishing the wound of entrance from that of exit have lost much of their value, for Macleod remarks that, with rare exceptions, if a conical ball be fired at close range, and traverse the soft parts so that its speed is not sensibly diminished by its passage through a limb, the difference in size will be very slight, and may even be in favor of the wound of entrance. He did not notice either the difference generally observed of inversion and eversion in the contrasted lips of the wound.

From the comparative little employment of the round ball during the late war, there were fewer illustrations of the splitting of balls on the edge of bone than usually occur in a campaign, and less instances of the *eccentric course* pursued by balls were observed than generally marked the experience of former campaigns. The conical ball takes the shortest cut possible through a limb, and was seen at Alma to pass through the body of two men and lodge in that of a third.

Hæmorrhage, the great bug-bear of the old surgeon, is now little feared from being well known not to be of frequent occurrence on the field, and from the means of arresting it being well understoood. Unless the projectile includes an

artery between itself and a bone the vessel usually escapes, and Blandin has shown that although primary bleeding always takes place when a large vessel has been injured, it is soon spontaneously arrested by the torsion and retraction of the arterial coats. Convalescence was observed by our author to follow between the fifth and twenty-fifth day, and appeared in the majority on the fifteenth day after the receipt of the wound. Dr. Macleod concurs in the views lately put forth by Nelaton, in opposition to the opinion of Dupuytren, as to the unsound state of the artery in suppurating wounds. He is sure that a vessel will in most cases bear a ligature, if carefully attached, long enough to fulfil the desired end. Under these circumstances, when the bleeding appears late, the French perform Anel's operation of tying the main vessel.

It is interesting to remark that the author met with many cases of hæmorrhage from the hand, succeeding gun-shot wounds. These injuries resulted often from the accidental explosion of the patient's own gun, "and I suspect," he adds, "in not a few cases from intention." The reader will remember that Baron Larrey, by order of Napoleon, held a court-martial upon upwards of three thousand soldiers, presenting mutilated hands after the battles of Lutzen, Bautzen, and Wurchen, who had been reported to have done this voluntarily. It is pleasant to record that these gallant boys,—their average age was not over eighteen,—were acquitted and honorably restored, through the skill and honesty of the great Surgeon-in-Chief, but there can be little doubt that these *accidents* will very frequently occur in all armies raised by conscription.

Chloroform was almost universally administered in the British army, and extensively among the French. Baudens says that the latter had no fatal accident to deplore from its use in over thirty thousand cases. One death only occurred among the English that could be fairly attributed to its employment. The patient was a man of thirty-two years, who was about to have a finger removed. The chloroform was administered on a handkerchief, as he *sat in a chair*. It

is the author's conviction that the experience of the Crimean war has shown that chloroform saves life directly and indirectly, and abates a vast deal of suffering; that its use is as plainly indicated in gun-shot as in other wounds; and that if carefully administered, it matters not by what accident the operation is necessitated.

We pass over the chapters on injuries of the head, face, abdomen and bladder with a mere *resumè* of the number of cases and result. The necessity of doing this is extremely unfortunate here, but cannot be avoided, and the interested reader is cordially referred for particulars to the work itself.

Injuries of the Head.—From April 1, 1855, to the end of the war, the returns show 630 cases of gun-shot wounds of the head attended by contusion merely, more or less severe, and 8 deaths are recorded among those cases. Of gun-shot fracture without *known* depression, 61 cases appear, and 23 deaths therefrom. Of cases of fracture and depression, followed by sensorial disturbance, 74 cases are mentioned, and 53 deaths therefrom; while of wounds penetrating the cranium, 67 cases and 67 deaths are recorded. Of 19 cases in which the skull was perforated, all died. The trephine was employed 28 times, and of these cases 24 ended fatally.

Wounds of the Face and Chest.—After the 1st of April, 1855, to the end of the war there occurred 382 cases of simple flesh contusions, and wounds of the face more or less severe, and one death is classed under this head. Of wounds penetrating, or perforating the bony structure of this region without injuring important organs, there were 107 cases, and 10 deaths; and of those accompanied by lesion of important organs, 44 cases appear, and 3 deaths. Most of the fatal results were owing to other concurrent causes.

Gun-shot Wounds of the Abdomen and Bladder.—The returns of the war, after April 1st, 1855, show flesh contusions and wounds (simple and severe) of the abdomen, among the privates, as having occurred over 101 times, with a fatal issue in 17 cases. There were 38 penetrating wounds with lesion of viscera, and 36 deaths in consequence; while 65

times the abdomen was perforated, and 60 deaths resulted.* Four cases of rupture of viscera without wound were fatal.

Compound fractures are among the most serious injuries received in battle, from the manner in which their extent and gravity are marked, and the uncertainty which prevails in regard to their treatment.

From time immemorial the danger of these lesions has been dwelt upon by surgeons, and immediate amputation unanimously advised. In the language of Dupuytren, "In compound fractures from gun-shot, in rejecting amputation we lose more limbs than we save lives," and all leading modern surgeons have adopted and acted upon this opinion.

In the Crimea it was hoped that conservative surgery would save many a before-condemned limb, but an unfortunate experience taught both English and French surgeons that the experience of civil life in compound fractures from accident, was far from applicable to those resulting from gun-shot wounds. In trying to save such limbs many lives were lost in the beginning of the campaign, and the attempt was given up as hopeless. Two circumstances seem to have had to do with the irreparable character and mortality of these compound fractures,—the state of health of the wounded men, and the effects on the bone of the large, conical ball,—both of which circumstances have already been touched upon. After making every effort, the author was able to find a record of three cases only, in which recovery followed a compound fracture in the upper third of the femur, without amputation. In *two* of them the injury was occa-

* M. Legouest mentions 3 cases of penetrating wounds of the abdomen in the Dolma Batchi hospital, all of which died. Alcock reports 18, only 1 of which recovered. Meniere mentions 14, in which the ball penetrated, 2 of them being through the side, and all died; while of 7 others, in which the ball passed through the side only, recovery followed. In the Indian wars I find the record of 38 penetrating or perforating wounds of the abdomen, of whom 32 died and 6 recovered. Colles states, that in the sieges of Moultan "not one case recovered in which the abdomen was fairly shot into and the small intestine wounded." Sedillot tells us, that in the expedition against Constantine, they lost all those whose abdomens were penetrated by gun-shot.

sioned by *round balls*, and the comminution was slight; in the third case the species of ball could not be ascertained.

Some excellent remarks on wounds of joints and amputations conclude the work proper, to which are appended very valuable tables, exhibiting the geology of the Crimea, thermometric observations, the weight and nutritive value of the soldier's ration, the return of duty in the Crimea, the weight of balls used by the belligerants, the mortality following the greater amputations for gun shot wounds and accidents, and a resumè of M. Scrive's work on the French losses in the Crimea, and of the Report of the British Government on the Surgery of the War.

We have thus hastily, and much to our regret, skimmed over a book which every one will read with great interest, and few without profit. It is the work of a good scholar, an able surgeon, a close observer and most faithful reporter. The meagre outline we have given of his performance, is but a poor sketch, barren of all the lights and shades with which the author has skilfully brought out the desired effect. Among the details of cases—which are very numerous—will be found examples of all kinds of gun-shot wounds, as interesting as any to be met with elsewhere in the whole annals of military surgery.

ART. VI.—*Five Essays.* By JOHN KEARSLEY MITCHELL, M.D., late Professor of Practice of Medicine in Jefferson Medical College, of Philadelphia, &c., &c. Edited by S. WEIR MITCHELL, M.D., Lecturer on Physiology in the Philadelphia Association for Medical Instruction. Philadelphia: Lippincott & Co., 1859, 12mo., pp. 371.

No act of filial reverence could be performed by a son worthier a father's memory, than offering to the public such a series of papers as constitute the bulk of the volume before us. The highest testimony to the intellectual worth

of such men as the late Dr. Mitchell is to be found, not in the eulogy or regret, however lavish and earnest, of surviving friends, but in such living proofs and enduring records of lofty and well employed talents as these essays furnish; and all who are thoroughly imbued with the spirit of scientific inquiry, or a love of medical progress, will thank the hand that has rescued these waifs from the shifting currents of journalism on which they were first cast, and given them the collected and more permanent form which they so well deserve.

With a single exception, these articles have already been submitted to public criticism, and if the verdict has not always been favorable to the entirety of the views promulgated, they have attracted no little attention from all quarters, and gained the uniform consideration and not unfrequent approval of our best informed and most acute thinkers. Two of them are models of accurate, philosophical, and rigid scientific deduction, and a third will long rank highly among the most ingenious and original hypotheses which have been projected in regard to a class of diseases eminently interesting, and whose etiology and pathology is still involved in mystery and doubt.

The first essay, on the Cryptogamous Origin of Malarious and Epidemic Fevers, was delivered in substance to the author's classes, some years before Dr. Charles Cowdell published his disquisition on Pestilential Cholera. In this dissertation Dr. Cowdell attempts to explain its cause, nature, preventive, treatment, &c., by reference to a fungous origin, and Dr. Mitchell, upon its appearance, very properly yielded to the solicitation of friends, who were unwilling that his reputation should be sacrificed to a false modesty, and sent his lectures at once to the press.

With the theory and argument of the author every one is too familiar to render quotation necessary. A critical review of the paper was published in the May number, 1849, of this Journal, immediately after the work was issued, to which we must refer the interested reader. The hypothesis is, that miasmatic or marsh fevers, as they are called, are the pro-

ducts of a living organic cause, capable of reproduction by germs, which are not produced by the organism of the sick, but exteriorly to and independent of the human body. Of these living agents the fungi are distinguished for their diffusion, poisonous properties, season of growth, number, minuteness of spores, &c., and are to be considered the chief, without excluding the agency of other cryptogamous plants. In developing this view, much ingenuity is exhibited, and though the objections against its conclusiveness are not all satisfactorily removed, sufficient plausibility is given to it to lift the hypothesis from the category of reckless and unwarrantable assumptions, and place it among those well grounded opinions which open up new fields of thought and research richly worth cultivating.

The second essay, upon Animal Magnetism or Vital Induction, treats of a subject which attracted at the time of its preparation, much attention from some of the ablest and wisest men throughout the civilized world.

The novelty and startling character of its phenomena, the obscurity of its laws, the general mystery which hung around the whole topic, and the vague and boastful pretensions of some of its disciples, gave it, for a season, a fictitious importance in the minds of all pschyologists. To an ingenious and bold thinker, and no less apt and enthusiastic experimenter, like the author, it was specially inviting, and we are not surprised at the earnestness which he brought to its investigation, or the interest which this paper, intended for presentation to the College of Physicians of Philadelphia, manifests in the solution of a now almost forgotten problem.

We do not think that the editor has acted judiciously in giving it a place among this collection. It is not a fair example of the author's usual vigorous and discriminating judgment, though it exhibits forcibly his subtlety as a dialectician, and his uniform honesty and power of investigation. The author himself, with a commendable prudence, withheld it from print, and although, now that the heat of controversy has been chilled, there is no fear of any charge

of empiricism or delusion being urged, we are of opinion that if his life had been spared, Dr. Mitchell would have subjected it to much elision, and modified very seriously some of the opinions here expressed, before giving them an irrevocable publicity.

Perhaps our own convictions in regard to the character and meaning of the facts here discussed, and total lack of interest in the subject, except as a psychological curiosity, may bias our judgment, and render an impartial award difficult, if not impossible; but we are sure that time and its changes have altered greatly the supposed relations of mesmerism to medical science, and that the followers of the latter feel no longer the same interest in these phenomena, true or false, as they formerly did. Whilst the essay, therefore, appearing for the first time, will be subjected to the criticism properly applicable only to a new performance, it will not be sustained by that freshness of enthusiasm and feeling with which it would have originally been greeted, and its errors will be the more readily detected and longer remembered than its general excellencies or interest of detail.

After reviewing the examinations of this subject previously made, and pointing out their sources of error and palpable contradictions, the author endeavors to establish the following important points, among others, by a report of a large number of cases personally observed, experimented on, and recorded, many of which are curious and attractive.

The mesmeric sleep and awakening is producible without the aid, knowledge or consent of the subject.

The phenomena of this artificial somnambulism are, an exaltation of the circulation without a corresponding increase of the respiration; an obtunded sensibility to pain; obliviousness of the thoughts and events of the mesmeric state while awake, though memory of the events of the natural state is strong in the artificial; the retention of locomotion and a facility of being led into suggested dreams. To this last property may be referred the alledged miracles of clairvoyance, intuition and prevision.

Sex does not appear to exercise any marked influence on the mesmeric susceptibility; age is a more modifying cause, though no age is exempted; of the temperaments, the nervo-sanguineous seems most liable to mesmeric action.

The *rapport, relation, or communication*, supposed to have an absolute existence dependent on the mesmeric fluid, is entirely voluntary on the part of the patient, and rests on his knowledge of its supposed necessity; but while it is therefore a delusion, the subject is no more answerable for it than for any other of the unreasonable dreams to which he may be led while in this condition.

In the mesmeric state, sight, hearing and touch are usually improved; taste, smell, and sense of pain as commonly impaired.

As the sense of touch and pain are so diversely affected by mesmerism, we are led to regard them as independent senses, probably supplied by separate nervous fibres; the sense of pain may therefore be a *sixth sense.*

The phrenological phenomena of mesmerism when rigidly examined are found to be delusive. The excitement of the brain is general, whilst the direction of that excitement is given by the mesmerized person's knowledge of phrenology, but the subject is not aware of his own mental co-operation.

The mesmeric influence is the effect of *induction.* The will of the operator acts *solely on himself; his altered system reacts by proximity* on the subject of the experiment by an unexplained power, analagous to the equally inexplicable induction, and presence, of the mechanicians, and chemists.

Mesmerism may be employed to relieve, temporarily, nervous irritation, to procure sleep, and obtund nervous sensibility during surgical operations, but it is sometimes dangerous, seldom successful—from the fewness of susceptible persons—has never cured serious disease, and should therefore be used rarely and with great caution.

The claim to a peculiar medical intuition, set up by traveling exhibitors is absurd and without foundation. The pathology is rediculous, the pharmacy cumbrous, and mischievous,

or inefficient, and magnetism has not yet detected a new theory of disease, or suggested a single useful remedy.

We cannot enter into discussion of the above propositions, a bare outline only of which we have room to give. Stripped of all the delusions, impostures, and unconscious deceit, the charlatanry and credulity with which the subject is invested, something in mesmerism unaccountable, and so far unexplained, remains—a sympathy remote or personal—which is true and undeniable. How much of this is real, what are its causes and effects, under what laws does it operate, and how may its phenomena be explained, are questions which will never be devoid of interest to the physiologist. For their solution they require cautious and patient investigation, a profound knowledge of the functions of the nervous system, a correct psychology and a capacity on the part of the investigator for the highest and most rigid induction. That we are not yet prepared to bring these requirement to its study, few will doubt; but we must not lightly disregard or reject any light however feeble, or coming from whatever quarter.

While we do not refuse to this essay the praise of being an earnest and scientific effort, directed with much skill, unimpeachable honesty, and great assiduity, we do not think that it has advanced our knowledge of its subject in any marked degree, however well it has pointed out some before unnoticed errors and indicated their sources. It is moreover marred by occasional loosness of logic and argument, and defects of physiological knowledge, the last of which may possibly be owing to the state of the science at the time when it was written. Of this we can offer but a single example.

In speaking of the modification of the senses produced by mesmerism, the author observes that he was at first surprised to find that the gustatory and olfactory nerves had lost their power while those of the eye, the ear, and tactile sensibility were often greatly improved. "A little reflection," he adds, "pointed out a natural distinction on which the difference is probably founded. The *sight*, *hearing* and *touch* are purely *mechanical* senses, and probably vision and audition are mere modifications of touch. But *taste* and *smell* are *chemi-*

cal senses, and therefore not likely to be influenced by the same causes as those which are mechanical."

Now, of all the senses, that of taste is most nearly allied to the mechanical one of touch. In the first place its organ the tongue contains no special nerve of taste, gustative impressions being received and conveyed by the lingual branch of the Fifth, and the Glosso-pharyngeal; both afferent and sensitive nerves. These nerves minister to sensibility and give rise to pain, which the optic and olfactive do not. Again, actual *contact* of the sapid substance with the organ of sense, is absolutely necessary to the exercise of the function; and, lastly, the papillary structure of the tongue and skin very nearly resemble each other. Moreover, as Baly, Wagner and others have shown, ordinary mechanical and electrical stimuli may give rise to gustative sensations. If the tongue be made part of a galvanic circuit—as in the school-boy feat of placing its tip between a piece of silver and a piece of zinc,—a saline taste will be perceived. It the tip or edge of the organ he struck lightly and quickly with the finger or a current of cool air directed on it, the same phenomenon will ensue; and on the other hand if the root of the tongue be touched with a clean glass rod or a drop of distilled water be placed on it, a bitter taste will be excited which by continued pressure of the rod may pass into nausea. Other facts might be adduced to the same end, but the above are sufficient to show that the dissimilar impressibility of the senses under the same conditions is *not* due to the alledged cause.

The two succeeding essays on the Penetrativeness of Fluids, and the Penetration of Gases, deal with a totally different department of experimental research, in which the author was not merely skilled but eminent, and where, if he had continued to labor, he might have gathered abundant and imperishable laurels. The facts here taught have become an integral part of physical and medical science. The exquisite nicety of method, the clearness of thought, the boldness and originality of investigation, and capacity for broad generalization which characterize these papers, indicate a peculiar fitness for these studies, and make us keenly regret

that he ever abandoned them, for the harrassing and absorbing cares of busy professional practice. It is true that as a practitioner and teacher of medicine, Dr. Mitchell effected incalculable good, and enjoyed honor and profit well deserved and fairly awarded, yet had he prosecuted the intentions and carried out the labors of his youth, which was passed mainly in the laboratory, he might have attained a rank as a chemist and physicist second to none in this country and enviable in any other.

In 1829, Dr. M. read a memoir before the Philosophical Society, on a new method of forming gum-elastic into thin plates, sheets and bags. The balloons formed by this process had been filled with hydrogen-gas, and those which were confined in the atmosphere of a room, were observed to descend after some hours. The cause of this descent became a subject of investigation, and led to the discovery of the penetrativeness of gases for this membrane. Analogy, the experiments of Dutrochet and the imperfect observations of Priestly and Graham, suggested the certainty of finding animal membranes performing relatively the same functions to the same gases, and this he not only demonstrated beyond a doubt, but while disproving the then fashionable electrical theory of Dutrochet, established the following facts, since corroborated, though not always referred by their discoverer to their true laws.

Substances formed of organic matter are generally penetrable by gases and liquids of all kinds. Each tissue is differently penetrable as to time by different fluids, but these rates can be ascertained; in the case of gases, whatever the penetrability of the tissue, the ratio is the same observed in experiments on other tissues.

The ratio is not so uniform in the instance of denser fluids, therefore with regard to gases the ratio of penetration depends on them alone while in the case of liquids it depends on the joint agency of both liquids and tissues. When the quantity of fluid is limited, the rate of progression diminishes with infiltration, is proportional to the state of dilution, and

ceases when a balance is attained, unless some extrinsic force is operating.

The power of penetrativeness is superior to two, possibly equal to more than that of forty atmospheres and it acts on gases and liquids as well as on organic tissues.

In addition to these phenomena, the power of extrinsic agents in affecting penetration and other related facts are dwelt upon at length, but we must refer the reader who is not familiar with them—if any such there be—to the essay itself as a most beautiful example of analogical and direct reasoning upon equally ingenious and delicately executed experiments.

The Essay on the Penetration of Gases was written shortly after the publication of Mr. Graham's discoveries in regard to the transmission of Gases through stucco plugs and other inorganic substances. It was intended incidentally to prove, the error into which the latter had fallen on the subject of the "*diffusion*" of gases through penetrable barriers, and as an experimental review of the ground previously travelled, is remarkable for its nicety of detail and the laborious accuracy and variety of its tables.

This whole subject has been beautifully simplified by Prof. Draper, who has reduced the complex and much mystified doctrine of osmosis to one of the simplest principles of physics, and shown all its phenomena to be dependant upon the laws of capillary attraction. Together with the supplementary account of the diffusion and penetration of gases treated of in their relation to respiration, the last mentioned author has given a resume of the whole subject, which leaves nothing to be desired.

The concluding Essay on a New Practice in Acute and Chronic Rheumatism, attracted much notice on its first appearance, and, in the words of the Editor, "affected the treatment of Rheumatism throughout the country." The author having observed some cases in which lesions of the Medulla Spinalis, produced by accident or disease, were followed by Rheumatic arthritis, had his attention attracted to their re-

lation, and finally was led to the reflection, that Rheumatism originates in the spinal cord, and depends on an irritation of that organ; of the truth of this belief, he became afterwards convinced, from the observation of a considerable number of cases. The therapeutic instituted, in accordance with the pathology, consisted of course in local depletion and counter-irritation of the spinal region, by cups, leeches, blisters, etc. We do not think that it would be found generally applicable, nor are our best practioners of the same opinion in regard to the usual origin and character of the disease.

ART. VII.—*A practical Treatise on the diseases of Infancy and Childhood.* BY T. H. TANNER, F. L. S. Late Physician to the Hospital for women, &c. Philadelphia, Lindsay & Blakiston, 1859, 12 mo., pp. 464.

Dr. Tanner, has in the present volume offered the Practioner a really useful work. It is concise, clearly written, free from all ambiguity of thought or expression and directly practical throughout. The author is familiar with the best literature of his subject and has drawn from it freely and to the best advantage. There is of course much condensation and on some topics a too great meagreness of description and pathology which might have been avoided, but the evident aim of the writer has been to eschew discussion and escape prolixity and this commendable effort has been apparently the sole cause of whatever deficiencies we have remarked.

The general hygienic and domestic directions are most admirable, and we would call particular attention to the philosophical observations on the period of infancy and childhood, the diagnosis and prognosis of infantile diseases, and the therapeutics, management and education of children. These portions of the work above all others, indicate a large

acquaintance with their several topics, and are the conclusions arrived at after much thought, and serious study of the conditions affecting these important subjects. Their sensible treatment and excellent deductions, will commend themselves to every one, while the practical and easily followed directions, which the author lays down, may uniformly be followed with advantage.

To the junior practitioner, this hand-book will be specially valuable, and it is one whose small compass and excellent arrangement facilitates very much the references which may be made to it for purposes of general or special instruction in the management of cases.

ABSTRACTS AND TRANSLATIONS

FROM FOREIGN AND DOMESTIC JOURNALS.

PATHOLOGY AND PRACTICE OF MEDICINE.

Typhoid Fever in Children. From the Second Edition of the Traité des Maladies des Enfants, by MM. F. RILLIETT and E. BARTHEZ. Translated for the Charleston Medical Journal and Review. By DAVID E. EWART, M.D., Newberry, S. C.

[Continued from Vol. XIV., No. 2, p. 290.]

ART. IV.—DIAGNOSIS.

Most authors who have written upon typhoid fever in the adult, have in general too lightly passed over so important a subject as its differential diagnosis. They are satisfied with a rapid enumeration of some of the diseases which present an analogy to dothinenteritis; but they have not sufficiently dwelt upon the difficulties of diagnosis in a large number of cases; and especially in the first stages of the disease. It is still more difficult in the child than in the adult to distinguish typhoid fever from many diseases which bear a considerable resemblance to it. We therefore deem it our duty to dwell upon so important an article.

We will however refrain from comparing typhoid fever with those more or less well marked diseases which, whilst some physicians confound with this pyrexia, others seperate completely from it. Such are prolonged ephemeral fever, synochial fever, and certain imperfectly known remittent fevers. We forbear making this comparison, because these diseases, which we have not described in our work, bring up questions of nosology and doctrine, which we do not yet wish to enter upon.

To attain the end we propose to ourselves, and ever keeping within the bounds of truth, we will, in the following article, not only enumerate the affections which may be taken for typhoid fever, but will dwell upon those diseases which

we ourselves have mistaken for it. The question must be treated in the past for the future.

To render the distinction easier, we will establish the diagnosis according to the form of the disease.

Forms of Typhoid Fever.	*Diseases with which it may be confounded.*
Light. - -	Gastric derangement. Intestinal Catarrh.
Grave. - -	Tubercular meningitis. Enteritis secondary to scarlatina or rougeola. Pneumonia and broncho-pneumonia.
Very Grave.	Typhoid enteritis. Inflammatory meningitis. Acute general tuberculization.

In addition to these, the eruptive fevers, at their commencement, and especially variola, may be mistaken for the three forms of typhoid fever.

We will here limit ourselves to presenting the diagnosis between dothinenteritis and the diseases which we have already described, referring for other details, to the chapters on variola, tubercular meningitis, acute general tuberculization, etc.

Gastric derangement.—We have elsewhere pointed out some of the differences which distinguish this affection from light dothinenteritis, and we have shown that they consist rather in the duration than in the phenomena proper to the two diseases. We will here state that the symptoms to which most value should be attached in recognizing dothinenteritis are: 1st a somewhat notable loss of strength, though it may be often difficult to estimate its degree; 2nd, a slight nocturnal agitation or delirium; 3rd, an abundant diarrhœa accompanied with gurgling in the iliac fossa; 4th enlargement of the spleen; 5th, lenticular spots and numerous sudamina; 6th, a more intense febrile movement than in gastric derangement, and persisting beyond the ninth day; 7th, the existence of a sibilant râle.

Intestinal phlegmasiæ and catarrhs.—Most of the symptoms just enumerated—save the diarrhœa, which is common to both diseases—taken collectively, will enable one to distingish a light typhoid fever from acute primary catarrhal diseases of the intestines. The absence of cephalalgia in enteritis must also be borne in mind. We must however state that in two young children observed by us, the typhoid fever displayed itself under a sub-acute form, and was in every respect like

enteritis. The febrile movement was but slightly intense, the abdomen was neither tense, nor distended, there were neither rose-colored spots nor sudamina, the diarrhœa was abundant, and nervous symptoms were entirely wanting. In one of the cases we were completely deceived, and to such an extent that, believing the case to be one of enteritis tending to the chronic stage, we administered astringents internally. If acute intestinal catarrh in very young children, can be easily confounded with dothinenteritis, the mistake is still easier when the phlegmasi assumes a typhoid form.

Typhoid Enteritis, in fact presents a very great resemblance to the worst form of typhoid fever. We have already (vol. 1., page 786,) endeavoured to lay down the basis of diagnosis, but we feel it to be our duty to here show how easily a mistake may be made, by comparing two cases of primary typhoid fever. Our table is made after the model of the one published by M. Louis in his reply to Broussais.

TABLE.

Two cases of enteritis terminating in death.	*Two cases of typhoid fever, terminating in death.*
AGE.	AGE.
One, three years; the other, five and a half.	One, four years and a half; the other five.
SEX.	SEX.
Both boys.	Both boys.
CAUSES.	CAUSES.
One had been in Paris for six months, had always enjoyed good health, was of a strong constitution, and had a fair complexion. The other's arrival at Paris was not ascertained; he had suffered from several abdominal diseases.	One had been in Paris three years; the arrival of the other was not known. Previous health good, very strong constitution; brunetts.
MODE OF ACCESS.	MODE OF ACCESS.
In one it was sudden, with cephalalgia, epistaxis, fever and constipation; in the other, loss of appetite, diarrhœa, afternoon fever.	In both it was sudden, with drowsiness, head-ache, fever, vomiting, pains in the limbs, and diarrhœa.
DIARRHŒA.	DIARRHŒA.
In one case, there was diarrhœa at the commencement;	In one from the commencement, in the other from the

in the other, on the sixth day. In both cases, it persisted until death. Always involuntary, not very abundant in one case, abundant in the other.

ABDOMINAL PAINS.

In both cases from the commencement; they were general in one case, limited to the iliac fossa in the other.

METEORISM.

In one case, abdomen distended from the third to the eighth day, afterwards flaccid. In the other there was tympanitis on the seventh day; abdomen very much distended from the eighth to the tenth.

CONDITION OF THE SPLEEN.

The spleen could not be felt.

VOMITING.

No vomiting.

APPETITE.

In both cases, no appetite from the commencement.

TONGUE.

Tongue dry and red on the sixth day, dark and cracked on the seventh. Lips encrusted in one case, in the other slightly moist. On the ninth day the tongue was red at the tip, yellow at its base, and remained thus during the entire course of the disease, being alternately moist and dry.

CEPHALALGIA.

Observed in one case at the commencement, not noticed in the other.

third day, and in both cases until death. Nearly always involuntary; three or four stools a day.

ABDOMINAL PAINS.

It could not be ascertained that any existed.

METEORISM.

Abdominal distention from the third day until death, alternately with flaccidity, in one cases. In the other, distension from the third day until death.

CONDITION OF THE SPLEEN.

Enlargement of the spleen in both cases until death. In one case it was noticed on the third day; in the other on the eighth.

VOMITING.

In one on the first, second and third day; in the other none.

APPETITE.

In both case no appetite from the commencement.

TONGUE.

In one the tongue began to became dry on the sixth day; it was red and raw; in the other it was dry on the ninth day.

CEPHALALGIA.

In both cases at the commencement of the disease.

SOMNOLENCE.

None.

DELIRIUM, CRY.

Delirium, cries, extreme agitation in one case, during the night between the fourth and the fifth day; it persisted during the whole course of the disease. In the other, extreme agitation, cries during the night from the thirteenth to the fourteenth day; could not recognize his mother on the fourteenth day. The following days, the agitation was replaced by continual moans.

EYES, EARS, NOSE.

In one case epistaxis at the commencement; dilatation of the pupils the two last days. In the other, the nose was dry and encrusted, the eyes sunken and surrounded with a dark circle, from the thirteenth day.

LENTICULAR SPOTS.

In both cases there were lenticular spots: in one on the ninth and eleventh day, on the right thigh; in the other, two poorly marked spots around the umbilicus, on the ninth day.

SUDAMINA.

In one case, numerous sudamina on the ninth day; fresh eruption on the seventeenth, and on the eighteenth very abundant. In the other there was none.

CHILLS; HEAT.

Could not ascertain that there was any chill. In both cases the skin was hot.

SOMNOLENCE.

In one case a little drowsiness from the commencement.

DELIRIUM, CRY.

Very sharp cries, delirium, agatation, from the fifth day until death, in both cases.

EYES, EARS, NOSE.

Nothing special, save that one of the patients presented on the third day an alternate contraction and dilatation of the pupil, with convulsion of the eye-balls.

LENTICULAR SPOTS.

In one there were none; in the other they were discovered on the tenth and eleventh day.

LUDAMINA.

No sudamina.

CHILLS; HEAT.

None ascertained. In one case the skin was tolerably hot all the time; in the other, it was very hot until death.

PERSPIRATIONS.	PERSPIRATIONS.
No perspirations.	No perspirations.
PULSE.	PULSE.
In one small and feeble, from 120 to 132; in the other from 140 to 160.	In one, from 112 to 152; in the other from 120 to 144.
RESPIRATORY FUNCTIONS.	RESPIRATORY FUNCTIONS.
Cough from the outset in one case; in the other it preceded the commencement of the disease fifteen days. There was sub-crepitant râle in both cases.	A very frequent cough from the commencement, which persisted until death. Sibilant and sub-crepitant râles.
DURATION.	DURATION.
Twelve and eighteen days.	Nine and twenty-three days.
FORM.	FORM.
Ataxic.	Ataxic.
TERMINATION.	TERMINATION.
In death.	In death.
ANATOMICAL ALTERATIONS.	ANATOMICAL ALTERATIONS.
Very intense inflammation of the two last feet of the small intestine and of the entire large intestine in one case (redness, gangrened appearance, thickening, softening of the mucous membrane.) In the other inflammation over an extent of six inches above the valve, (redness, thickening, softening;) disseminated inflammation in the large intestine Peyer's plates were areolate in one case, but not so in the other.	Tumefaction, redness, and softening of the plates in one case. The plates ulcerated in the other.
Mesenteric ganglions healthy.	Swelling, redness and softening of the mesenteric ganglions.
Hepatization with gangrene of the lungs in one case, lobular pneumonia in the other.*	Lobular pneumonia in both cases.

From our comparison we see that, in some cases, it is impossible to distinguish typhoid fever from enteritis. This

* These two cases were published in the *Journal des Connaissances Medico Chirurgical.*

conclusion is very different from that arrived at by M. Louis for the adult, since in closing his comparison, this wise pathologist thus expresses himself: "Certainly the two classes of patients whose history we have just analyzed, were not affected with the same disease; for, to mention only the symptoms, their difference consists *much less in their intensity than in their number, which is considerable in the one and very limited in the other.*"

In comparing the varieties of enteritis with the different forms of typhoid fever, one is led to the conclusion (already given elsewhere) that in a large number of cases, in the youngest children, these two diseases, upon which there has been so much discussion, and which physicians have so much endeavoured to distinguish from each other, are closely allied and easily confounded. These considerations are not trifling, and should we think exercise some influence upon the therapeutics of the disease. The moment one is convinced that, in children from two to five years old, typhoid fever and enteritis resemble each other, he will avoid the employment of any means which, directed against dothinenteritis, may be capable, in being misapplied, of adding a new stimulus to an inflammatory disease and ultimately giving rise to serious symptoms.

When a typhoid enteritis is secondary, the diagnosis is not so difficult, for we then have the previous existence of an eruptive fever to guide us. Yet, if we have no information relative to the antecedents, we may very easily commit an error in our diagnosis. It is under such circumstances that a careful examination of the skin may be of great service. For if traces of desquamation, or any signs of a previous eruption can be found, this symptom alone will be sufficient to assure our diagnosis. This symptom however is not infallible; for we have frequently observed a general desquamation in persons affected wish typhoid fever, though this epidermal shedding, it is true, does not exist in the face, and shows itself only at a very advanced stage of the disease.

Pneumonia and broncho-pneumonia.—In our chapter on pneumonia, we stated that the bilious and typhoid forms were very rare. Yet, when the physician encounters cases of this kind, he may confound the pulmonary phlegmasia with a grave or very grave dothinenteritis, as typhoid pneumonia assumes the ataxic or adynamic form. Ascultation will ordinarily take away all doubts. When, however, one has not been called in at the commencement of the disease, the diagnosis is sometimes embarrassing. It may in fact happen that the child is affected with a typhoid affection complicated with

pneumonia, and that the time at which the complication was developed is not known. It will then be doubtful whether the case under consideration be one of typhoid pneumonia or a dothinenteritis complicated with pneumonia. A solution of this problem however would not be very difficult; for we would only have to await the time at which the pulmonary inflammation would be resolved,—which would take place between the seventh and eighth day—to see all the morbid symptoms disappear. If however the inflammation, in place of becoming resolved, increased in extent after the seventh day, the rose-colored spots and sudamina which, in typhoid fever, appear at this period, would establish the diagnosis.

A catarrhal broncho-pneumonia may be mistaken for that form of typhoid fever called the pectoral form. The distinction is certainly not very easily made when the case is seen sometime after the commencement of the disease, and it will be principally from the history of the case that one will be enabled to form a diagnosis; typhoid fever being nearly always a primary disease, and broncho-pneumonia a secondary one. The prevailing disease, the season, among the symptoms the rose-colored spots, the condition of the gastro-intestinal canal and the tongue will contribute to the diagnosis.

(TO BE CONTINUED.)

On Jerking Respiration (respiration saccadée.) By DR. A. BOURGADE. (Archives Générales, November, 1858.) *Brit. and For. Rev., April*, 1859.

Dr. Bourgade is of opinion, and we think justly, that the profession have not paid sufficient attention to interrupted or jerking respiration, as one of the earliest symptoms of pulmonary tubercle. He details nine cases, and states that he has observed others, in which jerking respiration heard at the apex of one lung was the first auscultatory phenomenon indicating the deposition of tubercle subsequently proved by more palpable symptoms, and in part confirmed by post-mortem examination. In discussing the rationale of its production, he quotes one post-mortem examination, which absolutely disproves the views of Messrs. Barth and Roger on this point. They hold that it is due to the presence of the adhesions which are so frequently met with at the apex of the lungs in phthisical subjects. In the case in question there were no adhesions at the apex or about the middle of the

lung in which the jerking respiration had been heard. It is to be remembered that the symptom is most commonly met with at a time when no evidence exists of preceding or accompanying pleurisy; the character of the sound and the structure of the parts in which it is found has always seemed to us to justify our attributing it to a partial constriction of the smaller bronchi owing to adjoining tubercles pressing upon them, the effort of inspiration causing the successive opening of the obstructed passage. We have met with cases in which spasm appeared to be the cause of the construction; but we hold with Dr. Bourgade that, in the great majority of cases, jerking respiration is a sign of tubercular deposit.

We would endorse the following summary of observations made by Dr. Bourgade. Respiration is jerking when the respiratory murmur presents three or four brief intervals, and the ausculting ear experiences the sensation of a certain difficulty in the expansion of the pulmonary tissue from the prolonged expiratory murmur. The respiratory murmur otherwise preserves its normal timbre, and the symptom is chiefly observed during inspiration; it occurs, but less frequently, during expiration. This change in the respiratory murmur does not persist beyond a limited period; a prolonged expiratory murmur follows, or, more rarely, progressive diminution of the respiratory sound. This fact probably explains the circumstances of the symptom having been overlooked by many hospital physicians, who, seeing phthisis chiefly in its more advanced forms, do not meet with jerking respiration here as frequently as in private or extra-hospital practice. Jerking respiration is commonly limited to the apices of the lungs, which we should explain by the greater resistance of the parieties of these parts favouring the pressure of tubercle upon the adjoining bronchial tubes. When the patient is made to breathe more fully, the symptom is not rendered more palpable, but commonly disappears altogether. Jerking respiration is not always continuous, but at times presents regular intermissions; it may occur at every second, third, or fourth inspiration, sometimes even at greater intervals. It is chiefly at its first appearance that it presents this character, but when well marked it is commonly continuous; like other auscultory phenomena, it may vary in strength and precision from day to day.

In concluding his paper, the author, though attributing much more importance to jerking respiration as an early sign of pulmonary tubercle, states he does not regard it as an

absolute evidence of the deposit having taken place; for this purpose he justly requires the presence of collateral, general, or local symptoms.

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On Intermittent Fever; the Result of the Observations made during several Epidemics. Dr. HEIDENHAIN in Marienwerder. (Archiv fur Pathologische Anatomie, Band xiv., Hefte 5 and 6.) *Brit. and For. Rev., April*, 1859.

This paper is one of interest, as bearing upon the question of the change of type of disease generally, as well as on account of the special matter to which the other devotes his attention. Marienwerder, a town of western Prussia, not far from the delta of the Vistula, is the seat of intermittent fever and its congeners; and Dr. Heidenhain, having been in practice in that town and its vicinity for twenty-eight yeas, gives us the results of his experience during that period. He commenced his practice in 1831, shortly after cholera had claimed its first victims in the district surrounding Marienwerder. This town and the immediate vicinity had been spared, but instead of cholera there was an epidemic of gastric or bilious febrile conditions, with a remittent, or, more frequently, intermittent type. Gastric, bilious, and intermittent fevers almost disappeared when the cholera actually invaded Marienwerder; but the cholera itself assumed an intermittent character of the tertain type in some cases. On the cessation of cholera in 1831, intermittents ceased entirely in Marienwerder, and did not occur in the epidemic form until 1849. Even in the August of 1844, when the valley at the foot of the heights upon which Marienwerder is built, was flooded by the Vistula, and after the subsidence of the waters the atmosphere was poisoned with effluvia of decomposing vegetable matter, intermittents did not make their appearance. During these eighteen years febrile intermittents only occurred sporadically; the paroxysm was not ushered in by a severe rigor, but merely by a passing sense of cold, and the other symptoms of fever were not strongly marked. These affections were cured by large doses of quinine. In the place of genuine intermittents, intermittent neuralgic affections made their frequent appearance; the nerves affected were almost exclusively the first and second division of the fifth pair; the patients were affected with severe hemicrania for three or more hours; the forehead, the orbital region, the cheek, half the nose and

lip were the seat of the affection; the face was pale, at times slightly swollen, the eye dull, the pulse small, no trace of fever, the hands cool; the type was quotidian or tertian. The paroxysms commonly occurred in the morning, without premonitory symptoms, and ceased almost as suddenly. Twice the author observed similar neuralgic affections in the nape of the neck, and once in the right tibial nerve; in the last instance the subject affected was a multipara, in whom the neuralgia took the place of the after pains immediately after confinement; the neuralgia lasted eight hours, then put on the tertian type. In addition to these neuralgic intermittents, the doctor observed a convulsive intermittent disease in children. It occurred between the years of two and nine, and the affection was at times difficult to distinguish from an apoplectic condition, unless reference was had to the intermittent type. Dr. Heidenhain's description is briefly this:—The child loses consciousness, the eyes are closed, the pupil enlarged and sluggish, the head hot, the facial muscles are distorted by convulsive movements, the lips quiver or are drawn on one side, permitting the passage of frothy saliva, at times tinged with blood; it is almost impossible to effect deglutition. The extremities are violently thrown about; breathing is short and intermittent; there are suffocative attacks; profuse perspiration breaks out over the body. After four, six, or eight hours the symptoms abate, and the child appears to wake up exhausted, but otherwise is comparative healthy.

In the autumn of 1849, with the cholera, all the forms of intermittent fever returned with greater frequency; but in 1850 and 1851 they increased still more; and in 1852, the bad year of the cholera, they were at their maximum, but did not as in 1851, disappear with the cholera, but retained the prominent form of disease until the autumn of 1856, since which time they have diminished. The paroxysms were not characterized by severe rigors, and the cold stage was uniformly short, compared with the length of the hot and sweating stages. The affection attacked all ranks and ages, and however favorable the circumstances and healthy the dwellings of the individuals. A characteristic feature during this period was the severe pain which, in the fever, often affected the lumbar and sacral regions, and the tenderness of the last cervical and first dorsal vertebræ. This "spinal irritation" disappeared on the curative effect of the treatment being manifested.

During the period last spoken of, in addition to the neuralgic affection, neuroses of the motor nerves have made their

appearance. Intermittent affections of the vaso-motor system of nerves have also been observed, such as intermittent ophthalmia with a textian type; the pain was unusally severe in these cases, and out of proportion to the severity of the inflammatory symptoms.

Enlargement of the spleen and anasarca are the chief sequelæ which the author has met with. Of the latter he says, that it used, previous to 1854, never to be associated with albuminuria, but that since that time the majority of cases exhibited albuminous urine; these cases commonly proved fatal.

With regard to the treatment of all these various conditions, Dr. Heidenhain recommends that no energetic treatment be adopted during the paroxsysms, not even in the convulsive intermittent affection of children. Quinine is his sheet anchor, given in large doses during the free intervals; he has found eight grains generally suffice for an adult, given about the middle of the free interval, or immediately after a paroxysm.

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Researches into the possibility of Recalling to Life Temporarily, Persons Dying of Disease. By Dr. Brown-Sequard. *(Journal de la Physiologie, Oct.*, 1858.*)*

A considerable number of cases of transfusion have satisfied Dr. Brown-Sequard that in mammiferæ dying of various diseases, and especially of peritonitis, life may be restored for some hours by the process, when the agony has all but ended in death; he concludes that in human beings who have already become unconscious, and in whom agony has commenced, the intellectual faculties, the senses, and speech, may be restored for some hours by the combined effect of transfusion, artificial respiration, and bleeding at the jugular. Dr. Brown-Sequard does not give us the result of his entire experience, but he finds that of eleven experiments made upon dogs, cats, and full-grown rabbits, four animals came to life entirely, for two, three and four hours, that three others recovered the circulation, respiration, and reflex action for one or two hours without restoration of the voluntary movements or sensibility, while in the four remaining animals there was no result, except a slight increase of the movement of the heart. The following is a brief snmmary of one experiment which the author gives in detail:

In October, 1851, a dog in whom the abdominal sympathetic had been divided was attacked with peritonitis and death was at hand. Voluntary and reflex movements had entirely ceased: there was no respiratory action, and the convulsions of death were limited to a few muscular tremors; the fæces and urine were expelled, the pupil was dilated, and no movement of the heart was perceptible. A silver T-shaped tube was now inserted into the left carotid of the dying dog, and connected with the carotid of another dog, which was attached firmly to a table. The healthy arterial blood at once passed to the head and heart of the dying animal. At the same time, the left jugular vein and one of the femoral veins of the dying dog were opened. The jugular almost immediately, and the femoral after twenty or thirty seconds, yielded some blood. The period of transfusion lasted two minutes, and ligatures were placed on the carotids of both animals. The jugular vein was left open four or five minutes, during which the heart was felt beating. Gradually the pulse was restored as the blood flowed from the jugular. Artificial respiration was then had recourse to, and continued for half an hour. Eight minutes from the commencement of the insufflation, the cornea became sensitive, and soon after, respiratory movements were perceptible. After twenty minutes, the animal executed voluntary movements, and on leaving off insufflation, respiration continued regularly. The return to life was complete as regards all the chief functions of animal and organic life. Though feeble, the animal raised himself on his fore-feet and wagged his tail when caressed. The dog lived eleven hours and a half after the transaction.

EDITORIAL AND MISCELLANEOUS.

AMERICAN MEDICAL ASSOCIATION.

LOUISVILLE, May 3, 1859.

The Twelfth Annual Meeting of the Association met at eleven o'clock, a.m., in Mozart Hall, the President, Dr. Harvey Lindsly, of the District of Columbia, in the chair, supported by Drs. W. L. Sutton, of Kentucky, Thomas O. Edwards, of Iowa, Josiah Crosby, of Massachusetts, and W. C. Warren, of North Carolina, as Vice-Presidents, with Drs. Alexander J. Semmes, of the District of Columbia, and S. M. Bemiss, of Kentucky, acting as Secretaries. Dr. Caspar Wister, of Pennsylvania, Treasurer, was also in attendance.

The President introduced the Rev. Mr. Robinson, of Louisville, who opened the proceedings with prayer.

Dr. Robert J. Breckinridge, chairman of the Committee of Arrangements, then welcomed the delegates to the city.

Prof. Joshua B. Flint, of Louisville, accompanied by Drs. Sutton, Chipley, Spilman, and Snead, then came forward, and in behalf of the "State Medical Society of Kentucky," bid the Association welcome to the medical jurisdiction of Kentucky.

The Secretary, Dr. Bemiss, then called the roll of members of the Association.

The President now announced a recess of fifteen minutes, to enable the various State delegations to choose their members for the Committee on Nominations.

Upon call to order, the following members were reported as the Nominating Committee:

New Hampshire, Dixi Crosby; Massachusetts, Solomon D. Townsend; Rhode Island, J. H. Eldridge; New York, D. M.

Reese; New Jersey, A. N. Dougherty; Pennsylvania, R. K. Smith; Delaware, H. F. Askew; Maryland, G. W. Lawrence; District of Columbia, Cornelius Boyle; Virginia, L. S. Joynes; North Carolina, Edward Warren, Jr.; South Carolina, J. McF. Gaston; Georgia. John W. Jones; Alabama, J. B. Coons; Louisiana, S. O. Scruggs; Tennessee, E. B. Haskins; Kentucky, D. D. Thomson; Ohio, George Fries; Indiana, J. H. Brower; Michigan, William Brodie; Illinois, C. Goodbrake; Missouri, M. L. Linton; Iowa, D. L. McGugin; Wisonsin, C. B. Chapman; Army, C. S. Tripler.

The President then appointed the following gentlemen a Committee on Voluntary Essays: Drs. L. P. Yandell, of Kentucky, James Bryan, of Philadelphia, and C. G. Comegys, of Ohio.

Dr. R. J. Breckinridge, from the Committee of Arrangements, announced the hours of business from 9 a.m. to 1 p.m, and 3 p.m. until such an hour as the Convention should adjourn upon resolution; which arrangement was adopted.

Dr. Harvey Lindsly, the President of the Association, then read his retiring address, which was listened to with marked attention, and was an eloquent tribute to the dignity of the medical profession and the importance of its improvements.

After he had concluded, Dr. L. A. Smith, of New Jersey, moved that the thanks of the Association be tendered to the President for his able and eloquent address, and it was ordered to be placed in the hands of the appropriate committee for publication among the proceedings of the meeting.

Dr. Caspar Wister, chairman of the Committee of Publication, read the annual report, and on motion of Dr. Sayre, of New York, the following resolutions appended to it were unanimously adopted:

"*Resolved*, That hereafter every paper intended for publication in the Transactions must not only be placed in the hands of the Committee of Publication by the first of June, but it must also be so prepared as to require no material alteration or addition at the hands of the author.

"*Resolved*, That authors of papers be required to return their proofs within two weeks after their reception, otherwise they will be passed over and omitted from the volume."

Adjourned until 3 o'clock, p.m.

AFTERNOON SESSION.—Dr. W. L. Sutton, one of the Vice-Presidents, took the chair, in the absence of the President.

Dr. D. Meredith Reese, of New York, chairman of the Committee on Nominations, reported the following officers for the ensuing year:

President.—Henry Miller, of Kentucky.

Vice-Presidents.—H. F. Askew, Delaware; C. S. Tripler, U. S. Army; L. A. Smith, New Jersey; Calvin West, Indiana.

Treasurer.—Caspar Wister, Pennsylvania.

Secretary.—S. M. Bemiss, Kentucky.

Dr. Sayre moved the adoption of the report, which was unanimously agreed to.

Dr. Brainard, of Illinois, moved the appointment of a committee to conduct the newly-appointed officers to their respective chairs. The Acting President selected Drs. Brainard, of Illinois, Mattingly, of Kentucky, Sutton, of Indiana, McDowell, of Missouri, and R. J. Breckinridge, of Kentucky, and they accordingly performed the duties assigned to them.

The newly-elected President, on taking the chair, then addressed the Convention.

Dr. R. J. Breckinridge moved that the thanks of the Association be tendered to the retiring officers for the faithful and assiduous manner in which they have conducted the business committed to their charge, which was unanimously adopted.

Several names were now offered as candidates for membership, by invitation, when

Dr. T. O. Edwards moved that no person be admitted as a member by invitation, unless his good standing in the profession be vouched for by some member of the Association, which was adopted.

Dr. Eve moved a reconsideration of the motion by Dr. Edwards, which was carried; and upon motion of Dr. Edwards, all applications for membership, by invitation, were referred to the Committee of Arrangements and Credentials.

Dr. J. B. Lindsly, of Tennessee, offered the following:

"*Resolved*, That a committee of three be appointed by the chair to inquire into and report upon the propriety of

dividing the Association into sections, for the purpose of performing such parts of its scientific labors as may relate to particular branches of medicine and surgery."

Dr. Brodie moved its reference to the Nominating Committee.

Dr. Brainard explained at some length the object of the resolution of inquiry, and urged its adoption, as the means of giving more effect and usefulness to the proceedings of the Association, the reports of which had heretofore gone out unmatured in consequence of the want of concentrated action.

A motion by Dr. Sayre to lay the motion on the table was negatived, and the motion of Dr. Lindsly was then adopted.

The chair appointed as the committee: Dr. Lindsly, of Tennessee; Dr. Brainard, of Illinois; Dr. G. C. Blackman, of Ohio.

Dr. Davis moved that no person be permitted to speak more than twice on the same subject, or more than ten minutes at one time, except by consent of the Association, which was adopted.

The Standing Committee on Prize Essays was called on for their report, but without a response. This was also the case with the Committee on Medical Education. The Committee on Medical Literature had no report to present.

A letter from Dr. J. G. F. Holston, of Ohio, chairman of the Special Committee on the Microscope, was read, reporting progress, and begging a continuance for more extended investigation, which was referred to the Committee on Nominations.

A letter from Dr. Stephen Smith, of New York, from the Special Committee on Medical Jurisprudence, had the same reference.

The Special Committee on Quarantine was not ready to report.

Dr. Mattingly, of Kentucky, from the Special Committee on Diseases and Mortality of Boarding Schools, asked a continuance until next year, in order to obtain further information requisite to the full investigation of the important subject. This was referred to the Committee on Nominations.

The Special Committee on Surgical Operations for the Relief of Defective Vision, on Milk Sickness, and on the Blood Corpuscle, had the same reference.

A report from the Committee on Medical Ethics was read, laid on the table, and such portion of it as related to the action of the Dubuque Medical Society, in the case of an expelled member, was, on motion of Dr. T. O. Edwards, made the special order for to-morrow, at 12 o'clock, m.

Continuances were asked by the Committees on the Pons Varolii, Medulla Oblongata, and Spinal Marrow—their Pathology and Therapeutics; on American Medical Necrology; on the Hygienic Relations of Air, Food, and Water, the Natural and Artificial Causes of their Impurity, and the best Methods by which they can be made most effectually to contribute to the Public Health; on the Effect of the Virus of Rattlesnakes, &c., when introduced into the System of the Mammalia; on the Climate of the Pacific Coast, and its Modifying Influences upon Inflammatory Action and Diseases generally; on the Constitutional Origin of Local Diseases, and the Local Origin of Constitutional Diseases; on the Physiological Effects of the Hydrocarbons; on Epilepsy; on the Causes of the Impulse of the Heart, and the Agencies which influence it in Health and Disease; and on the best Substitute for Cinchona, and its Preparations, in the Treatment of Intermittent Fever, &c.; all of which were referred to the Committee on Nominations.

The Special Committee on Government Meteorological Reports made a report, written by Dr. R. H. Coolidge, of the U. S. Army, but read by Dr. Paul F. Eve, of Tennessee, which was referred to the Committee on Publication.

The Committee, appointed in May, 1857, on Criminal Abortion, submitted a report, written by Dr. Storer, of Boston, which was read by Dr. Blatchford, of New York, and referred to the Committee on Publication. The following resolutions appended to this report were unanimously adopted:

"*Resolved*, That while physicians have long been united in condemning the act of producing abortion at every period of gestation, except as necessary for preserving the life of

either mother or child, it has become the duty of this Association, in view of the prevalence and increasing frequency of the crime, publicly to enter an earnest and solemn protest against such unwarrantable destruction of human life.

"*Resolved*, That in pursuance of the grand and noble calling we profess—the saving of human lives—and of the sacred responsibilities thereby devolving upon us, the Association present this subject to the attention of the several Legislative Assemblies of the Union, with the prayer that the laws by which the crime of procuring abortion is attempted to be controlled may be revised, and that such other action may be taken in the premises as they, in their wisdom, may deem necessary.

"*Resolved*, That the Association request the zealous co-operation of the various State Medical Societies in pressing the subject upon the Legislatures of their respective States, and that the President and Secretaries of the Association are hereby authorized to carry out by memorials these resolutions."

The Convention then adjourned till the following morning at 9 o'clock.

WEDNESDAY, May 4, 1859.

SECOND DAY.—The President, Dr. Miller, called the Association to order at 9 o'clock.

Dr. D. Meredith Reese, chairman of the Committee on Nominations, called attention to the fact that the committee could not act definitely until the place for next year's meeting should be designated. He stated also, that the Medical State Society of Connecticut had requested that an amendment to the Constitution, proposed two years since, should be taken from the table, relative to the time of meeting.

It was moved by Dr. Blatchford, and seconded by Dr. Sayre, that the amendment to the third article of the Constitution be taken up, which proposes to add after the words "first Tuesday in May," the words "or first Tuesday in June;" and after the words "shall be determined," the words "with the time of meeting."

The amendment was adopted by a constitutional vote.

Dr. D. M. Reese also stated that the Connecticut brethren had extended a kindly invitation to the Association, through Dr. C. Hooker, to hold its next meeting at New Haven;

which invitation was referred to the Committee on Nominations.

Dr. Reese also called attention to the necessity of some radical change in the mode of appointing committees to prepare treatises on scientific subjects to be reported at the annual meetings. It had been seen that, on the preceding day, a large majority of the committees made no reports, and did not even see proper to send in any communication explanatory of delay. The difficulty heretofore has originated in the mode of selection adopted by the Nominating Committee. It has been customary for gentlemen to hand in their names and the proposed subjects on slips of paper, and the committee, without further investigation, have so published in the annual reports. Thus it has happened that appointments have been most injudiciously made, and gentlemen, to whom a special duty has been assigned, have been found to know less of that than any other subject. He therefore hoped that no committee of last year would be reappointed or continued from which no report had been had, and no communication received.

On motion, the Nominating Committe was unanimously instructed to act upon the suggestions of the chairman, who also stated that there should be some definite expression of disapprobation as to the course of those gentlemen who had volunteered essays, and had their names reported in the newspapers and spread over the land, and then paid no further attention to the matter.

Dr. Flint, from the Committee on Prize Essays, begged leave to report that they received four dissertations in time for a careful and thorough examination, and two others, quite voluminous, only two days before the meeting of the Association. The latter we have felt constrained to exclude altogether from the competition of the present year, on account of the absolute impossibility of reading them with a critical purpose and effect. The others have been carefully examined by all the surviving members of the committee—one estimable associate, Dr. Evans, having been called from all his earthly labors before the active duties of the committee began.

More than one of the four essays we examined exhibited much labor, and a commendable scholarship in their preparation—are voluminous, and in some respects very meritorious papers; but, in the unanimous judgment of the committee, none of them possesses the degree and species of merit which should entitle its author to the Association prize.

The committee beg leave furthermore to report, that in their opinion, and as the suggestion of their own recent experience, the Association should determine in more precise and formal manner than has yet been done, the terms and conditions of competition and of success in the contest for prizes, for the government alike of contestants and the committee of adjudication, and that a committee be now appointed to consider and report upon the subject.

Dr. Gordan, chairman of Committee on Etiology and Pathogy of Cholera, made a partial report, and asked continuance of time.

On motion, the report was accepted, and referred to the Committee on Publication, and the petition for continuance referred to nominating committee.

Dr. J. B. Lindsly, chairman of the committee appointed to inquire into the propriety of dividing the Association into sections, for the better performance of its work in considering the various branches of medicine and surgery, recommended the adoption of such a plan, as being indispensably necessary to making this body a working scientific association. They do not deem it necessary to enter into any argument in favor of this plan, it being the one already universally adopted by similar bodies. They would simply recommend, for the present, a division into the following sections, as being most suitable to facilitate the transaction of business, viz:

1. Anatomy and Physiology. 2. Chemistry and Materia Medica. 3. Practical Medicine and Obstetrics. 4. Surgery.

The committee do not propose that this subdivision of labor shall in any manner interfere with the regular business of the Association as now conducted; but only that after having assembled each day in general session, each section shall meet separately for the purpose of hearing and discussing papers on such subjects as properly belong to them, and they

therefore recommend that the Committtee of Arrangements for the ensuing year be requested to provide suitable accommodations for the service of these sections, and that each of said sections shall be authorized to make such arrangements as may be required for the proper transaction of its business.

This report was considered and adopted, after a very able speech in its support by Dr. Davis.

Dr. J. W. Singleton, of Ky., moved the suspension of the rules for the introduction of the following:

"*Resolved*, That in the death of Dr. A. Evans, of Kentucky, the Association has lost one of its most manly and efficient members, and society a friend and benefactor."

The Resolution was unanimously adopted.

Dr. W. L. Sutton, under the resolution appointing a committee on registration of births, marriages, &c., proposed a plan of general action, an abstract of which he read on motion of Dr. Gibbes, of S. C., and on motion of Dr. N. P. Yandell, the subject was referred to a committee to report during the present session.

Drs. Sutton, J. B. Lindsly, W. R. Gibbs, James Bryan. Z. Pitcher, and G. C. Shattuck were appointed such committee.

Dr. Blatchford stated that he had received from Dr. Willard, Secretary of the New York State Medical Society, fifty volumes of their Transactions for 1858, for distribution to the medical press, the medical colleges, and all the medical societies of the South, and sent with a request for an interchange of civilities. Gentlemen present can be supplied by application to Dr. Bemiss; and if the number sent be not sufficient for the supply, they will be cheerfully forwarded to any gentleman by applying to the Secretary, Dr. S. D. Willard, Albany, N. Y., the postage which is twenty-two cents, being included in the application.

A report from Dr. Thomas Logan, of California, on Medical Topography and Epidemics, was received, and referred to the Committee on Publication.

The chairman of the Committee on Voluntary Essays stated that he had received a paper on a case of extra-uterine fœtation from Dr. Enos Hoyt, of Transylvania, Mass., and another on a case of accidental poisoning by strychnine from

Dr. Doughlas Bly, of Rochester, N. Y. He also presented a very voluminous paper entitled "Observations on some of the Changes of the Solids and Fluids in Malarial Fever, by Joseph Jones, Professor of Medical Chemistry, in the Medical College of Georgia, at Augusta." By request, Prof. Jones gave a verbal abstract of his paper and an exposition of his theory, and on motion of Dr. D. W. Yandellthe communication was referred to the Committee on Publication.

Dr. D. W. Yandell announced that the following railroad companies had agreed to pass delegates to this Convention over their roads at half price: Pittsburg, Fort Wayne, and Chicago; Pennsylvania Central; Jeffersonville; New Albany and Salem; Louisville and Nashville, and Cleveland and Pittsburg.

On motion, a vote of thanks was tendered to these companies for their liberality.

Dr. J. B. Flint offered the following resolution:

"*Whereas*, Our brethren of Great Britain are engaged in erecting a monument to the memory of John Hunter, whose invaluable services in behalf of Physiology and Surgery are recognized and honored, as well on this side the Atlantic as in Europe; and whereas, this Association, as the represetatives of American Medicine, would rejoice in some suitable manner to participate in so grateful a testimonial of gratitude and respect: Therefore,

"*Resolved*, That a committee of three be appointed to consider in what manner this participation can best be effected, so as to be acceptable to our British brethren, and consistent with our means and opportunities of action, with instructions to report at the next annual meeting."

The resolution was adopted, and Drs. Flint, Bowditch, and Shattuck appointed as the committee.

Dr. Harvey Lindsly offered the following:

"*Whereas*, Parliamentary rules of order are numerous, complicated, sometimes obscure, and often inapplicable to such a body as the American Medical Association; and whereas, from the nature of the pursuits of medical men, they cannot be familiar with these rules: Therefore,

"*Resolved*, That a select committee of three members be appointed to prepare a system of rules for the government of this Association, as few in number, as concise and as per-

spicuous as possible, to be reported at the next annual meeting."

This resolution was adopted, and Drs. Lindsly, Comegys, and Blatchford appointed as the committee.

The paper of Dr. Bly, on Accidental Poisoning by Strychnine, was read by its author; and, as individual cases are not reported in the Transactions of the Association, (except as illustrative of principles,) thanks were returned for the communication, with a request that it be published in some Medical journal.

An invitation from Grand Master Morris, of the Masons, was received, urging medical brethren to attend the Masonic Convention now in session in this city.

The Nominating Committee made the following report:

The next annual meeting to take place at New Haven, on the first Tuesday of June, 1860. Dr. Eli Ives is elected Junior Secretary.

Committee of Arrangements—Drs. Chas. Hooker, Stephen G. Hubbard, and Benjamin Silliman, jr., with power to add to their numbers.

Committee on Prize Essays—Drs. Worthington Hooker, Conn.; G. C. Shttuck, Mass.; Ursher Parsons, R. I.; P. B. Jewett, Conn.; and John Knight, Conn.

Committee on Publication—Drs. F. G. Smith, Philadelphia, Pa.; Wister, do.; Bemiss, Louisville, Ky.; Ives, New Haven, Conn.; Hollingsworth, Philadelphia, Pa.; and Askew, Wilmington, Del.

Committee on Medical Literature—Drs. Henry Campbell, Ga.; D. F. Wright, Tenn.: O. Wendell Holmes, Mass.; S. C. Armor, Ohio; and W. H. Byford, Ill.

Committee on Medical Education—Dr. D. M. Reese, N. Y.; W. R. Bowling, Tenn.; Charles Fishback, Ind.; John Bell, Penn.; Z. Pitcher, Mich.

The following Special Committees were appointed:

On Morbus Coxarius, and Surgical Pathology of Articular Inflammation—Dr. Lewis A. Sayre, of New York.

On the Surgical Treatment of Strictures of the Urethra—Dr. James Bryan, of Philadelphia.

On Drainage and Sewerage of Large Cities, their Influ-

ence on Public Health—Drs. A. J. Semmes, D. C., Chairman; Cornelius Boyle, and G. M. Dove.

On Puerperal Tetanus, its Statistics, Pathology, and Treatment—Dr. D. L. McGugin, of Keokuk, Iowa.

On Hospital Epidemics—Dr. R. K. Smith, of Philadelphia.

On Puerperal Fever—Dr. S. N. Green, of Stilesville, Ind.

On Anæmia and Chlorosis—Dr. H. P. Ayres, of Fort Wayne, Ind.

On Veratrum Viride—Dr. James B. McCaw, of Richmond, Va.

On Alcohol—Its Therapeutical Effects—Dr. J. R. W. Dunbar, of Baltimore, Md.

On Meteorology—Dr. J. G. Westmoreland, Atlanta, Geo.

On Milk Sickness—Dr. Robert Thompson, Columbus, Ohio.

On Manifetations of Disease of Nerve Centres—Dr. C. B. Chapman, Wisconsin.

On the Medical Topography of Iowa—Dr. T. O. Edwards, Iowa.

On Microscopic Observations on Cancer Cells—Dr. Geo. D. Norris, New Market, Ala.

On the Philosophy of Practical Medicine—Dr. James Graham, Cincinnati, Ohio.

On some of the Peculiarities of the North Pacific, and their Relations to Climate—Wm. H. Doughty, Ga.

The following Special Committees were continued or altered:

On Microscope—John C. Dalton, jr., N. Y.; David Hutchinson, Ind.; A. R. Stout, Cal.; Calvin Ellis, Mass.; Christopher Johnson, Maryland.

On Diseases and Mortality of Boarding Schools—Dr. C. Mattingly, Ky., and Dixi Crosby, N. H.

On the Various Surgical Operations for the Relief of Defective Vision—Drs. M. A. Pallen, Mo.; T. J. Cogley, Ind., and W. Hunt, Penn.

On the Blood Corpuscle—Dr. A. Sagar, Michigan.

On American Medical Necrology—Dr. C. C. Cox, Maryland.

On the Hygienic Relations of Air, Food, and Water, the natural and artificial causes of their impurity, and the best methods by which they can be made most effectually to con-

tribute to the public health—Dr. C. C. Cox, Maryland.

On the Effect of Virus of Rattlesnakes, &c., when introduced into the system of Mammalia—Dr. A. S. Payne, Virginia.

On the Climate of the Pacific Coast, and its modifying Influences upon Inflammatory Action and Diseases generally—Dr. O. Harvey, California.

On the Constitutional Origin of Local Diseases and the Local Origin of Constitutional Diseases--Drs. W. H. McKee, North Carolina, and C. F. Heywood, New York.

On motion of Dr. Brodie, Dr. A. J. Semmes was requested to serve as Secretary *pro. tem.* during the remainder of the session.

The Association took up the special order, being the report on the Medical Ethics, to which had been referred the action of the Dubuque Medical Society, which, after debate, was laid over until 12 o'clock to-morrow.

On motion of Dr. E. F. Campbell, a section of meterology, medical topography, and epidemic disease, and of medical jurisprudence and hygiene, was added to those already adopted by this Association.

The Association then proceeded to consider and act upon amendment to the Constitution proposed at the last annual meeting, and laid over under the rules. The following amendments was adopted:

"*Resolved*, That the Constitution of this Association be so amended as to provide that no individual who shall be under sentence of expulsion or suspension from any State or local medical society of which he may have been a member, shall be received as a delegate to this body, or be allowed any of the privileges of a member, until he shall have been relieved from said sentence by such State or local society."

The next amendment, lying over from last year, was the proposition of Dr. Kyle, of Ohio:

That the Constitution of the Association be so amended as to prohibit the admission as a delegate, or the recognition as a member, of any person who is not a graduate of some respectable medical college.

This amendment was rejected, but, on the question of re-

consideration, a long and animated debate ensued. Without arriving at a vote, the Association adjourned.

AFTERNOON SESSION.—The Association was was called to order at 3 P. M., Dr. M. F. Askew in the chair. The discussion on the amendment under consideration at the time of adjournment was renewed.

Dr. Kincaid moved a further amendment, to insert the word "hereafter" after "prohibiting."

The chair ruled the amendment out of order at the present stage, or until the Association decide upon the question of reconsideration.

After a long discussion, Dr. Davis, of Ind., moved to lay the motion to reconsider on the table, which was carried, 97 yeas, nays not counted; so the amendment stands registered.

The next proposed amendment to the Constitution was that suggested by the New Jersey Medical Society, asking for such changes as would establish a Board of Censors in every judicial district of the Supreme Court, who should examine and grant diplomas to all proper members of the Association.

This was temporarily laid on the table, for Dr. Crosby to offer a report of the Medical Teachers' Convention, which met on Monday last. He strongly recommended a committee from this body to confer with the Teachers' Committee, and felt great confidence that something beneficial to medical education would be the effect of such conference.

Dr. Comegys moved the appointment of a committee of five to confer with the committee of Medical Teachers and report at the next annual meeting, provided that no medical teacher be selected on the part of this Association.

Dr. T. M. Blatchford, of New York, offered as a substitute the following preamble and resolution:

"*Whereas*, Of all the subjects which can engage our attention in our associate capacity, that of Medical Education is paramount; and *Whereas*, harmony of action is essential to success in establishing definite qualifications entitling to admission in our ranks; and *Whereas*, nothing can be gained by hasty action in a matter so vital to our very existence, as a permanent Medical Institution: Therefore,

"*Resolved*, That further action be suspended for the present upon the subject of the resolutions offered at the last meeting of the Association by the chairman of the Special Committee on Medical Education, and that a committee, consisting of S. W. Butler, of Pennsylvania, L. A. Smith, of New Jersey, Dixi Crosby, of New Hampshire, C. A. Pope, of Mo., and T. Buckler, of Maryland, shall be appointed to confer with the committee appointed at the meeting of Medical Teachers, to report some plan for action at the next meeting of the Association."

This amendment was lost, and the original resolution adopted.

The resolutions from the New Jersey Medical Society were then taken from the table and referred to the Committee of Conference.

Dr. Davis offered a resolution instructing the same committee to confer with the State Medical Societies for the purpose of procuring more decisive and uniform action throughout the profession in carrying into effect the standard of preliminary education adopted by this Association, at its organization in 1847. This was carried.

Dr. Gibbes, from the committee to examine into a plan of uniform registration of births, marriages, and deaths, offered the following report:

They have given the same a careful consideration, and they unanimously recommend that the report be adopted, and referred to the Committee on Publication.

They also recommend that the same committee be continued, with instructions to add to the report, in time for publication in the ensuing volume of Transactions, a form of registration law which may be likely to answer the requirements of the several States.

Dr. Sayre, of New-York, offered the following:

"*Whereas*, The medical profession at large have an interest in the character and qualifications of those who are to be admitted as their associates in the profession: Therefore,

"*Resolved*, That each State Medical Society be requested to appoint annually two delegates for each college in that State, whose duty it shall be to attend the examinations of all candidates for graduation; and that the colleges be requested to permit such delegates to participate in the examination, and vote on the qualifications of all such candidates."

This was, on motion, referred to the Committee on Conference.

The paper of Dr. Jones, presented at the morning session, was taken from the Committee on Publication and referred to the Committee on Prize Essays.

Dr. Eve moved to record the name of Dr. Benj. W. Dudley, as a permanent member, which was adopted by a unanimous vote, the delegates all rising to their feet in token of respect.

Adjourned till to-morrow morning, at 9 o'clock.

THURSDAY, May 5, 1859.

The President called the Association to order at nine o'clock, and the reading of the minutes of the preceding day was dispensed with.

The first business in order was an amendment to the Constitution, laid over from last year, and proposed by Dr. T. L. Mason, of New-York, to insert in the first line of the second paragraph of Article II, after the words "shall receive the appointment from," the words "any medical society permanently organized in accordance with the laws regulating the practice of physic and surgery in the State in which they are situated, and consisting of physicians and surgeons regularly authorized to practice their profession." Also, to add to the sixth paragraph of the same article the words "but each permanent member of the first class designated in this plan of organization shall be entitled to a seat in this Association, on his presenting to this body a certificate of his good standing, signed by the Secretary of the Society to which he may belong at the time of each annual meeting of this body."

Dr. Lyndon A. Smith, of New Jersey, said amendments to the Constitution should be adopted with care; and though, perhaps, that now proposed might be desirable, still, as Dr. Mason, who had proposed it, was not present to explain his views, he moved that the subject be laid over until next year. This suggestion was adopted.

Another constitutional amendment, proposed by Dr. Henry Hartshorne, of Pennsylvania, and laid over from last year

under the rules, provides to add to Article II. the words: "No one expelled from this Association shall at any time thereafter be received as a delegate or member, unless by a three-fourths vote of the members present at the meeting to which he is sent, or at which he is proposed."

This amendment was adopted.

Another amendment, proposed by J. Berrien Lindsly, of Tennessee, was called up, to omit in Article II. the words "medical colleges, hospitals, lunatic asylums, and other permanently organized medical institutions in good standing in the United States;" and also to omit the words: "The faculty of every regularly constituted medical college or chartered school of medicine shall have the privilege of sending two delegates. The professional staff of every chartered or municipal hospital containing a hundred inmates or more shall have the privilege of sending two delegates; and every other permanently organized medical institution of good standing shall have the privilege of sending one delegate."

This was laid on the table until the next annual meeting.

An invitation was received from Mons. Groux, requesting the delegates to meet him at the hall of the University at noon, to witness experiments on his congenital fissure of the sternum, which was deferred until four o'clock this afternoon, as the Association had previously accepted the hospitality of Mr. and Mrs. Robert J. Ward at the former hour.

Dr. McDermott submitted the following resolutions:

"*Whereas*, A vast proportion of the disease and misery that afflict our race is caused by the excessive use of intoxicating liquors; and whereas, in the opinion of this Association, the evils of intoxication can be most effectually remedied by the establishment of Inebriate Asylums, wherein the victims of intemperance may be subjected to such restraints and treatment as shall effect a thorough reformation in their habits: Therefore,

"*Resolved*, That this Association recommend the establishment of inebriate asylums in the various States of the Union.

"*Resolved*, That the State and County Medical Societies, and all members of the medical profession, be requested to unite in diffusing among the people a better knowledge and appreciation of the benificent purposes and important bene-

fits that would be conferred upon society by the establishment of such asylums throughout the various sections of the country."

This resolution was referred to the mover, as a Special Committee, with a request that he would report thereon at the next meeting of the Association.

Dr. Shattuck offered the following, which was adopted:

"*Resolved*, That the committee appointed in May, 1857, on Criminal Abortion, be requested to continue their labors, and especially to take all measures necessary to carry into effect the resolutions reported by them on the first day of the meeting."

Dr. Yandell, from the Committee on Voluntary Essays, made a further report that a communication had been received from Dr. Langer, of Iowa, on Subcutaneous Injections as Remedials, which, on motion, the author read.

The essay was referred to the writer as a special committee, with the request that he would report further at the next annual meeting of the Associations, and continue his investigations.

Invitations to visit the Insane Asylum and the Library and Museum of Transylvania University were received.

The President appointed as the Committee of Conference, to meet the committee from the Teacher's Convention, the following gentlemen: Drs. Blatchford, Troy, N. Y.; Condie, Philadelphia, Pa.; Bozeman, Montgomery, Ala.; Brodie, Detroit, Michigan; and Sneed, Frankfort, Ky.

Dr. D. Meredith Reese. from the Nominating Committee, made the following final report:

Special Committees continued.

On Quarantine.—Drs. D. D. Clark, Penn.; Snow, R. I.; Jewell, Penn.; Fenner, La.; and Houck, Md.

On Medical Ethics.—Drs. Schuck, Penn.; Murphy, Ohio; Linton, Mo.; Powell, Ga.; Eve, Tenn.

On Tracheotomy in Membranous Croup.—Dr. A. V. Dougherty, N. J.

On Mercurial Fumigation in Syphilis.—Dr. D. W. Yandell, Louisville, Ky.

On the Improvements in the Science and Art of Surgery,

made during the last Half Century.—Dr. Joseph McDowell, St. Louis, Mo.

On the Cause and Increase of Crime, and its Mode of Punishment.—Dr. W. C. Sneed, Frankfort, Ky.

On the Education of Imbecile and Idiotic Children.—Dr. H. P. P. Ayres, Fort Wayne, Ind.

On the Uses and Abuses of the Speculum Uteri.—Dr. C. H. Spillman, of Kentucky.

On the Topography of Vermont.—Dr. Perkins, Vermont.

On the Pons Varolii, &c.—Dr. S. B. Richardson, of Kentucky, and Dr. Fishback, of Indiana.

On the Physiological Effects of the Hydro-Carbons.—Dr. F. W. White, of Illinois.

On the Effect of the Perineal Operations for Urinary Calculi upon Procreation in the Male.—Dr. J. S. White, of Tennessee.

The paper from Dr. Ellis, of Massachusetts, on the subject, "Does the microscope enable us to make a positive diagnosis of cancer, and what, if any, are the sources of error?" was referred to the Special Committee on the Microscope, of which Dr. Dalton is chairman.

On motion, the report was adopted as a whole.

Honorary resolutions were passed to the memory of the following members of the Association, deceased: Dr. W. W. Boling, of Alabama; Dr. Thomas D. Mütter, of Pennsylvania; Dr. P. C. Gaillard, of South Carolina; Dr. Jabez G. Goble, of New Jersey; Dr. John K. Mitchell, of Penn.

Dr. R. K. Smith, of Philadelphia, submitted the following:

"*Resolved*, That the death of Dr. John K. Mitchell, one of the members of this Association, has been to this body a loss keenly felt by every man who knew him. His eminence as a teacher, his varied acquirements in every department of learning, and his generous social qualities in every relation, endeared him to every member of the profession who had the pleasure of his personal acquaintance.

"*Resolved*, That the family be notified of the action of this Association."

Other more formal resolutions were offered, and feeling eulogies pronounced.

Dr. Sayre offered the following, which were adopted by acclamation:

"*Resolved*, That the thanks of the American Medical Association are eminently due and are hereby presented to the citizens of Louisville, Ky., for the princely hospitality publicly and privately extended to the members of this body during its present session.

"*Resolved*, That to the Committee of Arrangements and the profession of Louisville, generally, our thanks are due for their kind and assiduous attention to the Association, and for the hearty welcome with which they have greeted our convention in their flourishing city.

After the transaction of some other unimportant routine business,

On motion of Dr. Davis, the Association adjourned, to meet at New Haven, on the first Tuesday in June, 1860.

FREE MEDICAL ATTENDANCE.

In a recent number of the London Lancet my attention was attracted by an article on the "*Habit of prescribing for the sick without charging.*" As a practice this is, I am glad to find, condemned by the author of that paper, and having observed that it exists to a great extent among retired physicians of our own country, I deem it well to call the attention of the profession thereto.

Let it be at once understood, however, that I do not in these remarks refer in any manner to those cases in which the service is rendered to some unfortunate one, who, from his pecuniary condition, cannot afford a fee without depriving himself or his family of some necessary, or even some comfort. To administer to suffering humanity is the physician's calling; to do all in his power to relieve distress is his duty; but to afford comfort and assuage pain in cases in which the pleasure of doing good is the only return that can be expected, to labor hard for and minister to the unfortunate who cannot and never can pay, is one of the noblest privileges of our profession. The case is far different, however,

where medical attendance is required by those well to do in the world, and often far better able to pay for services rendered than he who serves can afford to lose the just return for his labors. It is in such cases as these,—by no means uncommon ones,—that by omitting to charge, the physician injures himself, his profession, and his patient; nay, virtually violates his oaths at graduating, and should hence forfeit his diploma.

First, this course is injurious to all parties. To the sick, because advice so obtained is, with all respect to those giving it, in the majority of cases, not as good as might otherwise be procured. Medicine is in its practice a science, and as such, must from day to day progress. This must, like others, be influenced by the improved lights brought to bear upon it, and affected by the constant expansion of sister sciences. As long as a physician continues regularly to practice, *conscience, interest, pride*, every generous or selfish impulse, prompts him to keep up with the march of the times; to read, study, and observe, so as not to be found wanting in his vocation. Very different, often, are his feelings when he has withdrawn from, or refused to enter upon, general practice. Some other business is adopted, and its demands on his time and intellect asserted and granted, while medicine and its study are laid aside and forgotten, or only thought of when, through kind feeling, it is thus unwillingly resumed. And now, if I may use the phrase, it is resumed where it was left off, several, or it may be many years before,—pursued timidly, and in many cases injudiciously. The patient thus may, and often does, receive positive harm when naught but his good is intended; whilst the attending physician is caused, by the facts of the case, to occupy a false position, and by the unwilling resumption of responsibilities he desires to cast off, will, we maintain, in many cases experience nought but discomfort and uneasiness during the entire persistence of the disease.

Did the evil stop here we might regret, but could not complain of such a course being adopted by any physician, as he would have a right to yield to his kindly feelings and be governed by persuasion, at whatever cost it might be to

his personal happiness or temporary comfort. But when, as is the case, the entire profession is concerned, each member of it has a right to remonstrate, and if the act complained of be generally disapproved, the parties thereto are in duty bound to desist.

Now, how does the matter under consideration affect the profession as a body? That the laborer is worthy of his hire is a maxim no more true than strongly impressed on the minds of men, and that which can be procured without cost is apt to be valued accordingly. This fact cannot be denied. And just in proportion as the thing or service thus gratuitously procured causes a greater or less amount of inconvenience to the individual affording it, just so much, in the estimation of mankind, is its value decreased. The practice of physic is always troublesome, and sometimes becomes extremely so. Lookers on will apply this rule to our profession or business, as to all others, and where it is procured, easily, for nothing, at great sacrifice, too, on the physician's part, and without there being any necessity for such a course, it must become lowered in the eyes of the world; and as valued at naught by him who possesses it, the profession will ere long become equally undervalued by those around him. Thus thoughtlessly, and perhaps unknowingly, may the individual injure, nay, by his actions, tend to degrade an entire class. Thus unwillingly may he violate his oath, given when he received his degree, and so become deserving of an immediate expulsion from our ranks.

Does not each physician, on receiving his diploma, pledge himself to do all in his power for the interest, and to advance the honor and dignity of the science he professes, and is this practice in accordance with that oath? Does not every member of the medical society or association bind himself to charge always in accordance with established rates, and is it compatible with the spirit or letter of such a pledge, to make no charge at all?

I make these observations and put these questions with the hope that those whose position gives them a right to be regarded as authority on such subjects, will say whether

this habit be compatible, or not, with the dignity and well-being of the science of medicine.

Respectfully, yours, W.

OBITUARY NOTICE.

DIED, in Montgomery, Ala., on the 4th of March last, after a lingering illness, Dr. William M. Boling.

Dr. Boling was born in Baltimore county, Md., July 14th, 1811. His father, dying very soon after his birth, left him under the protection and guidance of his mother. What his early advantages were we are not informed. We know that he went to Greensburg, Pennsylvania, to reside when eighteen years old, having received only an ordinary English education. Whilst here he determined upon the study of medicine, and in 1833, entered the office of Dr. S. P. Brown. Prosecuting his studies with great assiduity, he applied himself not only to his text books but to the study of the Greek, Latin, and French languages, a very fair knowledge of all of which he acquired.

Dr. Boling attended his first course of lectures at the Jefferson Medical College, 1835 and 6; and in the fall of 1837, after having married he returned to Philadelphia to attend his second course of lectures. Graduating the following spring in the Jefferson Medical College, he returned to Alabama and settled in Montgomery where he continued to reside up to the time of his death.

For Dr. Boling's position as a writer and teacher we need only refer to the periodicals of the day. His contributions to the medical literature of our country were always well received. Many of his papers possess great merit, and none perhaps more than the one entitled, "Observations on Remittent Fever," published in the American Journal of the Medical Sciences. In this essay he has shewn most clearly the wide range of his thought, the shrewdness of his intellect, the force of his reasoning powers and the knowledge

he possessed not only of the resources of his art, but of nature herself. The views here set forth have been generally adopted by nearly if not all subsequent writers upon the subject in this country.

In 1848. he was invited to take the chair of Materia Medica, in the Memphis Medical School, which he accepted. His connection with this school lasted but one session, during which he gave, however, ample proof of his ability as a teacher, and endeared himself to a wide circle of friends throughout the south and south-west. Soon after leaving Memphis he received a call from the Transylvania Medical School to the chair of Obstetrics. In this school he delivered one course of lectures (1849 and 50) with entire satisfaction to his colleagues and pupils. At the close of the session he resigned his chair and returned to his home in Alabama. Other positions of trust and honor he was afterwards called upon to fill such as President of the Alabama State Medical Association, and Vice-President of the American Medical Association.

Dr. Boling steadily refused to accept another professorship in any school after leaving Transylvania, although several advantageous offers were made him. Possessed of a wide circle of friends he preferred ramaining among them to spending his life as a public teacher. Occupied with an overwhelming practice, he for many years bore up under immense physical and mental labor, but at last his feeble frame began to give way, and in the spring of 1853, paraphlegia showed itself, from which, although temporarily relieved, he never recovered. Notwithstanding the gravity of his infirmity he continued, as long as he was able, to attend to practice and never ceased to manifest the same interest in medical science which had characterized the whole course of his professional life.

The gradual progression of his disease rendered him at last almost completely helpless and he finallly sank under the prostrating effect of an intercurrent attack of intermittent fever.

Thus ended the short but useful career of one who was

truly a martyr to his noble calling. Had he enjoyed better advantages and a wider field for his labors, all can safely assert that he would have occupied a very high position in the ranks of his profession.

The private character of Dr. Boling was spotless. His moral worth united with the kind and generous impulses of his heart, rendered him one of the noblest of men. His intercourse with others was always characterized by the strictest integrity, truth and candour. To many his manner appeared austere, though not to such an extent as to render him unapproachable. Dignified and self possessed he always commanded self respect. In the relation of husband and father, the goodness of his nature was ever dominant. Gentle and affectionate in his disposition he smoothed the asperties of life and diffused love and happiness to all around him. Long will the memory of so good and wise a man be cherished by the many friends who knew and revered him.

B.

MEDICAL CONVENTION FOR REVISING THE PHARMACOPŒIA OF THE UNITED STATES.

The Medical Convention for revising the Pharmacopœia, which met at Washington in May, 1850, provided for assembling a Convention for the same purpose, in the year 1860, by the following resolutions:

"1. The President of this Convention shall, on the first day of May, 1859, issue a notice requesting the several incorporated State Medical Societies, the incorporated Medical Colleges, the incorporated Colleges of Pharmacy, throughout the United States, to elect a number of delegates, not exceeding three, to attend a general Convention, to be held at Washington, on the first Wednesday in May, 1860.

" 2. The several incorporated bodies, thus addressed, shall also be requested by the President to submit the Pharmacopœia to a careful revision, and to transmit the result of their labours, through their delegates, or through any other channel, to the next Convention.

"3. The several Medical and Pharmaceutical bodies shall be further requested to transmit to the President of this Convention, the names and residences of their respective delegates, as soon as they shall have been appointed, a list of whom shall be published, under his authority, for the information of the medical public, in the newspapers and medical journals, in the month of March, 1860.

In accordance with the above resolutions, the undersigned hereby requests the several bodies mentioned to appoint delegates, not exceeding three in number, to represent them in Convention for revising the Pharmacopœia of the United States, to meet at Washington, on the first Wednesday in May, 1860; and would also call the attention of those bodies to the second and third resolutions, and request compliance with the suggestions therein contained.

GEO. B. WOOD,
President of the Convention of 1850.

Philadelphia, May 1st, 1859.

N. B. Medical and Pharmaceutical Journals will please copy the above notice.

HONOR TO WHOM HONOR IS DUE!

We have observed that nearly all the daily papers, and many Medical Journals, in noticing the death of the late Dr. P. C. Gaillard, speak of him as the founder of the Charleston Medical Journal and Review.

We feel it to be our duty to correct this error, and do justice to those who really deserve the praise attaching to the inception of so laborious and hazardous an undertaking. To Dr. J. Lawrence Smith, the now distinguished Professor of Chemistry and Toxicology in the University of Louisville, and Dr. S. D. Sinkler, since deceased, belong the credit of having established this Periodical, known at first as the Southern Journal of Medicine and Pharmacy. Any of our subscribers who will turn to the volumes edited by these

gentlemen, will see what contributions of time and talent they made to its support, and Dr. Gaillard, their able successor, would have been the last to deny the value of the obligation under which, at that period of scanty journalism, they laid the whole southern profession.

ERRATA IN OUR MAY NUMBER.

Owing to our absence from the city during the month of April and some part of June, the last issue of the Journal was carried through the press in unusual haste, and several errors of importance were allowed to creep into the text, which we do not think advisable to leave uncorrected. We note those only which seriously affect the construction.

Page 343, line 4, for last, read least.
" " " 19, for valve, read vulva.
" " " 29, for harm, read hæmorrhage.
" 344, " 8, for which, read while.
" " " 29, for health, read strength.
" 345, " 5 from bottom, for exerts, read excites.
" 352, " 15, for urine, read serum.
" 395, " 31, for two hours and a half, read twenty-four hours.

TO CORRESPONDENTS AND PUBLISHERS.

A communication has been received from Dr. John H. Packard. The following Books and Pamphlets have been received:

Durkee on Gonorrhœa and Syphilis; Jewett & Co., Boston.
Taylor on Poisons; Blanchard & Lea, Philadelphia.
Dickson's Elements of Medicine; Blanchard & Lea, Philadelphia.
Meigs on Woman and her Diseases; Blanchard & Lea, Philadelphia.
Fownes' Chemistry for Students; Blanchard & Lea, Philadelphia.
Transactions of American Medical Association; Collins, Philadelphia.
Tanner on Diseases of Infancy and Childhood; Lindsay & Blakiston, Phila.
Bell on Baths; Lindsay & Blakiston, Philadelphia.
Five Essays by J. K. Mitchell, M.D.; Lippincott & Co., Philadelphia.
Macleod's Surgery of the Crimean War; John Churchill, London.
Sixteenth Registration Report of Massachusetts.
Dr. E. R. Peaslee's Anniversary Discourse before the N. Y. Academy.
A Valedictory Address to the Medical Graduates of Harvard, by Dr. Bigelow.
Dr. Edward J. Coxe on the Treatment of Yellow Fever.

RETURN OF DEATHS IN THE CITY OF CHARLESTON

FOR APRIL, 1859.

WHITES.		BLACKS AND COLORED.	
Accident,	1	Accident,	2
Cerebritis,	1	Apoplexy,	1
Consumption,	7	Birth, Premature	1
Dropsy,	1	Brain, Congestion of	1
Dropsy of Chest,	1	Bronchitis,	1
Drowned,	2	Cerebritis,	1
Erysipelas,	1	Cholera Infantum,	1
Fever, Typhus	1	Convulsions,	2
Heart, Disease of	1	Consumption,	3
Intemperance,	1	Debility,	1
Old Age,	1	Dropsy,	4
Pneumonia,	1	Fever, Catarrhal	1
	—	Gastro Enteritis,	1
Males, 16; Females. 3; Total,	19	Hydrothorax,	2
	—	Intemperance,	1
		Leprosy,	1
		Marasmus,	4
		Old Age,	4
		Pericarditis,	1
		Pneumonia,	2
		Trismus Nascentium,	4
		Worms,	1
			—
		Males, 21; Females, 19; Total,	40
			—
Ages.—Under 1 year,	1	Ages.—Under 1 year,	13
1 to 5,	1	1 to 5,	4
5 to 10,	0	5 to 10,	1
10 to 20,	2	10 to 20,	1
20 to 30,	3	20 to 30,	3
30 to 40,	6	30 to 40,	3
40 to 50,	1	40 to 50,	5
50 to 60,	1	50 to 60,	2
60 to 70,	3	60 to 70,	4
80 to 90,	1	70 to 80,	1
	—	80 to 90,	3
Total,	19		—
	—	Total,	40
			—
Natives of Charleston,	6	Natives of Charleston,	33
Natives of United States,	3	Natives of United States,	5
Foreigners,	10	Foreigners,	2
	—		—
Total,	19	Total,	40

J. L. DAWSON, M.D., *City Registrar.*

RETURN OF DEATHS IN THE CITY OF CHARLESTON

FOR MAY, 1859.

WHITES.		BLACKS AND COLORED.	
Brain, Congestion of	1	Anæmia,	1
Bronchial Inflammation,	1	Apoplexy,	2
Cancer of Uterus,	1	Apthœ,	1
Congestion,	1	Asthma,	2
Convulsions,	2	Birth, Premature	1
Consumption,	3	Brain, Congestion of	2
Delirium Tremens,	1	Cancer,	2
Diarrhœa,	1	Catarrh,	1
Dropsy,	2	Cholera Infantum,	2
Enteritis,	1	Convulsions,	6
Erysipelas,	1	Consumption,	7
Fever, Scarlet	1	Debility,	1
Gastritis,	1	Diarrhœa,	1
Heart, Disease of	1	Dropsy,	2
Hepatitis,	1	Fever, Typhus	4
Pneumonia,	2	Heart, Disease of	1
Poison,	1	Hydrocephalus,	1
Tetanus,	1	Hydrothorax,	1
Want of Vitality,	2	Lungs, Congestion of	2
		Marasmus,	7
Males 15; Females 10. Total,	25	Meningitis,	1
		Old Age,	5
		Spanæmia,	1
		Teething,	5
		Trismus Nascentium,	2
		Want of Vitality,	1
		Males, 37; Females, 25. Total,	62
AGES.—Under 1 year,	4	AGES.—Under 1 year,	19
1 to 5,	0	1 to 5,	10
5 to 10,	2	5 to 10,	3
10 to 20,	2	10 to 20,	2
20 to 30,	3	20 to 30,	4
30 to 40,	3	30 to 40,	1
40 to 50,	4	40 to 50,	7
50 to 60,	2	50 to 60,	4
60 to 70,	2	60 to 70,	5
70 to 80,	3	70 to 80,	2
		80 to 90,	4
Total,	25	90 to100,	1
		Total,	62
Natives of Charleston,	12	Natives of Charleston,	50
Natives of United States,	4	Natives of United States,	11
Foreigners,	9	Foreigners,	1
Total,	25	Total,	62

J. L. DAWSON, M.D., *City Registrar.*

METEOROLOGICAL ABSTRACT FOR APRIL, 1859.

BAROMETER.

Maximum, 30.444, - - Minimum, 29.760, - - - Range, 0.684

THERMOMETER ATTACHED.

Mean.

7 A. M., 63.76, - - - 2 P. M., 68.46, - - - 9 P. M., 66.26

THERMOMETER DETACHED.

Mean.

7 A. M., 59.66 - - - 2 P. M., 69.93, - - - 9 P M., 63.83

Highest Degree.

7 A. M., 71, - - - 2 P. M., 81. - - - - 9 P. M., 75

Lowest Degree.

7 A. M., 41, - - - 2 P. M., 53, - - - - 9 P. M., 51

REGISTER THERMOMETER.

Mean.

Night, 57.36, - - - - - - - - - - - Day, 72.36

DEW POINT.

Mean, - - - - 56.80

WINDS.

Sun Rise—NE 5 days; NW 4; S 1; SE 2; SW 16; W 2. Prevailing SW.
4 *P. M.*—NE 4 days; NW 2; S 1; SE 3; SW 18; W 1. Prevailing SW.

WEATHER.

Fair, 23 days—Cloudy, 2 days—Rain, 5 days

QUANTITY OF RAIN.

Inches, - - - 1 48.

JOHN L. DAWSON, M.D.

METEOROLOGICAL ABSTRACT FOR MAY, 1859.

BAROMETER.

Maximum, 30.400, - Minimum, 29.872, - - - Range, 0.528

THERMOMETER ATTACHED.

Mean.

7 A. M., 69.35, - - 2 P. M., 74.83, - - - 9 P. M., 72.48

THERMOMETER DETACHED.

Mean.

7 A. M., 66.61, - - 2 P. M., 77.64, - - - 9 P. M., 70.54

Highest Degree.

7 A. M., 75, - - - 2 P. M., 82, - - - - 9 P. M., 76

Lowest Degree.

7 A. M, 53, - - - - 2 P. M., 70, - - - 9 P. M., 64

REGISTER THERMOMETER.

Mean.

Night, 65.35, - - - - - - - - - - - Day, 79.77

DEW POINT.

Mean, - - - 63.41.

WINDS.

Sun Rise—NE 13 days; NW2; SE 9; SW 6; E 1. Prevailing NE.
4 *P. M.*—NE 5 days; NW 1;SE 15, SW 5; E. 5. Prevailing SE.

WEATHER.

Fair, 25 days—Cloudy, 3 days—Rain 3 days.

QUANTITY OF RAIN.

Inches, - - - - 0.28.

JOHN L. DAWSON, M.D., *City Registrar.*

MEDICAL COLLEGE OF VIRGINIA.

AT RICHMOND.

SESSION OF 1859-60.

The regular course of Lectures will commence on Monday, the 3d of October, and continue until the 1st of March.

Charles Bell Gibson, M.D., Professor of Surgery and Surgical Anatomy.

David H. Tucker, M.D., Professor of Theory and Practice of Medicine.

Beverley R. Wellford, M.D., Professor of Materia Medica and Therapeutics

Arthur E. Peticolas, M.D.. Professor of Anatomy.

Levin S. Joynes, M.D., Professor of Institutes of Medicine and Medical Jurisprudence.

James H. Conway, M.D., Professor of Obstetrics and Diseases of Women and Children.

James B. McCaw, M.D., Professor of Chemistry and Pharmacy.

Marion Howard, M.D., Demonstrator of Anatomy.

The study of Practical Anatomy may be prosecuted with the most ample facilities and at a very trifling expense.

The Infirmary, under the same roof with the College, is at all times well filled with Medical and Surgical cases, and furnishes peculiar facilities for Clinical instruction.

Many Surgical Operations are performed in presence of the class, and the Students being daily admitted to the wards, enjoy, under the guidance of the Professors, unusual opportunities for becoming familiar, not only with the symptoms and diagnosis of disease, but with its daily progress and treatment.

Students also enjoy the Clinical advantages afforded by the Richmond Almshouse.

FEES.

For the entire Course of Lectures, - -	$105 00
Demonstrator's Fee, - - -	10 00
Matriculation Fee, - - - -	5 00
Graduation Fee, - - -	25 00

A Prize of ONE HUNDRED DOLLARS, offered by Dr. Thomas D. Warren, of North-Carolina, will be awarded to the member of the graduating class who shall present to the Faculty the best Essay on any medical subject.

Board may be obtained in the city at prices ranging from $3.50 to $5 per week. The catalogue, containing fuller information concerning the Institution, will be forwarded to those who apply for it, or specific enquiries will be answered by letter.

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Dean of the Faculty, Richmond, Va.

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CHARLESTON

MEDICAL JOURNAL AND REVIEW.

VOL. XIV.] CHARLESTON, S. C., SEPT., 1859. [No. 5

ART. I.—*Surgical Cases.* Reported by JOHN H. PACKARD, M.D., of Philadelphia.

CASE I.—*Fracture of the Radius at the Wrist. Resection fourteen months after, on account of Necrosis. Result satisfactory.*

Resection of the wrist joint is an operation which has not found much favor among surgeons, but that it is sometimes practised with advantage the following case will, I think, be considered as proof.

William Brummage, colored, aged forty-five, applied to me at the Moyamensing Dispensary, in May, 1857, to be treated for an affection which rendered his right wrist stiff and useless.

About fourteen months before, he had sustained a fracture of the radius, by a fall. A surgeon in the country had treated him for the injury, but the result had not been satisfactory. A large and firm prominence existed at the back of the wrist, and a depression in front; the carpal bones seemed twisted away from the radius, increasing the deformity. By passing a probe into a sinus opening at the radial side, dead bone could be felt; an orifice existed also at the ulnar side, but no sign of necrosis was perceptible through it. His condition was very good, although he had at one time been intemperate; and being incapable of steady work, he was anxious to have some operation performed for his relief.

Upon consultation with my colleague, Dr. Brinton, I decided to perform resection. Accordingly, on the following morning the patient was thoroughly etherized; the opening on the radial side of the joint was enlarged upwards and downwards to the extent of about two inches, and the incision carried across the swelling at the back of the wrist, so as to form a flap. The tendons were then drawn aside by means of retractors, the dead bone exposed, cut through with a pair of strong bone-nippers, and extracted. Some remaining portions of the necrosed bone were removed with a pair of gouge-forceps. The incision was now carried round to the ulnar side, and the lower end of the ulna removed in the same manner, after which the wound was carefully examined, and no trace of dead bone was felt. All the tendons were saved except that of the supinator longus.

In the course of the operation several vessels required ligation, and some blood was lost; but the patient reacted extremely well. The wound was closed with five points of the interrupted suture, and dressed with lint and cerate; the hand and fore-arm were bandaged upon a straight palm-splint, and he was placed in bed. In the evening there was some fever, and I substituted the warm water dressing for the lint and cerate at first applied.

On the third day the dressing was again changed, a mixture of Liq. Plumbi Subac. and Tinct. Opii being now used. The wound began to suppurate freely on the next day. He complained simply of pain at the radial side of the wrist, but this passed off in about a week. By the twelfth day all the ligatures came away.

On the twenty-fourth day, as there still remained an opening at the radial side, leading down to dead bone,—the posterior portion of the radius,—he was again etherized, and an incision about two inches and a half in length made in the median line of the fore-arm, directly down to the bone. The tendons were drawn aside, and it was then found necessary to enlarge the radial incision also, in order to gain space for the introduction of the forceps. By means of these the necrosed portion was nipped off, and then pried out with an

elevator, the soft parts being divided with a bistoury. Some bleeding ensued, which was checked by the insertion of pledgets of lint dipped in Tinct. Ferri Chloridi. Adhesive strips were employed to close both the wounds, which were also dressed with lint and cerate; the whole joint was surrounded with lint dipped in the same mixture of lead-water and laudanum as before; the fore-arm was then bandaged to a splint, and arranged upon an inclined plane. Suppuration became fully established in three or four days, and he did extremely well. The dressings were reduced to simple lint and cerate, after the lapse of about three weeks, and subsequently Ung. Elemi Comp. was employed, to bring on a somewhat higher grade of action in the wound. Passive motion of the fingers was made with great care daily, and about five months from the date of the first operation, I allowed the hand to rest in a mere sling, the wounds being still protected with lint and cerate. He was doing very well, and his hand gaining strength, when one day he became intoxicated, and ran away. More than a year afterwards, however, I met him in the street, pushing a hand-cart; he had very good use of the hand, except that the stiffening, inevitable after such long continued disease and extensive loss of bony substance, had deprived him of the power of pronation and supination. The discharge had ceased, and the openings closed; the member was, in fact, sound.

Now, this case was not one which would, *a priori*, afford much hope either to surgeon or patient. The man's habits had not been good; and it was strongly to be suspected that his lameness, by unfitting him for steady work, had paved the way to greater irregularities of life. There was reason to believe, as subsequently proved to be the case, that the mass of necrosed bone was by no means inconsiderable; the whole lower end of the skeleton of the fore-arm had to be removed. In spite, however, of these discouraging circumstances, it seemed that my duty was to attempt something by way of improvement upon the then existing state of the limb, and the result fully answered my expectations.

Perhaps the event of this case may induce other surgeons

to look more favorably upon the operation of resection at the wrist, and diminish still further the small number of instances in which this joint is considered so hopelessly diseased as to call for amputation.

CASE II.—*Fracture of the Clavicle.*

James Breshman, aged thirteen, applied to me at the Moyamensing Dispensary, in November, 1857, having met with an accident forty-six hours previously. He had tripped while running, and had fallen sideways, striking the point of his left shoulder on the ground.

There was slight swelling, and great pain on pressure, at the middle of the left clavicle; no mobility at that point. The motions of the shoulder were not lost, but very much impaired. A Velpeau's bandage was applied, and he went home. I saw no more of him until two months after, when he came to the Dispensary for medicine for some one else; I recognized him, and upon examining his left clavicle found a very well-marked projection of the inner fragment at the spot where there had been pain on pressure. Here there is every reason to believe that the fracture was the result of indirect violence, that it was nearly transverse, and that if the boy had been less impatient of restraint the deformity might, perhaps, have been obviated entirely.

Two points of interest are presented by this case. One is, the illustration afforded by it of the value of a precisely localized pain in the diagnosis of fractures; a value directly proportionate to the superficiality of the bone involved. The other is, that although the violence causing the fracture was manifestly indirect, the line of separation between the fragments was only slightly oblique. And this I would attribute, in some measure, to the fact that the actual force was not great, either because the fall was an easy one, or because of the mechanical conditions under which the bone was placed. From many cases which have fallen under my own observation, there seems to me to be a relation quite clearly traceable between the degree of the causative force, be it direct or indirect, and the degree of obliquity of the

resulting fracture. Whether this rule applies equally to comminuted and single fractures,—if, indeed, the experience of others be found to sustain it,—I am by no means prepared to say; but in comminuted fractures there is apt to be one predominant line of division, in reference to which the above suggestion might be tested.

CASE III.—*Simple, uncomplicated Fracture of the Coracoid Process of the Scapula.*

Mary Anne Gray, aged forty, came under my charge in February last, having the night before fallen backwards on the ice in a narrow alley, striking the inner side of her right elbow. She had pain in her shoulder and breast, and down the arm, especially near the elbow; was unable to carry the elbow backward to any extent.

On moving the arm in various directions, I felt crepitus somewhere about the shoulder joint; and by carrying the elbow backwards and forwards on a level with the shoulder, I succeeded in localizing the crepitus, and thus in detecting a fracture of the coracoid process of the scapula. It was during the forward motion of the humerus that this sign became perceptible.

This fracture, of the reality of which I convinced myself by repeated and most careful examinations, was probably the result of a direct blow from the head of the humerus, since the elbow must have been carried backwards in order to strike the ground by its inner side, and thus the coracoid process would be in a direct line with the axis of the humerus. Nor would this condition of things be sufficient, had not the muscles around the scapula been firmly contracted in the effort to avoid the fall. In spite of the strongly expressed opinions of authors generally, as to the almost impossibility of fractures of the coracoid process of the scapula, I think that the explanation just given amply accounts for the occurrence of this lesion, and no other, in the case related.

No apparatus was employed except a support for the arm, and the patient did very well.

CASE IV.—*Abscess laying bare the Sternum. Recovery without Exfoliation or Necrosis.*

Thomas Aiken, aged thirty-eight, an Irish shoemaker, of temperate habits, placed himself under my care some time since, on account of swelling and pain affecting the left shoulder. About four weeks previously he had sustained a severe fall, by which the left arm was rendered useless. On examination a swelling (which, he says, began under the left pectoralis major,) was found occupying the entire left pectoral region, the upper and inner part of the arm, and the sub-scapular space. This swelling gave the shoulder a flattened appearance; it was tense and fluctuating, but there was no point of softening anywhere in its extent. It was poulticed, and his general condition carefully regulated.

Two days afterwards, as he was in great pain, and the swelling very tense, I made an incision at the upper and inner part of the arm, giving exit to an immense quantity of pus, and greatly relieving him. The discharge of matter continued for about three weeks to be extremely profuse; he was placed upon the most nutritious diet at his command, and tonics freely administered, but the system at large gave tokens of exhaustion.

He now complained greatly of pain over the sternum; there was evidently swelling and fluctuation there, and I made an incision, by which a very large quantity of pus escaped. At the bottom of the suppurating cavity there was plainly to be felt the second piece of the sternum, *perfectly bare, and bathed in pus.*

On account of night-sweats, which had become very exhausting to him, I now ordered Ac. Nitro-Muriat., gtt. viii., t. d., the other tonics to be continued.

The improvement which took place after the opening of this second abscess was very rapid. The discharge of pus from each orifice lessened daily, the cavities filled up, and the man's general health became re-established. Only three weeks from the time just mentioned I ceased my attendance upon him entirely. Not a particle of bone had been perceptible in the discharge at any time, nor was there

any remaining tenderness after the apparent healing of the abscess.

CASE V.—*Traumatic Aneurism of the Brachial Artery, of very rapid formation. Ligature. Recovery.*

Patrick Welsh, aged forty, a railroad laborer, was admitted into the Pennsylvania Hospital on the 26th of January, 1856, for an injury of the right arm. He stated that while mending some harness upon a horse, the evening before, the horse moved round in the stall so as to cause him to wound his right arm with a knife, which he held in his left hand. Much hæmorrhage occured at the time, but was checked by the bystanders with cotton and firm pressure, until a doctor came. The doctor sewed up the wound, and applied a Spanish windlass above it. No recurrence of the bleeding took place, and the patient came a distance of thirty miles to enter the Hospital.

At the time of his admission, a little serum was oozing from the wound, which was about an inch long, and about three inches above the elbow. Just above the wound was a rather firm swelling about as big as a pigeon's egg, pulsating and thrilling distinctly. The arm and fore-arm were much swollen, owing to the application of the Spanish windlass during the preceding twenty-two hours. Both pulses at the wrist were almost imperceptible; pressure upon the axillary artery controlled the pulsation in the tumor.

The arm was bandaged to an inside angular splint, and the patient kept as quiet as possible until the next day. A consultation was then called, and an operation decided on. Dr. Peace, the attending surgeon on duty, exposed the partially divided artery by enlarging the wound, and then applied ligatures above and below the seat of injury. Notwithstanding the very short period which had elapsed from the time of the accident, the wall of the aneurism was already firm and well-defined.

No untoward symptom ensued, and on the 21st of February the patient was discharged, entirely cured.

ART. II.—*Neuro-Hypnotism, or Artificial Nervous Sleep. A Critical Examination of the mode of Induction of this Condition of the System, and of the Phenomena accompanying it, and also its importance as a Therapeutical Agent: with a brief history of its discovery* by Dr. BRAID, of Edinburgh. An Inaugural Essay. By R. Y. DWIGHT, M.D., of Colleton, S. C.

The world has at different times been attracted by the experiments of the mesmerists, the Animal Magnetists, and others, who claimed by certain processes, which altered the condition of the nervous centres, and changed the mental states, to give rise to most extraordinary phenomena; different from those exhibited in ordinary sleep, though the physical appearances were generally similar. In these conditions patients were peculiarly impressible, and responded promptly and powerfully to certain stimuli—among which, the most remarkable were those employed to produce phrenological manifestations. No very decided impression, has probably been made on the scientific world as to the truth and correctness of *all* for which the advocates of these various theories have contended, but the great pretensions that were made aroused men of scientific attainments and ability to investigate the facts, and though little or nothing of very great value has resulted from such examination, and much for which the believers in these doctrines contended, has been shown to be the invention of fertile imaginations or the result of inaccurate observations—it is nevertheless admitted that some phenomena are bona fide, and apparently the effect of a peculiar impression made upon the nervous system, by the process to which the patients were subjected. This process consisted, in general terms, in a fixing of the visual eyes intently upon those of the operator and retaining them thus until sleep was induced. The patient was then in a condition to be operated upon experimentally, and all the remarkable phenomena that were exhibited were attributed to a special influence of the operator over the patient. This influence

the Animal Magnetists contended, was magnetic in its nature, its special source the Mesmerists thought obscure. The Animal Magnetists and the Mesmerists both asserted that their influence was not circumscribed, but would act in spite of intervening space.

Theories like the above have not been uncommon, and under various names have become familiar to all. In the review then which we shall make of the subject of "Neuro-Hypnotism" as treated of by Dr. James Braid, of Edinburgh, we will not find his views altogether novel. It would have been interesting to trace minutely the history of Mesmerism and Animal Magnetism and its connection, analogy and points of resemblance, with the subject under review; but space and time will not permit it, and it must content us to have merely indicated that such connection exists—that much is necessary to show *hereafter* that they are very different.

Dr. Braid defines Neuro-Hypnotism to be "a peculiar condition of the nervous system into which it can be thrown by artificial contrivance," or "a peculiar condition of the nervous system, induced by a fixed and abstracted attention of the mental and visual eye, on one object, not of an exciting nature."

We propose first to examine the circumstances which led to the investigation and developement of the subject. Let us remark however, in passing, that, if we may judge by the character of those with whom Dr. Braid associated, we can have no reason to doubt his statements, or reject as untrue what he asserts, for many of the most scientific and learned men in Edinburgh were his correspondents and friends, and took an interest in his investigations.

Being a seeker after truth and scientific knowledge, he was witnessing experiments in mesmerism with the hope of detecting errors and impositions, or of discovering some cause which would philosophically account for the phenomena which were presented. He was very skeptical, he tells us, as to the whole subject, and saw nothing on the first evening he attended the experiments calculated to make him change his view—he still "believed the whole to be a system of collu-

sion or delusion, or of excited imagination, sympathy or imitation;" but one phenomenon attracted his attention, viz: the inability of the patient to open his eyelids. This he regarded as a *real* phenomenon; and was anxious to discover the physiological cause of it. He watched this phenomenon then intensely on the next occasion, and came to the conclusion that it was caused by the "continued, fixed stare producing paralysis of the nervous centres, (that exhaustion following too long continued or intense action of any organ or function) in the eyes and their appendages, and destroying the equilibrium of the nervous system." He did not divulge his views, until, in the presence of his family and several friends, he instituted such varied experiments as to assure all of the truth of his hypothesis.

He caused an individual to sit down and maintain a fixed stare at an object placed at such a distance above and in front of his eyes as to cause considerable strain to maintain it; after the lapse of three minutes his eyelids closed convulsively, tears dropped from his eyes, his head drooped, his face was slightly convulsed; he gave a groan and immediately fell into a profound sleep; respiration became slow, deep and sibilant; the right hand and arm slightly convulsed—and symptoms of alarm were presented on being awakened. The same result followed experiments on two other individuals. These experiments not only proved what he had asserted; but the state of the respiration, the condition of the mind as indicated by the alarm depicted upon the countenance of the patient when aroused, satisfied him that he had discovered the true explanation of the phenomenon which had attracted his attention in the mesmeric experiments. He attributed it to "a derangement of the state of the cerebro-spinal centres, of the circulatory, respiratory, and muscular system, induced by a fixed stare, absolute repose of body, fixed attention, and suppressed respiration, concomitant with that fixity of attention." "That it was owing to the physical or psychical condition of the patient caused as before explained, and was entirely independent of volition or the pass of the operator throwing out a magnetic fluid or exciting into activity some

mystical universal fluid or medium." He had no doubt furthermore, that having excited the *primary*, the other phenomena would follow successively—time being allowed for their gradual developement.

Though at first he believed that the agencies which produced the phenomena by his, and the method of the mesmerists were identically similar—except that as *he required* fixity of stare and attention, and they by their mode *could not command it*, he obtained effects more invariably—yet he was led to modify his opinion because of their assertion that they could produce effects which he vainly endeavoured to obtain. While he admits then that the agencies may be different, he thinks that the phenomena to which they give rise may be more naturally explained than by them, and contends that as his mode is more invariable in its effects, it is superior in its general applicability and practical utility, and is not obnoxious to the charge that it may be made use of for criminal or immoral purposes; a strong objection to animal magnetism and mesmerism, if, as the proselytes of those doctrines assert, they have the power to overmaster individuals distant from them by mere volition and secret passes.

We pass on now to the remarkable phenomena which in the course of his experiments, Dr. Braid found to result from this peculiar condition of the system which he called Neuro-Hypnotic. It has already been noticed, that the strained, fixed stare was followed in three minutes by convulsive closing of the eyelids, dripping of tears, drooping of the head, slight convulsive movements of the head, groaning, profound sleep, respiration becoming slow, deep and sibilant—with convulsive movements of the hand and arm, and alarmed appearance on being awakened. The eyelids will be found to close with a vibratory motion, or to close spasmodically. If the patient be intensely affected, it will be seen that, if, after the lapse of ten or fifteen minutes, the arm and legs be slightly and gently raised, they will have a tendency to retain their position. Should this not happen, in a low tone request him to retain his limbs in this position, and the pulse will quickly become accelerated and the limbs gradually rigid

and involuntarily fixed. All the organs of special sense, sight excepted, the sense of heat and cold, become at first greatly exalted—resembling the effects of opium, wine and spirits. This exaltation is followed at a certain point by depression much greater than the torpor of natural sleep. Patients may be brought from the rigid and torpid state in the shortest time back to the first stage of mobility, exalted sensibility, by directing a blast or current of air, either from the mouth, by a wave of the hand, or any other mode, against the organ which it is desired to reduce from the cataleptiform or rigid state. The senses return to their original condition by mere repose. The extent and abruptness of these transitions must be seen to be appreciated, so extraordinary are they. They are described by Dr. Braid as invariable, but he acknowledges his inability to account for them. An abrupt blow on any part will dehypnotise it if it be rigid, but gentle presure on the nose will not restore its function, nor presure on the ear hearing, nor rubbing—unless so slight as to be only titillation—sense to the skin; and yet the gentle contact of wind will throw any part into a state of exalted sensibility and mobility. In the first stage we have as the effect upon the mind, an extraordinary power of concentration of thought or disposition to wrapt contemplation: at a later we have the imaginative faculties brought into play, and the most expanded, bright, and glowing scenes are presented to the fervid imagination. At a certain stage the same abruptness of transition may be remarked in the mental phenomena as has been noticed of the physical, and it is at this point that phrenological manifestations are readily exhibited. When the individual is a second time operated upon, the same mental images, thoughts or emotions, are liable to recur as were excited during the first experiment.

Let us examine into its resemblance to, and difference from ordinary sleep. In the approach of this latter condition objects become more and more faintly seen, the eyelids close and remain quiescent, and all the other organs of special sense become more and more blunted, ceasing to convey their usual impressions to the brain; the limbs from suspen-

ded muscular tone and action become relaxed, the pulse and respiration grow slower, the pupils are turned upwards and inwards and are contracted. In the neuro-hypnotic state vision grows gradually less perfect, the eyelids close, but first attended with a vibratory motion, or sometimes they close forcibly or spontaneously—the spinal senses are greatly exalted at first, but subsequently become greatly blunted, much below the torpor of natural sleep, but contrary to the condition in that state they are much dilated and entirely insensible to light.

As before noticed, the pulse and respiration are slow, but on producing muscular action, they are quickened and a tendency to cataleptiform rigidity ensues. The limbs may be maintained for any length of time that may be thought prudent in this condition, and what is most remarkable, no corresponding exhaustion results. It is observed that in the progression of natural sleep any object held in the hand will be let fall. The opposite will be the result in the approach of the neuro-hypnotic state. The hand will grasp the object more firmly; this indeed would be inferred from what has been said of the condition of the muscular system in the two states.

The power of balancing the body is another circumstance to be noticed as the effect of the neuro-hypnotic state. Patients apparently acquire the centre of gravity by instinct in the most natural and graceful manner, and if allowed to stand in any position remain rigidly fixed. A tendency and taste for dancing is induced by this agency on hearing music, and varied and graceful postures are assumed by directing gentle currents ugainst the patient. Dr. Braid suggests that the use of this agent may have been the cause of the success of the Grecian Sculptors: and that it may be the satisfactory mode of accounting for those extraordinary feats of the Fakirs suspending themselves by a foot or an arm, &c.

We have seen that the symptoms are in general analogous to those following the free use of opium, spirits, &c., at first excitement and subsequently corresponding depression. Although the phenomena are somewhat analogous to those

produced by nitrous oxide gas; there is this marked difference—that an irresistible impusle to general muscular effort as well as to laughing is produced by the gas, whereas there seems to be no tendency to muscular action in the neuro-hypnotic state, except from impressions excited from without. Under the latter condition, too, we have accurate balancing and power of locomotion, very different from the partial paralysis of intoxication, in which though the intellect may be in a measure clear, we have no co-ordination of motion. The state of muscular quiescence, with acute hearing and dreamy flowing imagination, approximates, says Dr. Braid, to the effects of conium. It seems that the imagination, sympathy, and habit exercise great influence in inducing the condition in those who have previously been under its influence, for Dr. Braid gives us an example in which twenty-two persons became hypnotised, (all having once before been under its influence) by simply holding on to each others dress, concentrating the mind upon the act and anticipating the result. And, to show that this only happened to those previously operated upon, he records an instance in which seventeen, all of whom with one exception, had before been hypnotised, acted as the twenty-two above mentioned and were all affected save the one never before hypnotised. This one, on being operated upon alone in the usual way, became speedily affected.

As examples of the increased sensibility of patients under the influence of neuro-hypnotism, Dr. Braid states that an individual who could not hear the tick of a clock, beyond the distance of three feet, when hypnotised could distinguish it at a distance of thirty-five, and walk towards it understandingly. Another could trace a rose by its odor at forty-five feet.

Tactile sensibility is greatly heightened and exerts a powerful influence upon the muscles, and an impression upon the nerves of touch. Sensations of heat and cold can be excited by objects held at distances, and slight blasts of air could be felt at distances of from fifty to ninety feet. Patients exhibited a tendency to approach or recede from objects or stimuli

as their impressions were agreeable or not. A sudden sound produces shuddering and will almost cause the patient to fall. These phenomena are all presented in the first stages, it has already been noticed that the second is one of complete torpor. In it all sensibility is lost, and sound the loudest and most discordant, odors the most pungent and disagreeable, and tactile impressions, the most severe, produce no impressions whatever. A puff of air directed to any part will immediately restore exalted sensibility.

The whole body being in a state of torpor and the muscles rigid, gentle pressure over the eyeballs will arouse the patient as regards all the mobility of the head and neck—of all parts indeed, receiving impressions from the 5th pair or above their origin and those inosculating with them; while these effects will not follow simple mechanical application to other organs of sense. This seems to be a remarkable proof that an important connection exists between the state of the eyes and parts immediately adjacent, and the condition of the brain and spinal cord during the hypnotic state. Dr. Braid remarks that it is an excellent illustration of the propriety of Mr. Mayo's designation of the origin of the fifth pair, which he styles "the dynamic centre of the nervous system." As another proof of the connection between the state of the eyes and the condition of the nervous system, the brain and spinal cord, it may be stated that Dr. Braid's experiments showed that, supposing the state of torpor or rigidity to exist, a puff of air or gentle pressure on one eye would produce a return of sensibility to that eye, and sense and mobility to the same side of the body as that of the eye touched, and not to the other side; hearing and smell, however, were not restored to either side. Thus, by pressure upon both eyes, he could produce paraplegia, and by pressure upon a single eye, hemiplegia. Frequently, when the patient was hypnotised, while looking sidewise, his body had a tendency to turn in that direction. It seemed puzzling that by acting on one eye both sense and motion could be restored to the *same* side of the body, as the motor influence is communicated from the opposite side of the brain. It may be

accounted for by the partial decussation of the optic nerves, this partial decussation being perhaps for the express purpose of perfecting the union of sensation and motion through the eyes, "on which we lean as on crutches," thus enabling us to balance ourselves much more perfectly than we could otherwise have done.

Another remarkable circumstance noticed by Dr. Braid is, that, if a powerful galvanic current is passed through the arms of the hypnotised patient, not the slightest shock will be *felt*, although violent contractions of these members may be produced. Should the eyes be now gently pressed upon, or a current of air passed against the face, the same shocks *will be felt* as evinced by the movements of the head and neck, cries of the patient, &c., all of which were not exhibited in the first instance. If the wires of the battery be placed one in the hand and the other in contact with the face, head or neck, or any part released by pressure upon the eyes as before explained, evidence of a shock being felt, will be given, even though it be a less powerful shock than that which failed to produce any effect when both wires were held in the hands. This cannot be explained by saying that when the poles are touching the face and hand respectively, the circle is shortened and one pole is nearer the sensorium than when the poles touch both hands; for when *both* poles are made to touch the face, head or neck, all appearance of feeling will disappear unless the current be increased. Some other mode must be found of accounting for it. Another remarkable phenomenon is spoken of. The brain being in a state of torpor, the muscles rigid, skin insensible to heat, cold or any other excitant, if one or two fingers be pressed gently against the back of the head or any other part of the extremity, mobility, with tremors and quivering of the hand, arms, and fingers ensue, greatly increased by pressing another finger against the head, neck, or face. The phenomena cease immediately *if both* fingers are applied to the portions last mentioned.

Insulating rods act just as the fingers—the flat hand has not a very decided effect—pinching causes no change. If

pressure be made against both hands, the arms are contorted, and the patient complains of pins sticking him. These phenomena do not take place in the sensitive stage. It has before been mentioned that the circulation becomes greatly accelerated in the state of rigidity, being oftentimes of twice the natural velocity; it may be brought to the natural standard in less than a minute, by reducing the cataleptiform rigidity, and may be kept at any intermediate condition, by certain manipulations. The blood is circulated less *forcibly* in rigid parts, and correspondingly with greater volume and force in parts not thus affected. The fact that in rigid parts the blood is circulated with less force and the pulse contracted is accounted for simply on mechanical principles. We may alternately increase and diminish the force and frequency of the heart's action, by pressing or not upon the eyes as before mentioned. "The amount of change in the pulse thus induced by acting on the two eyes and liberating the organs of the spinal sense, and the head and neck is about sixty per cent. of the actual rise of the pulse, when at the maximum above the ordinary velocity of the circulation;" and, adds Dr. Braid, "it might be inferred *a priori* that in this new condition of the nervous system, we have acquired an important power to act with."

Having noticed the phenomena, which are presented under the hypnotic influences, let us examine the process by which the condition is induced. Any small bright object, (Dr. Braid used his lancet case,) is held with the thumb, fore and middle fingers of the left hand, at such a distance in front and above the eyes as to compel the most intense strain in order to maintain a fixed stare. The patient must be made to understand that his eye must be firmly fixed upon the object, and his mind rivited to it alone. On account of the consensual adjustment of the eyes, the pupils will at first be contracted; they will soon begin to dilate, and if, when the dilation is considerably advanced and a wavy motion is noticed, the fore and middle fingers of the right hand, extended and slightly separated, are carried from the object towards the eyes; the lids will generally close with a vibratory motion.

Should this not occur or should the eyeballs be allowed to move, the patient must be requested to begin again, and must be instructed to *permit* the lids to close when the fingers are carried towards the eyes as before described; the balls being kept immovably fixed in the same position, and the mind riveted upon the one object. The eyelids will usually close with a vibratory motion or become spasmodically closed. In a few seconds, if the limbs be gently raised, they exhibit a tendency to become fixed in position, if the patient be very powerfully affected. As previously remarked, should this not be exhibited at first, if the patient in a gentle tone of voice be requested to retain his limbs in the extended position, the pulse will become much accelerated and the limbs rigid and involuntarily fixed. As the first stage is merely one of excitement, with the retention of consciousness and docility, many suppose that they are not affected, while, to the experienced, the acceleration of pulse, peculiar expression of countenance and other characteristic symptoms are sufficient evidence, that they are in the first stage. All the phenomena above described may then be exhibited successively. There is great difference in the susceptibility of patients to the impression of neuro-hypnotism; some becoming rigid and insensible in a few minutes; others not going beyond the first stage or being with difficulty brought into the torpid state. Many cures have, however, been effected in patients who could not be reduced to the latter state. It is hazardous to bring the patient to the state of rigidity at the first trial. By staring in any direction with the eyes fixed as before directed (i.e.) in a double internal and upward squint, Dr. Braid was at first in the habit of letting the lids close of themselves, but finding that it caused much pain and sometimes inflammation of the conjunctivæ, he adopted the mode of closing the eyelids, which obviates the disagreeable consequences alluded to. When the eyelids close of themselves it is generally impossible for the patient to open them, but when closed by the hand, they can be opened for some time subsequent. In making experiments on the acuteness of the senses, it is best to allow all to become dormant and then

arouse any particular one, which it may be desired to exhibit in the state of exalted function.

The patient may be aroused from the condition by an abrupt clap of the hands, by a sudden blow with the palm of the hand on the leg or arm, by pressure on, or friction to the eyes, or by a blast of air against the face. He should always be aroused when the breathing is much affected or oppressed, the face greatly flushed, the limbs very rigid, or the heart tumultuous and excessive in action.

We have now run over the principal points of interest connected with the subject of neuro-hypnotism as set forth by Dr. Braid. We have seen the circumstances which led to its discovery; the phenomena presented under its influence, and the process by which the condition is induced. It remains for us to say, whether we shall dismiss the subject as unworthy of belief and reject it as an imposition, or receive it as deserving our confidence. The remarkable nature of the whole subject, the number and peculiar character of the phenomena which are asserted to be exhibited as its effects, and above all the wonderful cures that it is claimed to have effected, all tend to fill us with doubt and perhaps to assure us, upon a superficial investigation, that it is the mere invention of a busy and imaginative mind. But look more deeply into the subject, view it dispassionately and without prejudice, and it presents very strong grounds of respect. And first let us see what claims to confidence the author of this new discovery has upon us. He is a member of the medical profession, of some eminence in one of the most learned and enlightened cities in Europe, being a citizen of Edinburgh. He is the associate and intimate friend of the distinguished of his native city, and of other parts of Europe, connected with scientific, associations, having titles indicative of his standing, and being an earnest seeker after knowledge and truth. The record of his discovery is made with modesty, without dogmatism or extravagance. He plainly and simply lays down, what he asserts to be facts, accounts for them when he can, and freely acknowledges his inability when he is at a loss for satisfactory explanations. Indeed if we

judge the discovery by the character of the author, we cannot regard it as an attempted imposition. We must believe from his narative that what he records is true. Much may be unaccountable, but are we to reject the whole matter because much of it is inexplicable, because no philosophical principles with which we are acquainted, are adequate to explain the striking phenomena connected with this novel condition. Let us be careful—with all the advances in physiological science, how far are we from having attained to perfection of knowledge of its laws; how far, very far, are we from having attained to thorough acquaintance with the functions of the brain and the whole nervous system! Can it be expected that we shall ever know the mysteries of that wonderful connection between the material and immaterial, which are locked up within the human brain, which *we* are *conscious* do there find effectual concealment from the eyes of man. It were as vain to attempt to look into the dispensations of an inscrutable Providence, or to discover the origin, or law of existence of the Divinity itself, as to seek to penetrate the mystery of *human life*, or lift the veil that obscures from mortal ken the wonderful union of mind and matter. We can never know more than that the brain presides over certain functions, and that certain phenomena are produced by stimuli applied to this organ; we will probably in time be able to assign to each *particular* portion of brain that function or those functions which we only know at present is or are exercised by *some* portion of the brain; whatever, indeed, we may in the lapse of ages learn of this wonderful organ, our knowledge must fall short of the *mode* of its action.

As our knowledge is so limited, we hardly have a right to be surprised at any phenomena which changes in the condition of this organ may be said to give rise to, nor any more indeed, at the slight, or trivial, or peculiar causes which are asserted to produce such changes of condition. We can confidently assume then, that the delicacy and complexion of structure of the nervous system are quite sufficient to account for all the phenomena of the hypnotic state, and as it is *possible* that they *might* be mani-

fested, we have no right to deny that they were manifested as Dr. Braid asserts. Moreover, are not many of the phenomena exactly simulated in diseases—in hysteria, &c.? It is admitted by the Editor of the "Lancet," that "the hypnotic phenomena are wonderful only to those who are unacquainted with the aspects of the disease," and that "we continually see patients laboring under hysteric and analogous forms of nervous disease, falling suddenly into various states of stupor, trance, and convulsion, without any assignable cause." "If then," the author (Braid) remarks, "such effects as those named may spring from causes so slight as to be said to arise without any assignable cause, can it be wondered at that important changes may be induced by acting on the nervous system, in the way I have adopted, of which Mr. Herbert Mayo, (whose competence to give an opinion on any physiological subject no one will question, and who himsel publicly submitted to be operated upon by me,) observed in the course of our correspondence that it induces 'a feeling of stupor, which any one may observe has a disposition to creep upon him, when he tries your experiment of looking fixedly at an object as you direct.'"

The condition known as Somnambulism simulates in many respects that described as the hypnotic by Dr. Braid. In the cases of Somnambulism, says Dr. Draper, "it seems as if the mind was wrapped up in one idea, and wholly unable to comprehend any thing else. If the eyes of the Somnambulist are wide open, he sees nothing, and even though a bright light be presented before him, they will not contract; yet he moves about in a manner as if he were, in one respect, guided by understanding, the air of his movements being as if he knew what he was about, yet in another respect as though he was impelled by the most unaccountable folly, walking along the roof of the house, seating himself on the chimney, and finding his way in safety over the most precipitous places, past which it would be impossible to go if awake, no matter how steady his head might be. Besides this complete condition of Somnambulism, there are intermediate forms, during which the various senses of seeing, hearing, &c., are in partial activity. There are also

differences in the intensity or depth of the state, as is shown by the ease or difficulty of arousing the patient, some times to speak to him is enough, some times he must be violently shaken or otherwise roughly treated." Now notice that the very condition of mental abstraction from all other objects, and concentration upon one only, which is described by Dr. Draper as that of Somnambulism, and in which certain remarkable and unaccountable phenomena are presented, is the very same condition which Dr. Braid requires the minds of patients to be placed in, for the induction of the hypnotic state, and the phenomena connected therewith—many of which resemble those of Somnambulism. The analogy is marked. It simulates the cataleptiform and ecstatic conditions, more closely than any other. We need not enter into details as to the phenomena presented under these conditions, we are all familiar with those most marked. Suffice it to say, that catalepsey is "a sudden deprivation of sensation and motion, in which the patient is speechless, senseless, and fixed in one posture, with his eyes open, without seeing or understanding," and ecstacy a condition very similar, except that in it we have "the muscles rigid, and the body erect and inflexible." We have then the various conditions not differing greatly from that which is described as hypnotic. It can not now be very difficult to reconcile with truth what Dr. Braid has said of hypnotism.

What has caused us most doubt and hesitation, are the assertions in reference to the phrenological manifestations said to be exhibited in patients in the hypnotic state. Dr. Braid unhesitatingly declares that they are demonstrable, and he gives numerous instances in which, individuals being hypnotised, pressure on particular portions of the cranium, gave rise to definite and well marked manifestations, which varied with the particular part touched. The results of experiments were not identical in all cases, the same mental state or manifestations not showing themselves, or at least *not following* pressure upon similar points. He did not always obtain, moreover, those manifestations which phrenologists declare will arise from pressure upon determin-

ed and specified parts of the cranium; (e.g.) pressure applied to the phrenologist's organ of *wit*, excited in one patient the desire "to walk," in another pressure over the organ of benevolence, the desire "to travel," &c. But we have said that Dr. Braid gives many instances in which pressure upon different parts of the cranium, were followed by evident manifestations in the mental state. Now it is probable that there are special portions of the cerebral mass which preside over certain emotions, intellections and other states of mind and it may be possible, as Dr. Braid asserts that certain conditions with characteristic indications, may be induced by pressing particular portions of the cranium; but of course it is impossible that these effects are caused by the direct transmission through the walls of the cranium, to the brain within, of the pressure so applied externally; the toughness and unyielding nature of the external coverings of the organ preclude the possibility of such an hypothesis. We must find some more rational and philosophical mode of explaining the matter, if we would explain it at all. That phrenological phenomena are presented under certain conditions of the system, we are perfectly convinced; and, indeed, we have witnessed their exhibition under circumstances which effectually precluded the possibility of any deception. We will not therefore believe it impossible, because we are unable to explain it. Dr. Braid offers two explanations which seem to us very plausible; at least in the present state of knowledge of the nervous system, it seems to us it would be difficult to prove them erroneous. Whether indeed there is any natural and necessary connection existing between the points manifested, or whether it may depend entirely upon associations, which have originated from some partial knowledge of phrenology, from arbitrary arrangements, or accidental circumstances, or causes which have been entirely overlooked and forgotten, and which afterwards produce the results from "that ultimate law of the mind, which ordains, that the repetition of a definite sensation shall be followed by a renovation of the past feelings, with which it was before associated"—must be determined by investigation and reasoning—but if

the manifestations are uniformly excited as declared to be by Dr. Braid it can in no wise lessen the importance of hypnotism as a curative agent and extraordinary means of directing and controlling mental functions in a particular manner, by a simple association of impressions, whether caused in one way, or another. We submit Dr. Braid's explanation: "It must be obvious to all that every variety of passion and emotion can be excited in the mind by music; but how does this arise? Simply by the different effects produced by the varied degrees of velocity, force, quality, and combinations of the oscillations of the air acting on the auditory nerves, these again communicated to the brain, and this action on the mind and body, creating corresponding mental and bodily manifestations. Every one must have observed the remarkable effects produced by these means on the physiognomy, and the more critically observant must have noticed, that in susceptible individuals there is also a very marked change in the state of the respiration and general posture of the body. They must also have experienced, in themselves and others, how prone we are to assume a sympathetic condition, both of mind and body, from those with whom we associate, or during a temporary interview. These physical changes seem to result fron a mental influence imported through the eyes and ears, and then reflected from within through the respiratory, facial, and spinal nerves, on the external form and features. Now such being the case, is there any great improbability, that by calling the muscles of expression into action during the hypnotic state, by titillating certain nerves, that the impression of the feeling with which such external manifestation is generally associated, should be reflected on the brain, and excite in the mind, the particular passion or emotion?

I think it highly probable, that this is the true cause of the phrenological manifestations during the hypnotic state; and as it is the peculiar feature of this state, that the whole energies of the soul should be concentrated on the emotion excited, the manifestations becomes very decided.

I presume the different points pressed on, through the

stimulus given to various fasciculi of nerves, call into action certain combinations of muscles of expression in the face and general frame, and also influence the organs of respiration, and that the mind is influenced, *indirectly*, through the organs of common sensation and the sympathetic, as sneezing is excited in some by too strong a light irritating the optic nerves. In this view of the subject, it would resolve itself into the laws of sympathy, and the question then is, where are the external or superficial points of the sympathies located? Experience must decide this, and in the peculiar condition induced by hypnotism, according to my own experience, this can be more readily and certainly determined, than in the normal state. These points having been ascertained, we can then determine how and where to act according to our particular object and it can be of no real importance, where the cerebral points, or special organs may be posited. If the solution of the cause of these remarkable phenomena now given shall not be deemed correct, the only other which occurs to my mind as at all satisfactory, is this, that the different fasciculi of sentient nerves excite *directly* the *corresponding* points of the brain, and these again the physical manifestations. We know by what musical combinations and movements we can excite the different passions; we know also that this arises from some peculiar impressions communicated to the brain, through the portio mollis of the seventh pair of nerves; and whether this is conveyed to it as a single organ only, or as a combination of organs, it is clear that as the organ of the *seventh* is *more remote* from the brain than the organ of the *fifth*, there must consequently be at least as great difficulty in accounting for such results being excited through the different branches of the *seventh*, as through those of the *fifth* pair." It strikes us that there is much reason in what has been quoted from Dr. Braid.

We propose to give one or two, out of the very many examples which Dr. Braid records of cures effected by hypnotising the patients; they are very remarkable. He accounts for the cures effected, on the general principle of revulsion—one disease being substituted for another—an acute for a

chronic. He details the special modes of procedure in the particular cases recorded; we will not attempt to describe them, but will, before transfering thecases recorded—or a few of them—remark that Dr. Braid declares that the beneficial results of the operations have been so *immediate* and *decided*, as to leave no *doubt* that they stand in the relation of *cause* and *effect*. We relate first the case of Miss Collins, of Neward, Nottinghamshire: she had a spasmodic seizure, during the night, by which her head was bound firmly to her shoulder. The most energetic and well directed means, under a most talented physician, and aided by the opinion of Sir Benj. Brodie, had been tried, as far as known remedies could be carried, (amongst other means, narcotics in as large doses as were compatible, with the safety of the patient;) and although she was carefully watched by night and day, there had never been the slightest relaxation of the spasm, which had continued nearly six months. When I first examined her, Dr. B. says, no force I was capable of exerting could succeed in separating the head and shoulder in the slightest degree. Experience led me to hope, however, that I might be able to do so after she was hypnotised. Having requested all present, excepting the patient, her father, and her physician to retire, I hypnotised her and in three minutes from commencing the operation, with the most perfect ease to myself, and without the slightest pain to the patient, her head was inclined in the opposite direction, and in two minutes more she was roused and was quite straight. I visited this patient only three times, after which she returned home. Shortly afterwards, she had a nervous twiching of the head, and on one occasion it was again drawn down to her shoulder. She was hypnotised by Dr. Chawner, and her head put straight. She is now, twelve months after the last operation, in good health, her head quite straight, with perfect control over the muscles of the neck.

Miss Atkinson had been unable to speak above a whisper for four years and a half, notwithstanding every known remedy had been perseveringly adopted, under all practitioners. After the ninth hypnotic operation, she could speak almost

without effort, and has continued well ever since; now about nine months. It is needless to cite other examples of cures recorded by Dr. Braid, as following the use of this agent. Many much more remarkable than those we have noticed might be given, but these will be sufficient to indicate the importance of this agent in a therapeutical point of view. The names of those operated upon, as well as the names of many who witnessed the operations and their effects are stated, and in such a manner, as to show clearly that there is no attempt at imposition, but a desire to record only truth.

Dr. Braid does not offer hypnotism as a panacea, an infallible cure for all diseases; but says particularly that he believes it useful only in certain forms of disease; he does not hold it up as a *universal* remedy. We must notice even though briefly, the *very* interesting investigations and observations which Dr Braid has made on the subject of Trance, or, as he styles it, Human Hybernation.

His attention was attracted to the subject by hearing of the wonderful power which the Fakeers of India, had of so controlling, in an unknown way, the vital processes, as to submit to *interment* for *many days* and even *weeks*, and yet be restored by warmth and friction to the natural condition, after presenting, upon being disinterred, all the appearances of dead bodies; eyes sunken and glazed, breathing and movements of the heart suspended as far as could be detected, skin wrinkled, sensation utterly lost, &c.

He inquired diligently of the Officers of the British Army stationed in India, who had ample opportunities of judging of the truth of these reports and many of them supplied him with evidence that is incontestable; establishing that some of the Fakeers really did possess the power alluded to, and actually performed the wonderful feat, within the limits of the English Camp, under circumstances that prevented effectually any trickery or deception. It was to this peculiar condition into which the Fakeers passed when thus buried, that Dr. Braid applied the term "*human hybernation;*" and believes in general terms, that it is a condition analogous or exactly similar, perhaps, to that which some of the lower orders of

animals present, when they *hybernate*—in which the functions are still active, though *very* greatly enfeebled, and that the Fakeers reduce themselves to this torpid or hybernating state by *hypnotising* themselves, and by gradually increasing the length of time during which they remain in this state, at last are able to be buried for a great many days, and even two weeks or more, and yet be restored perfectly by the application of friction, heat and moisture. The remarkable case of Col. Townsend of the British Army, who could at will suspend the functions as far as the most careful examination could detect, and assume the exact appearance of death, is probably familiar to all; it is cited by Dr. Braid as related by Dr. Cheyne who with Dr. Baynard and Mr. Skrine, *witnessed* the remarkable feat and testify it its truth.

Now these facts are proved by irrefragible evidence, and we cannot reject them. The phenomena of hypnotism are not more remarkable—not more inexplicable—indeed we may agree with Dr. Braid, that the feat of the Fakeers is performed *through the agency of hypnotism.* It seems to us a plausible theory. At any rate we have no more right to reject hypnotism, than to disbelieve what is asserted of the power of the Fakeers. Can there be wanting further evidence to assure us that all that we have been told of hypnotism may reasonably be credited, and should not be dismissed merely because it is inexplicable? We would refer the curious to Dr. Braid's "Observations on Trance or Human Hybernation;" a most interesting little work.

Dr. Braid remarks, that he has examined analogous results with so many patients, as to assure him of the *reality* of the phenomena that have been alluded to in the former portion of this paper and to warrant him in drawing the following conclusions or inferences: 1st. That the effect of a continued fixation of the mental and visual eye in the manner, and with the concomitant circumstances pointed out, is to throw the nervous system into a new condition, accompanied with a state of somnolence and a capacity according to the mode of management of exhibiting a variety of phenomena, very

different from those we obtain, either in ordinary sleep or during the waking condition. 2nd. That there is at first a state of high excitement of all the organs of special sense, sight excepted, and a great increase of muscular power; and that the senses afterwards become torpid in a much greater degree than occurs in natural sleep. 3rd. That in this condition we have the power of directing or concentrating nervous energy, raising or decreasing it in a remarkable degree, at will, locally or generally. 4th. That in this state, we have the power of exciting or depressing the force and frequency of the heart's action, and the state of the circulation, locally or generally, in a surprising degree. 5th. That whilst in this condition, we have the power of regulating and controlling muscular tone and energy to a remarkable extent. 6th. That we also thus acquire a power of producing rapid and important changes in the state of the capillary circulation and of the whole of the secretions and excretions of the body, as proved by the application of chemical tests. 7th. That this power can be beneficially directed to the cure of a variety of diseases, which are most intractable, or altogether incurable, by ordinary treatment. 8th. That this agency may be rendered available in moderating or entirely preventing the pain incident in surgical operations. 9th. That during hypnotism, by manipulating the cranium and face, we can excite certain mental and bodily manifestations, according to the parts touched.

He appeals confidently to many professional and scientific men, who saw the patients; the diseases with which they were affected, the modes of operating, and the result which followed, as to the accuracy and fidelity of most of the cases reported. In reference to the cataleptiform condition, we present Dr. Braid's views, which he offers only by way of conjecture; each must judge of their plausibility, for himself. "Muscular contraction or motion is voluntary or involuntary. The voluntary arises from a mandate of the mind, proceeding from the brain, and effecting contraction or shortening of the muscular fibres; the involuntary or reflex, from irritation conveyed to the spinal cord, producing a like result,

and may be excited by tickling, pricking or pinching the skin of the extremities of a decapitated or pithed animal. It appears to me, however, that much of the efficiency and tendency to muscular contraction is dependent on another cause, namely, the state of tone or tension of the muscles when considered to be in a state of quiescence; and this state of tone I consider depends on the ganglionic system of nerves. Supposing from deficiency of this, the muscular system is relaxed a morbid tendency to reflex action will be induced, as a musical string will be more easily excited to vibrate if *moderately* tight, than if drawn very tense. It will also render muscular effort less efficient and certain; because part of the muscular contraction, which would have been efficient as available force or motion, will be expanded in bringing up the muscular structure to that state, which ought to have been its *normal condition* of tension or tone. On the other hand, supposing the organic system to have been extremely active, and rendered the muscular tone *abnormally great*, it will produce the very reverse effect, of that just referred to. It will not only offer resistance to reflex motion, but also to voluntary motion; and, if carried to a certain extent, may render the parts fixed and rigid, from the ganglionic system overpowering the cerebro-spinal system. This seems to be more than mere hypothesis, from the fact that muscles which had lost all power of motion, because of rigidity of contraction, had their motion restored by cutting them and allowing them to grow between the divided cords, thus being elongated; and also muscles which were paralyzed from relaxation had their power restored by cutting out transverse portions of the *tendons*, bringing the ends together and thereby giving them artificially that tension—the want of which I regard as the cause of the continuance of the paralysis. These cases were reported in the Edinburgh Medical and Surgical Journal, for October, 1841. It therefore appears to me, that during the hypnotic state, there is a complete inversion of the ordinary condition, and that the force of the ganglionic system becomes predominant, instead of being as usual, only subordinate. Another argument in

favor of this view, is the well known fact, that all voluntary motion or reflex muscular action, speedily exhausts the powers, and renders the subject unable to combine such efforts, and fatigued in consequence of them. Voluntary efforts are also *strongest at first*, and gradually become weaker. The functions of the organic system of nerves, on the contrary, are more equable and persistent in their nature; and although they may be influenced in some degree as to the activity of their functions, by directing attention in a particular manner, as the secretion of saliva by thinking of food, the secretion of milk by the nurse thinking of her child, &c., &c., still they cannot be said to be under voluntary control in the same direct manner and degree as muscular motion. The cataleptiform state induced by hypnotism comes on gradually. For some time voluntary power predominates; but at length the involuntary rigidity, or organic tonicity gains the ascendency; and although persisted in for a great length of time, is followed by no exhaustion or fatigue. On the contrary, so far as I have carried the experiments, the whole function seems to be invigorated by the continuance of this condition."

From a careful analysis of all of his experiments, Dr. Braid is convinced that it is a law of the animal economy that by a continued fixation of the mental and visual eye on any object which is not of itself of an exciting nature, with absolute repose of body, and general quietude, they become wearied; and, provided the patients rather favor than resist the feeling of stupor of which they will soon experience the tendency to creep upon them, during such experiments a state of somnolency is induced, accompanied with that condition of the brain and nervous system generally, which renders the patient liable to be affected, according to the mode of manipulating, so as to exhibit the hypnotic phenomena. It is probably not so much the *optic* nerve that is affected, as the sentient, motor, and sympathetic nerves, and the mind through which the impression is made, since the experiment succeeds with the blind. So throughly convinced is Dr. Braid, that it is, a law of the animal economy that such effects should follow such condition of mind and body,

that he does not hesitate to give it as his opinion, *that it is a fact*, which can not be controverted. And he concludes by remarking that "as to the *modus operandi*, we may never be able to account for that in a manner to satisfy all objections; but neither can we tell why the law of gravitation should act as experience has taught us it *does* act. Still, as our ignorance of the cause of gravitation acting as it is known to do does not prevent us profiting by the accumulation of the facts known as to its results, so ought not our ignorance of the *whole* laws of the hypnotic state, prevent our studying it practically, and applying it beneficially, when we have the power of doing so." We append several examples of curious nervous diseases, or conditions of the nervous system with which we have been favored by a medical friend. It is hoped that their applicability to the subject under consideration, will be so obviously apparent as to preclude the necessity of doing more than detail the cases merely.

The first was a case exhibited at a Clinical Lecture of Dr. Detmold's in New York, in 1847, of a girl who would writhe as if in convulsions, when ever told to thread a needle or to perform any other act requiring minute and fixed attention of the mind. Several minutes would elapse before she recovered. During the paroxysm her body was contorted as if in convulsions. Another case very similar to this, was seen in the service of Mr. Trousseau at the hospital for children in Paris, in 1853. This patient, also a little girl, could be thrown into a state analogous to the first by Prof. Trousseau approaching her for the purpose of examination, or upon requiring her to perform any little manual operation requiring the fixed attention of the mind. Her attack, thus brought on, would last for a few minutes, during which she seemed unconscious and appeared to be suffering severely from the convulsion affecting her face and limbs. Another case is that of Mrs. ——— residing in one of our Parishes, aged fifty; she was liable to fall into a state very much resembling that of the patients above alluded to, and upon similar provocations. She was otherwise a woman of strong mind, and great decision of character.

Several examples might be cited of quasi-epileptic, and cataleptic attacks where the patients though unable to move were still conscious. During the time (1849) that the gentleman who has furnished us with these cases was in charge of the Charleston Almshouse, there was an inmate affected with a nervous disease presenting some unusual manifestations. He would start to go forward by a sudden and vigorous effort, then retrace his steps some distance, and repeat the effort with similar retrograde movements, seemingly entirely beyond his control; always managing to reach the point proposed by the faculty he possessed of advancing some distance farther than he was subsequently forced to retreat. His movements seemed to be effected as it were *per saltum*, under great apparent excitement, and as if the least progression required all the concentrated force of his mind, in order to succeed. He would start forward suddenly and move with great quickness, stop and retrace his steps more slowly, to start forward again as before.

We have concluded now what we have to say of hypnotism. We trust that we have shown clearly enough, that the condition of the mind, and the phenomena attendant thereon, is not impossible, but may very rationally be induced by the mode adopted and be satisfactorily accounted for by the complexity of the nervous system, and is moreover, quite analogous to what is frequently observed in diseased constitutions, and compatible with our present knowledge of nervous action, &c.

Investigation and experiment must satisfy each for himself of its utility as a remedial agent. And, indeed, the whole subject is entitled to that judgment which *examination alone* is competent to award. Let us be careful not to forget that the eminence of its author, the scientific manner in which it is treated, and the beneficial results which it promises, entitle the subject to our consideration. Let us not be prejudiced, and hastily pronounce it superstition and folly, an imposition and hoax, but be content to draw useful lessons of wisdom from the cases like Harvey's, or earlier, Galileo's, in which the most brilliant discoveries, because not

in accordance with established dogmas and popular modes of thinking, were consigned to public derision, but finally triumphed over all opposition, outlived the contempt and obloquy of the ignorant, and remain, and will be handed down from generation to generation the noblest monuments of genius and industry. It would be well for us to remember the just remark of Dugald Stewart: "Unlimited scepticism is equally the child of imbecility as implicit credulity."

ART. III.—*Counter-Extension with Adhesive Plaster.* By J. McF. GASTON, M.D., Columbia, S. C.

The use of adhesive plaster for the purpose of extension is generally and favorably known to the profession, but its application as a measure of counter-extension requires to be brought more fully to the attention of practitioners, and a case of fracture of the femur, which has been treated chiefly by this process, may serve to illustrate this procedure.

Rufus, a servant of Mr. John Davis, was placed under our care on June 16th, and upon a special examination with my partner, Dr. A. N. Talley, a comminuted fracture of the lower third of the shaft of the femur was discovered. With the assistance of two of our students, extension was effected, and after coaptation of the fragments, the roller bandage was applied to the entire limb. Three splints were next fitted to the thigh, and Welch's long extension apparatus was then adjusted by a gaiter, to secure the ankle to the foot board beneath, and a silk handkerchief, wrapped around a roll of cotton as a perineal bandage, tied firmly to the upper end of the long splint above the crest of the ileum. By means of a screw connected with the apparatus, extension was now made, until the measurement from the anterior spinous process of the ileum to the internal malleolus was found to correspond to that of the sound limb.

The muscular development of the thigh afforded great resistance, yet the extension was steadily maintained for a week, when there was such excoriation from the perineal bandage as to induce a discontinuance of this mode of counter-extension for the application of adhesive plaster, on the principle, but not precisely in the place, suggested by Dr. Gilbert, of Philadelphia.

A strip of strong linen plaster, two inches wide, and two feet in length, was doubled upon itself at an angle, so as to make a loop, and yet both of the extremities having their adhering surfaces in the same direction. One of the free ends was pasted closely over the spinous process of the ileum, and extended down towards the inguinal region; while the other end was carried down and pasted over the trochanter, so that both pieces had firm attachments, by the *closeness* of the *integument* to *these bony prominences*, and thus presented resistance to traction upon the loop above. Transverse strips were placed from one piece to the other to secure their adhesion. A piece of homespun was now passed through this loop and made fast to the upper end of the long splint, thus effecting counter-extension. When the apparatus was tightened by the screw it was found that all the indications were met satisfactorily, while the excoriated hip and the perineum were left entirely free for such applications as the ulceration required. The strips adhered closely for three weeks without interference, and have proven an efficient means of counter-extension.

Those who may be interested in this subject will find a well written article on the adaptation of adhesive plasters to counter-extension, by Dr. D. Gilbert, in the April number for this year of the American Journal of Medical Sciences, in which he details his mode of application and the principles involved in their use; while other articles from his pen may be found in the numbers for January of 1851 and 1858, in the same journal.

The only particular in which my proceeding is entitled to any superiority over the process of Dr. Gilbert is, that it makes the attachment of the strips extend over the firm,

unyielding points of bone, and thus serve, perhaps, to give a more fixed resistance to the force of extension. But while this is original with me, I prefer no claim which shall lessen the appreciation of the eminent service which has been rendered to the profession by the elaborate articles above referred to, and I trust that all may profit by their perusal, and cease to punish the subject of fracture with the excoriations of a perineal bandage.

In connection with this application of adhesive plaster, I would notice its use also in a case of fracture of the patella, which occurred in our practice recently. Being called on Feb. 7th, of this year, to a servant of Mr. Robert Bryce, Dr. Talley and I went together, and found a transverse fracture of the patella, with the upper fragment drawn up considerably by the contraction of the muscles. Bringing the fragments in apposition, a figure of eight bandage was first applied, but subsequently, at the suggestion of my brother, Dr. J. B. Gaston, of Montgomery, Alabama, we resorted to the use of adhesive strips, to keep the fragments in apposition. Three pieces of plaster, an inch wide and twelve inches long, were carried from above the knee round the inner side of the patella to the upper and inner part of the leg; while three other similar pieces were carried from above the knee around the outer side of the patella to the upper and outer portions of the leg, thus keeping down the upper fragment of bone securely. Another set of strips were attached in a semi-circular arrangement around the lower fragment, by which accurate apposition of the fragments was effectually maintained. This plan of treatment is attributed to Dr. Neill, and Erichson makes reference to it in his work on Surgery. Our case progressed favorably, and though the union is not osseous, the connecting layer of cartilage is perhaps shorter and firmer than is usual in this accident, and does not interfere at all with locomotion.

In a fracture of a similar kind, which I treated some years ago, the case was managed throughout by the figure of eight bandages, with a good result as to the union of the fragments;

but I had to keep the entire leg and thigh encircled with a roller,—the latter to control the action of the muscles, and the former to suppress the swelling which would have ensued from the obstruction to blood vessels and absorbents at the knee. This difficulty is entirely obviated in using the adhesive plaster, as it extends only partially around the limb, and leaving that region free where most of the vessels are located. And the same views hold in reference to their application to the upper and outer part of the thigh, instead of extending round the inner part, where the vessels and nerves are located.

Enough has been said to indicate the advantages of the adhesive strips in this class of cases, but at a future day I propose to give my personal experience in the treatment of inflammation by the compression of adhesive plasters.

ART. IV.—*London Hospitals and Lecturers.* By our Foreign Correspondent, J. J. CHISOLM, M.D., Professor of Surgery in the Medical College of the State of South Carolina.

Among the interesting sights of London none possess greater attractions for the physician than the Orthopœdic Hospital, where the maimed and halt of the kingdom are collected for treatment. My first visit was one of surprise. I had to push my way among a crowd of unfortunates, who thronged the vestibule, before I could reach the Surgery, where two of the surgeons, Mr. Tamplin and Mr. Coutts, were despatching business with American speed. Deformities of the legs and spine abounded, and as this was the routine day after day, these surgeons had acquired such facility of diagnosis that it seldom required a second glance to detect the disease and prescribe appropriate treatment. Mr. Ferguson, the instrument maker, is always in readiness, either to supply the wants of the patients or to

suggest such modifications in the supporting apparatus as any special case might require.

Although every confidence is placed in gradual extension for contracted limbs, as in false anchylosis, whether from adhesions, ligamentous or tendinous contractions, yet no attempts are made to cure the varieties of club-foot, however trivial, without section; experience having proved all efforts at overcoming such deformities by mechanical means alone, to be futile. Patients are operated upon at any time after the month, the younger, the better result. There is a very excellent and simple splint in use at this hospital, composed of a slip of tin varying in length and width, padded with cotton and covered with cloth. It is readily bent to any angle, and will ensure, whilst giving comfortable support, sufficient immobility to a limb. It is the sole apparatus applied after section of tendons, and is retained until union in the tendon is effected, when extending apparatus is called for.

A very common deformity in children which the surgeons of this hospital are called upon to remedy is knock-knees, some of them, as you may imagine, of the most *outre* order. Any degree of this deformity, provided the patient be young, is removed without difficulty by a long splint on the outside of the leg, extending from the trunk to beyond the external maleolus.

In redressing curvatures of the spine, pressure is applied over the broad surface of the ribs. I saw a case of much interest in a woman thirty-five years of age, who had been bed-ridden for seven years, with complete paraplegia, resulting from excessive curvature and pressure of the cord to such an extent as nearly to stop all function. By the judicious application of mechanical support, with counter-pressure, so as gradually to straighten the spine, she had so far regained the use of her limbs that she could sit up, and, when assisted, could support herself upon her feet and walk a little; a sufficient guarantee of final success, if the course of mechanical treatment be carried out.

We all know how unsatisfactory are the results of opera-

tions for the relief of those shocking deformities which follow burns. If the bands be cut up and the limb straightened the contraction rapidly returns. Here, by the *very gradual* application of an extending force, the cicatricial tissue is made to yield, and in time becomes so lax as to allow not only the limb assuming its normal position, but also of performing all of its original motions. I saw an exemplification of this kind of treatment by Mr. Coutts, at St. Bartholomew's Hospital. The case was just such an one as is placed for a frontispiece to a chapter upon burns,—one in which the chin had been fastened to the sternum by one sheet of anterior and bilateral cicatrices. By the application of a lever on either side of the cheek, working from a spinal bar as a fulcrum, the chin was gradually lifted from the sternum, and is now standing at right angles to the trunk. The extension must be very gradual, otherwise the cicatricial tissue will ulcerate, a troublesome accident, for all treatment must be stopped until such ulcers heal. Months are required to correct such deformities.

I attended the hospital on Wednesday, their operating day, and had the gratification of seeing the entire staff, composed of Mr. Tamplin, Coutts, Adams, and Broadhurst, operate. There were many cases requiring section of the tibial tendons, also of the tendo achillis. Two required section of the plantar facia to unroll an excessive puckering of the sole. In one case Mr. Broadhurst divided the rectus femoris, which he thought, by its shortening, prevented the knee from being flexed. In very young infants the three tendons are divided at the same time, although traction is never made upon the tendo achillis, until both tibials have been sufficiently elongated to allow the foot's retaining its normal straight position. Many failures arise from attempting too much at one time.

At a lecture which I heard in this institution upon deformities of the hip joint, resulting from acute rheumatism, the lecturer spoke of jugulating an acute attack in twenty-four hours by the lancet and calomel. This was an individual opinion, not sustained by his colleagues. It is needless to say

that this gentleman had been brought up in the old school.

At St. Bartholomews, I had the pleasure of meeting some of the Nestors in surgery, among them Lawrence, Stanley and Skey, and at King's College Hospital, saw Mr. Fergusson operate very skilfully upon several cases. Chloroform was freely used at all the public institutions which I visited; at some with inhalors, at others upon folded pocket-handkerchiefs. It appears that the object of using the inhaling apparatus, is more to answer the annoying inquiries of the jury, should any accident unavoidably happen, than from any security which it may possess; æther, I have not seen used. Chloroform is always administered by one physician attached to the Hospital, who is not interested in surgery, so that his attention is never called off from his duty by curiosity.

I have attended a portion of a course of lectures, at the College of Surgery, upon bone, by Mr. Quekett. They are delivered solely to members of the College, who have become by preparatory studies full surgeons. They are replete with interest. His subterranean Amphitheatre will not accommodate more than fifty auditors, and is well lit with gas. He adopts the following excellent arrangement: The entire front wall is covered with diagrams, colossal figures of microscopic specimens in his possession. In front of him is a long table, upon which stand ten of Beck and Smith's best microscopes, each having a stand for itself, together with an argand burner and a bull's eye condenser. Also a card naming the specimen under observation, and the magnifying power. Immediately in front of each row of seats, is a semi-circular rail-way connected by wings to his table, the central depot; and as a specimen is lectured upon, or a portion of the lecture requires elucidation, an attendant rolls the microscopes upon this rail, upon which they are rolled from one to another, until, without noise or confusion, the entire assembly have examined for themselves the natural specimens, drawings of which they had previously seen. Mr. Quekett is by no means a brilliant lecturer, in facility of utterance, all the English Medical Lecturers that I have heard, are deficient, words come to him slowly and are uttered with hesitation,

but every thing that he says is worthy of remembrance and can be treasured up as the result of his experience. He has in his possession nearly twenty thousand microscopic specimens, explanatory of Histology, both of vegetable and animal tissues, perhaps much the largest collection in Europe. We had him as our guide through the celebrated Hunterian Museum, pointing out to us the many gems which that collection possesses. The museum contains many very valuable duplicates of antideluvial mammalia which Mr. Quekett offered to exchange for sirens, boney pikes, &c., which are very common with us, but are considered very valuable here, as filling that link in the chain between fossil and recent classes. We saw here a curious specimen of a sea crab with an oyster of at least four years growth upon its back, a proof that the presence of a parasite prevents the annual exuviation of the shell of such crustacea.

As regards general medicine and surgery, we find that we are quite as advanced as the English, and that they have very little to teach us. Orthopœdic surgery is a comparatively new field, and as their Hospital is an extensive one, much material is there collected, and upon this branch alone much information can be gained. A larger number of cases can be seen there in one day than a surgeon in general practice will see in his life time. In other respects, New York already offers every inducement which London possesses, with the additional advantage of being much nearer home, with much less expensive living. Their hospitals are just as well endowed, contents as varied and as interesting, and officials much more communicative, and willing to give information. The medical Colleges here are very numerous, but the classes are small when compared to our own, if I may judge from the numbers I saw attending the leading men of the day. Microscopic studies are very general and every student is expected to be familiar with the uses of the instrument. There has just been established here a very excellent institution, offering facilities for the pursuit of microscopic investigations at a small cost. By the payment of a few shillings a year, a student has the free use of a room in which microscopes are

kept mounted, with specimens of every variety as well as a library of all the works on microscopy. A medical circulating library, has also just been established. We frequently meet medical men here from America, who like ourselves are on a tour of observation, and take much pleasure in comparing the result of our experience. One of the most curious incidents of the medical profession here, is a pamphlet from one of the oldest and most worthy Physicians of London, giving in his adhesion to Homœopathy as the proper system of medication, and this as the unbiased result of his long experience. A notable instance of a physician who had succeeded to the utmost of his ambition, as an allopath, turning back upon his alma mater, when his sands were just about running out.

REVIEWS.

ART. V.—*Elements of Medicine; a compendious view of Pathology and Therapeutics; or the history and treatment of diseases.* By SAMUEL HENRY DICKSON, M.D., L.L.D., Professor of the Practice of Physic in Jefferson Medical College, Philadelphia, etc. Second Edition, revised, Philadelphia: Blanchard & Lea, 1859.

"A book should be judged and appreciated with some direct reference to its declared purpose. The present volume is intended as an aid to young men who have engaged in the study of medicine, to physicians who have recently assumed the responsibilities of practice, and to my fellow-professors of the institutes of Medicine and private instructors, who have felt the difficulty of communicating to the first two classes the knowledge which they are earnestly seeking to acquire,"—so says the author in his preface to the first edition. Judging by this standard, we have on a former occasion expressed our high opinion of the merits of the work, and predicted for it a favorable reception, by both students and teachers. Judging, again by this standard, we are sure that the favorable reception it has met with will be continued, for this second edition, carefully revised as it is, has been rendered still more worthy of it than its predecessor.

As the volume on its appearance was reviewed at some length, we shall not now make any attempt to follow the author through its various sections, but shall content ourselves with noticing some points that have struck us in looking through its pages.

The book is divided into two parts, the first of which

treats of general Pathology. This first part is eminently to our liking. In it speaks the medical philosopher, and not obstrusely, but in language clear and concise; not heaping together learning only to bewilder, but using it to enforce an argument, and to stimulate the mind to further thought.

Respecting the primary seat of disease, Dr. Dickson, after referring to the conflicting views of the old humoralists and solidists, believes himself led to the conclusion, that the morbid action originates in the blood and nervous tissue. Now the whole question of the primary seat of disease—once a source of so many angry words and of so much bitter jeoling between these rival schools—has lost a great deal of the interest it possessed and of the importance formerly attached to it, and that mainly because modern investigation has swept away most of the old landmarks, by showing disease to be a compound, in which both fluids and solids have their share. Then again the medical men of this generation are disciples of a positive philosophy. They care little about the origin of matters, unless susceptible of absolute proof. The *whys* and the *wherefores* are not often made the subject of serious study, unless it is possible to do so by the aid of the experimental sciences, and the doctor of the present age takes things as they are, treats them according to what he believes to be the most successful plan, without troubling himself much about the causes which are supposed to have called them into existence. Very little attention is thus paid to the school of Medicine, as based upon particular theories of disease. And we know not indeed a more puzzling question that could be addressed to any one, than to inquire of a practising physician, what theory of disease he practices on, and of what doctrinal-school he is the disciple.

After what we have just said, we are not going to quarrel with Dr. Dickson, because he places the primary seat of disease in the blood and nervous system. But we would ask, whence is a large portion of the blood derived? Is it not from material elaborated by cells? What lies at the foundation of nearly all the tissues? Is it not cells? What secretes? cells. Hence, it does seem to us requisite, that the action

of the cells should not be completely passed over in any theory of primary morbid changes, and while cell-physiology plays so prominent a part in the explanation of the phenomena of health, cell-pathology should not be as thoroughly ignored, as it usually is, in explaining the phenomena of disease.

Dr. Dickson has retained in this edition the same classification as in the last; that is, he treats of diseases according to the systems they involve and the functions they disturb. Thus are separately discussed those of the Circulatory or Vascular system, including the organs of secretion and absorption, as well as of sanguineous distribution and nutrition: of the digestive system, embracing the collatitious viscera and those engaged directly in the solution and conversion of food: of the Respiratory system and of the Voice: of the Sensorial system: of the Motory system: of the Excretory system, under which head the affections of the cutaneous integuments and the diseases of the urinary organs are set forth: and of the Generative system. This physiological mode of classification is by no means faultless; what mode of classification is? It does not answer well for clinical teaching, but for didactic lectures, or in a book intended for a guide for students it is probably on the whole the best. One objection to it is, that it renders it very difficult to dispose of disorders of uncertain seat. Thus with fevers. They are placed among the diseases of the circulatory system, and yet it can hardly be said that they are diseases of that system. Still we are quite willing to acknowledge that it would be as objectionable if they were placed under the disorders of any other system. For our present knowledge of the pathology of fever is lamentably deficient, and although we can very well appreciate the symptoms of a fever, the hot skin, excited pulse, the disturbance of the secretions and of the nervous system, what can we tell of its real nature?

The individual fevers are admirably discussed by Dr. Dickson. They were well treated of in the first edition, and hardly anything remained to be said. Whatever more recent research or further reflection on the part of the author has enabled him to add, has been added, and we have no hesi-

tation in stating, that the chapter on fevers as it now stands is not only the best in any Manual on the Practice of Medicine with which we are acquainted, but is invaluable to the American Medical Student, since it gives him an excellent delineation drawn from observation, by a skilful hand, of those very diseases which are especially met with in this country, and in combating which his life will be spent.

Regarding typhoid fever, Dr. Dickson has still some doubt as to the propriety of separating it from typhus. He does so in his description, but evidently more in compliance with almost universal American usage, than from any very strong conviction. "While I approve highly of the disposition of modern pathologists, to investigate closely, and to draw nice distinctions, I still observe some caution, and wait until uniform and characteristic differences have been noted, before I would separate in place and name, diseases obviously connate and closely allied, as I consider the several varieties of typhus to be." The question is indeed, as yet, far from being satisfactorily settled. There are many physicians enjoying most favorable opportunities for studying, both typhoid and typhus fever, who believe in their identity. On the whole however, the medical mind is admitting more and more a distinction between these two low forms of continued fever. Dr. Watson in the last edition of his lectures, gives in his adherence to this view, evidently gained over to it by the admirable researches of "the Dr. Jenner of our time."

Influenza is spoken of by Dr. Dickson, and we think, with great justice, as a distinct disease, under the name of catarrhal fever. Certainly, any one who reflects on its varying phenomena, on the occasional absence of bronchitis and catarrh; on its attacking at times only the mucous lining of the frontal sinuses and of the nose; on the intestinal irritation so often conjoined to it, or alternating with it; on the swelling of the joints that sometimes accompanies it, must admit that this is far more philosophical, than representing it, as is commonly done, as a variety of bronchitis. But the very same reason which makes us approve of this view of the subject, forces us to take issue with our author on the rather anoma-

lous position in which we find typhoid pneumonia. It is classed as an idiopathic fever; we are told that it is not a mere pneumonia; that it is an error so to regard it; that the affection of the respiratory organs may be occasionally wanting, and inflammations and congestions of other parts substituted; and that the peculiar and characteristic disturbance of the sensorial and circulatory system has been, in a very numerous order of instances, of such paramount importance, as to have determined promptly the fatal event. We will willingly grant the latter; we will grant that typhoid pneumonia is not a mere pneumonia; we will grant that it may present complications of a very serious nature; but we cannot grant that a disease in which the respiratory organs are not involved is a pneumonia, or ought to be so called. There may be a form of fever, in which the lungs are specially prone to be implicated, although they are not so of necessity —such a form as we know that sometimes both typhoid and typhus fever assume—but this is not the same disease, as we see occurring in epidemics, in which the lungs are primarily affected, and which from some cause unknown to us is marked by a rapid failing of the vital powers; or which is equally met with at times as a consequence of phlebitis, or as supervening in cases of erysipelas or of delirium tremens. Any form of pneumonia of a low type we are justified in calling typhoid pneumonia, but we cannot speak of a typhoid pneumonia, where no disease of the lung is present.

Dropsies have received from Professor Dickson their full share of consideration, and we find many very valuable remarks on their treatment. Thus, to quote one on the use of diuretics:

"It is a fact which should be kept steadily in view throughout all our discussions on this subject, that all remedies which tend to bring back the system to a healthy and natural state, act in dropsy as diuretics. Those which reduce action when too high, and those which stimulate when it is defective; those which affect favorably the general constitution, and those which address themselves to particular organs whose functions are impaired, operate in a like manner.

Mercury is often our best diuretic. Emetics sometimes

produce large urinary discharges; so do cathartics. Bark and Port Wine have been employed with the same effects."

This is assuredly a great deal of sound doctrine, compressed into a very small space.

The chapters on the diseases of the heart and lungs have been in several respects altered by the author. So has the one on the affections of the kidneys. The description of Bright's diseases is quite in accordance with the more recent views of pathology, but the importance of the subject is such that we should like to have seen it more extensively considered. Addison's disease has received a brief, but clear notice. The chapter on the affections of the nervous system is very complete. We find all that modern observation has added to our knowledge of that fearful malady epilepsy, taken cognizance of. We say fearful, not so much on account of the danger of the fits, as of the wreck they commonly make of the intellect. It is true we hear of men of genius who have been subject to epileptic attacks, we are told that "Mahomed, Napoleon, Cæsar and probably Alfred the Great," retained in spite of them, their mental powers unimpaired. Yet, without wishing rudely to deprive any patient of the consolation, which reading that he has had such co-sufferers must afford, we are compelled to state that the evidences of these distinguished men having been epileptics, is far from being satisfactory. Of Alfred's epilepsy, we know nothing positive. Napoleon, we are given to understand by the gossips of his court, had epileptic convulsions whilst attempting sexual intercourse, but we are not, concerning them, in possession of any information of a more reliable kind. And Cæsar, whose varied talents would make his case such a particular comfort to epileptics—Cæsar, the statesman, the general, the jurist, the orator, the philologer, the mathematician, the poet, the historian, was but twice attacked with fits; which, as they occured in the excitement of transacting public business, and in one whose constitution was originally so delicate, we may well hesitate to accept as conclusive proof of his having been a sufferer from epilepsy.

With these rather desultory remarks, we take leave of our

author, assuring him that in reading his work we have been struck by the difference it presents from the many dry dogmatic compilations on the same subject. It exhibits not only the accomplished physician, but justice demands we should add, that the elegance and good taste of the style show alike the scholar and the refined gentleman.

ART. VI.—*A Treatise on Medical Electricity. Theoretical and Practical, and its rise in the treatment of Paralysis, Neuralgia and other Diseases.* By J. ALTHAUS, M. D. London, 1859, 8 vo., pp. 352.

When Thales more than two thousand years ago discovered the existence of electricity, by rubbing amber with a dry cloth, and showed the wondering Greeks how, when thus excited, it gained the remarkable faculty of attraction, he nor they little knew the mighty power of the principle he was invoking, or conceived, that centuries after their time it would be the subject of speculation and experiment, for the wisest philosophers of all sects and nations. Yet so it is, with the developement of the human mind, the progress of science, and the consequent understanding we have obtained of so many mysteries and laws of nature, the subject of electricity has been most thoroughly investigated; so that when we consider the depth to which the mind of man has penetrated in this direction, and the noble results he has obtained, we may well experience a feeling of awe at the contemplation of his labor, and question the philosophy of those who would place a limit to his reason.

It would be impossible without occupying more space than could really be accorded in a Journal where all departments of medicine are considered, to enter into the history of electricity, from the period we have mentioned, to our own day, and even if it were perfectly compatible with the condi-

tion we have specified, we should be treading ground familiar to most of our readers. In the advance which has taken place, however, we cannot over-look the fact that our own countrymen have performed their part, and that many of the most important acknowledged facts were established through their labors, and several most useful applications of the powerful agent under consideration, derived through their ingenuity.

Nevertheless, whilst claiming for our philosophers the credit which is their due, we must in candor admit that in its physiological and therapeutical relations, electrical science has gained little or nothing through our researches. In fact, we seek in vain for one important point established by us in these departments, for beyond the ordinary experimental applications of electricity and its empirical or speculative employment in the treatment of disease, when other remedies have failed, our physiologists and physicians leave to those of other countries the reputation and honor to be gained, by original investigation in this important branch of science.

We wonder at this, with so broad a field for research, with so many valuable results to be obtained, and with so active a corps of physiologists and physicians, who are constantly adding to our knowledge of other subjects connected with the science of medicine; it is indeed strange, that not one has been tempted to enter the lists, with Matteucci, duBois Reymond, Duchenne, Marshall Hall, Todd, Golding Bird and many others, whose investigations have added so much to their own renown, the cause of science and the good of the human species. We can but regret the existence of such a state of indifference, and express the hope that the day is not far distant when our remarks will be no longer applicable.

A work which would place before the English reader, the main facts established in electrical science, so far as pertains to it in its medical relations has long been wanted. Other languages have not been deficient in treatises of this character, but so far as we are aware, no work covering the whole ground has, prior to the publication of Dr. Althaus' volume, issued, either from the English or American press. Aside,

therefore, from any intrinsic merits of the book before us, the fact that it is the pioneer in this species of literature, to which it is devoted, should ensure for it the attention and respectful consideration of the medical profession.

In the first chapter of his treatise, Dr. Althaus gives a summary of the different forms of electricity; these he arranges under two grand divisions: statical, comprehending that produced by friction of amber, glass, wax, &c., and dynamical, comprising galvanism, electro-magnetism, and animal electricity. The various peculiarities of these several modifications of electricity are satisfactorily pointed out. The chapter however, is not so complete as could be desired, from the fact that atmospheric electricity is not considered here—it being degraded to the appendix, and the very interesting form developed by heat, thermo-electricity, is entirely overlooked. With these exceptions, Dr. Althaus has done sufficient justice to this portion of his subject. The remarks upon induction currents are especially to be commended for their perspicuity and completeness.

In the second chapter *electro-physiology* is considered. Under this head the physiological effects produced by the application of electricity to the different tissues of the living body, in their normal condition, are treated of in detail. The important discoveries of Galvani, Volta, duBois Reymond, Nobili, Bernard and others, are stated with sufficient fulness, but there is we think a disposition to underrate the investigations of Matteucci. Whatever errors this great physiologist and physicist may have committed (and what investigator can plead guiltless on this score,) the numerous electrological contributions which we owe to his industry and genius, must ever command for him a high rank among original observers, and as the suggestor of many of those experiments upon which the reputation of duBois Reymond is built, his labors are entitled to more consideration than Dr. Althaus seems disposed to bestow. Matteucci's last publication "*coursd' Electro-Physiologie,*" in which many of his earlier investigations are supported by new and more perfectly devised experiments, appears to be entirely unknown to Dr. Althaus, although

issued at a period considerably anterior to several from which he has quoted.

Without an extensive acquaintance with the effects following the application of electricity, to the several parts of the body in a state of health, it is impossible for the physician to make use of this agent with that intelligent and philosophical spirit, which should govern him when possible in the administration of his remedies. Thus if employed in too great quantity the animal tissues may be entirely decomposed through chemical action and of course the most violent effects produced. The *tension* of the current is also to be considered. When transmitted of proper intensity, electricity is capable of restoring the lost physiological function of either a muscle or nerve, but if a current of too great tension be employed, the natural action of these organs may be entirely destroyed. Again the *direction* of the current is also an important point. If passed from the periphery to the centre of the nervous system, the vital property is increased, but if transmitted in the opposite direction, the vitality of the nerves through which it passes becomes exhausted.

We might adduce other circumstances requiring consideration. The slightest reflection however upon those we have specified, will be sufficient to convince any one how necessary it is that the physician should be guided by a knowledge of the subject before venturing to make use of electricity as a therapeutical agent, and may serve to explain the failures of the ignorant and pretenders, who fancy they have nothing to do but to transmit a current of any kind of electricity in any manner, in order to cure the still more ignorant invalids who confide in them.

We cannot therefore insist too strongly on the study of physiological electricity, and we do not know of a better summary of what is known on the subject, than that contained in Dr. Althaus' treatise.

In the third chapter, the *various forms of electric apparatus* suitable for therapeutical purposes are considered. These are all very properly arranged into three classes, the *electric*, properly so called, serving for the production of statical elec-

tricity, the *galvanic* by which constant currents of dynamical electricity are obtained, and the *induction* by which localised induction currents are applied—Faradisation, as the principle has been called after its illustrious discoverer. In addition to descriptions of the several kinds of apparatus, the methods of using them, and the principles which govern their application, are also stated in detail. This chapter is very satisfactorily written.

The fourth chapter, relates to *electricity as a means* of diagnosis. In this relation electricity is mainly useful, as a means of distinguishing between several obscure forms of paralysis. Dr. Althaus furnishes a very good outline of all that is known on this subject.

The fifth and concluding chapter, treats of *electro-therapeutics*, or the application of electricity to the treatment of disease. The use of this agent in paralysis and neuralgia, is fully considered and many cases of these diseases are adduced, in which cures were effected by its employment. Dr. Althaus contends for the exclusive use of induction currents in the treatment of paralysis. In this he is undoubtedly borne out by the electro-physiological law, established by duBois Reymond, viz: that the motor nerves are not excited by the absolute amount of the density of the current, but merely by the variations which occur in the density of the current from one instant to the other, and that the amount of excitation caused is proportionate to the rapidity with which these changes take place. Tho weight of authority is certainly with Dr. Althaus, though both Bird and Remak, regard the continuous current as the only remedial form, in the treatment of paralytic affection.

In regard to the direction in which the current should be passed, there is great difference of opinion. In this respect in our opinion, the physician should be guided by physiology, the current being direct, that is, from the periphery to the centre, in a case of muscular paralysis, whilst in paralysis of sensation, it should be inverse, or from the centre to the periphery. In our own experience, we have always acted upon this principle, and we have not had occasion to be dis-

couraged with our success. Dr. Althaus appears to incline to the reverse course, though so far as we can perceive, from no correct idea. The assertion that in many cases of paralysis of motion, the inverse current produces more energetic contractions in the muscles than the direct, is not in accordance with our observations, nor with those of others, who have investigated this subject.

In hysterical aphonia the employment of Faradisation is almost certain to result in cure, if the affection be uncomplicated with other hysterical manifestations. Dr. Althaus of fifteen cases cured eleven, the other four resisted the remedy but they were attended with additional symptoms of nervous derangement. In our own practice, we have met with two cases in which this treatment proved entirely successful. In one the affection was of six months standing, the other was more recent. The latter yielded to a single application, the former was more resistant, requiring five applications at intervals of two or three days. In neither was there any relapse. The current was passed in the course of the recurrent laryngeal nerve.

In the treatment of neuralgia and rheumatism, electricity is frequently indicated, and may be used with success. We have no intention at this time of specifying all the diseases in which the agent under consideration may be used with a fair prospect of benefit. To do so would require more space than we have at our command. We will only therefore allude to a few cases, in which it has been used successfully in the United States, and, as far as we have been able to ascertain, prior to its employment, in similar cases elsewhere.

As early as 1841, Dr. Ruschenberger* of the United States Navy, succeeded in curing a case of hydrocele by galvanism, after injection with iodine, and the seton, had proved unsuccessful. Two acupuncture needles were introduced into the sac, and these connected with the poles of a Daniell's battery. The current was continued for half an hour. No subsequent

* Curling on the Testis. American Edition. edited by P. B. Goddard, M.D., 1843. Note by Editor.

application of the galvanism was required, and the fluid was speedily absorbed. Up to this period there has been no recurrence. Other cases have since (in 1846,) been treated successfully by Dr. Ruschenberger.*

To this able physician is also due the credit of first employing galvanism in the treatment of bed-sores. From a communication recently from him, we subjoin the following extract:

"The application of a galvanic pair to the treatment of bed-sores suggested itself to me in 1846. I had noticed several years previously that in treating neuralgia by Mr. Mansford's plan, i.e. removing the cuticle at two points by a small blister, and applying to the denuded surfaces, moistened buckskin, covered at one point by silver and at the other by a zinc plate, the two being connected by a silver wire, that whilst the surface under the zinc—the most oxydizable metal—ulcerated and some times sloughed, that under the silver rapidly healed.

I had the satisfaction to find that by raising a blister on the calf and applying a zinc plate over it, connected with a silver plate placed in the midst of a large and troublesome head-sore, on the back (two pieces of moistened buckskin being placed under each plate,) that the bed-sore previously unmanageable, was entirely healed at the end of less than three weeks. The plates were applied only five or six days. The sore on the leg from the blister was easily healed after discontinuing the zinc plate."

AnotherAmerican physician, Dr. LeConte, at present Professor of Physics at Columbia, South Carolina, has recorded† the case of a negro woman, who had not mensturated for seve-

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*We allude to these cases of Dr. Ruschenberger, more especially in view of the fact that a claim of priority has been recently set up, by M. Petrequin of Lyons, and Dr. Rodolfi of Lombardy, and a report of their cases published in a recent number of a prominent medical journal of this country, without the slightest reference to Dr. Ruchenberger's practice.

† Extraordinary Effects of a stroke of lightning. New York Journal of Medicine, old series, vol. 3, p. 295, et. seq. 1844.

ral years, having her catamenia perfectly restored, after being struck by lightning. Some cases are on record, where galvanism has proved highly serviceable in the treatment of amenorrhœa.

In the same paper Dr. LeConte relates the very interesting fact, that a woman, of the party with the one above mentioned, who had been always barren, being struck at the same time, soon afterwards became pregnant and was in due season delivered of a child.

In relation to the use of electricity, for the purpose of dissolving urinary calculi within the body, and the so called electro-chemical bath, by which it is contended certain poisonous minerals can be abstracted from the system, we must say that those who put faith in the various statements made in regard to the remarkable effects produced, are gifted with a much larger share of credulity, than we should care to acknowledge.

We must now bring our remarks to a close. We have perused this book with much pleasure, and would be glad to see it extensively circulated among the profession in this country, satisfied as we are of its comprehensiveness, and of the general correctness of the views which are expressed. It is only by the dissemination of works, such as that before us, that medical electricity can be taken entirely out of the hands of charlatans and placed upon its proper footing, as a therapeutical agent.

ART. VII.—*Anatomy, Descriptive and Surgical.* By HENRY GRAY, F.R.S., &c. The drawings by. H. V. CARTER, M.D., &c. The dissections jointly by the author and Dr. CARTER. With three hundred and sixty-three engravings on wood. Imp. 8 vo., pp. 754. Philadelphia: Blanchard & Lea, 1859.

There are some subjects which, either from their very nature, or from their present progressive state, can only be

written upon as it were transitionally. Such, in an eminent degree, is Physiology; the man who should write a treatise, professing to set forth this branch of medicine completely, would sadly disappoint any reader who might look for the fulfilment of his promise. Experiment and observation are ever bringing to light facts and conditions which modify some previously accepted doctrine in a greater or less degree; many points are still unsettled, and cannot be honestly made matters of assertion.

"Descriptive and Surgical Anatomy" presents a different aspect. Here we have to deal with facts as old as the race of man, facts which need only the accurate eye for their verification. For—whatever view may be taken upon the question once proposed for discussion in a medical society, "Had Adam and Eve navels?"—no one doubts that—perhaps, with this exception—a thorough anatomical book, such as the one before us, might have been founded upon a dissection of our parents, and their eldest-born. Normal Anatomy sets forth the type upon which all human frames are closely modelled; strictly speaking, it has nothing at all to do with function.

One would *a priori* have thought, therefore, that we had nothing more to expect in the way of anatomical works. And really it seemed as if Sharpey and Quain, Wilson, Morton, Richardson, and others of less note, had covered the whole ground so far as the text-books could go; that of the first-named might even take rank as a work of reference. Mr. Gray has however shown that what had been done well could be done better. Leaving, with great propriety, the discussion of microscopy and physiological anatomy for such works as those of Kolliker, or Todd and Bowman, he has bestowed his attention upon the "practical" portion of the subject; aiming "to furnish the student and practitioner with an accurate view of the anatomy of the human body, and more especially the application of this science to practical surgery." This task he has done well—better in our humble opinion than any of his predecessors.

The bones and ligaments are described at length, and illustrated, as indeed the whole work is, after the manner intro-

duced by Mr. Holden in his "Human Osteology;" that is to say, in each cut, each part is represented with its name marked upon it.

The muscles and fasciæ are treated of conjointly, and we think with great propriety. Few students can remember the arrangement of the fasciæ as distinct from the muscles; it is in fact only in connection with the latter, as a general rule, that the former have any importance. First the bony points, and next the muscular prominences, must be our landmarks in searching for an artery, or in any other surgical exploration; these being thoroughly understood, the fasciæ may be readily borne in mind, especially in parts where their division is necessary to give exit to pus or extravasated urine.

We are pleased also to mark the retention, by Mr. Gray, of the old Latin and Greek names of the different parts. It is absolutely necessary to have a technology, here as elsewhere; and there is a certain anatomical aroma, so to speak, an *unmistakeability* which belongs to *biceps*, *sartorius*, *saphena*, *falx major*, &c., and to no English substitutes for them. Moreover, their use saves time and space, in many instances, while it never can embarrass any one whose education qualifies him to have anything to do with them.

Mr. Gray has given a few pages on Fractures, as the surgical anatomy of the bones and muscles. These remarks are tolerably complete, but are illustrated by cuts taken from the well-known work of Mr. Hind, which has in our opinion been much too highly estimated; figs. 164, 166 and 167 (pp. 271 *et. seq.*,) are particularly poor. Nor can we think that the directions for treatment, appended to the description of each fracture, are full enough to be of much use.

The blood vessels and lymphatics are well described, while the brain, that mountain to medical students, is reduced to a mere mole-hill by the capital style in which it is explained and illustrated. Mr. Gray adheres to the arrangement of the cranial nerves into nine pairs, according to the plan of Willis, in preference to that of Soemmerring, who makes twelve.

Very good practical descriptions are given of the organs of special sense, of digestion, and of respiration. Fig. 336, representing the surgical anatomy of the neck in the infant, is especially valuable in connection with the clear and accurate remarks on the same page. Fig. 337, a transverse section of the thorax, although by no means novel, is an extremely useful illustration; we only wonder that it is ever omitted in any work on descriptive anatomy.

The urinary and genital organs are well treated of, but the cuts illustrating this part of the work are by no means equal to the rest; they are poorly executed, and some of them not altogether correct.

Upon the subject of hernia, our author gives us very clear anatomical descriptions, well illustrated, but not very copiously. Inguinal hernia would have been more exactly defined, by a distinct recognition of Poupart's ligament as the boundary between the inguinal and femoral regions; an idea which is of course implied, but which is not expressly stated.

There is a difficulty, which experience and observation have shown us to be a serious one, in giving students a clear idea of the precise position, &c., of the inguinal canal. Mr. Gray says:

"The *Inguinal* or *Spermatic Canal* contains the spermatic cord in the male, and the round ligament in the female. It is an oblique canal, about an inch and a half in length, directed downwards and inwards, and placed parallel with, and a little above, Poupart's ligament. It communicates above, with the cavity of the abdomen, by means of the internal abdominal ring, which is the point where the cord enters the spermatic canal; and terminates below, at the external ring. It is bounded in front, in its whole length, by the aponeurosis of the external oblique, and by the internal oblique for its outer third; behind, by the transversalis fascia, the conjoined tendon of the internal oblique and transversalis, and the triangular ligament; above, by the arched fibres of the internal oblique and transversalis; below, by the union of the fascia transversalis with Poupart's ligament. That form of

protusion in which the intestine follows the course of the spermatic cord along the spermatic canal, is called *oblique inguinal hernia.* * * * * * * * * The *Internal Abdominal Ring* * * * * transmits the spermatic cord in the male, and the round ligament in the female; and from its circumference, a thin, funnel-shaped membrane, the infundibuliform or transversalis fascia, is continued round the cord and testis, inclosing them in a distinct pouch. When the sac of an oblique inguinal hernia passes through the internal ring, the transversalis fascia forms one of its coverings."

Now we have for some years past been in the habit of teaching that *the infundibuliform fascia is nothing else than the inguinal or spermatic canal;* and we are satisfied that the idea thus given of that tube is clearer, more correct, and more readily retained, than any other can be. Students can remember the length, course and relations of a tangible form, better than those of a mere interspace.

In speaking of the obturator artery, in connection with femoral hernia, our author says that it "occasionally curves along the free margin of Gimbernat's ligament in its passage to the obturator foramen; it would, consequently, skirt along the greater part of the circumference of the crural canal, and could hardly fail in being wounded in the operation." Such a danger is alluded to by all writers upon this subject; Mr. Guthrie is quoted by Erichsen as saying that he has known some of the best surgeons in London to lose patients by hæmorrhage after the operation for femoral hernia. Mr. Fergusson, in his "Practical Surgery," alludes to this risk as having been very much exaggerated, and cites a case in Dupuytren's practice, in which the accident occurred without serious results. Malgaigne, Sedillot, and Guerrin agree with Mr. Fergusson, advising moreover the *debridement multiple*, or division of the constricting band at several points, in which the merest nicking of its edge at each point is sufficient. This plan is also recommended by Nelaton, who seems not without apprehension in regard to injuring the vessel. Now if such an injury should be inflicted, and hæm-

orrhage should occur to an alarming extent, what course ought the surgeon to pursue? We would recommend making an incision as if for ligature of the external iliac, but as near the median line as possible; endeavoring to secure the bleeding vessel, and failing in this, tying the iliac itself.

The surgical anatomy of the perineum is very fully explained and illustrated. We are not quite satisfied, however, with the meagre account given of the relations of the deep and superficial fasciæ of this part—a matter of which Mr. Gray, like many other good anatomists, may undervalue the difficulties.

The illustrations of Mr. Gray's book are, as a general thing, admirable; some of them are especially so, as for instance, fig. 330, showing the fœtal circulation, all those of the lymphatics, and most of those of the muscles. All of them are well-planned; and the student will find great advantage in the outlines annexed to the directions for dissection. But we think that some of the best of these designs lose very much of their value by the inartistic style of their execution; as for instance, fig. 187, showing the heart, aorta and pulmonary artery in position; fig. 204, showing the abdominal aorta and its branches; fig. 209, the arteries of the pelvis; fig. 261, the course and distribution of the eighth pair of nerves. All these cuts are, in our opinion, deficient in that *effect* which is especially necessary in illustrating complicated parts to those not far advanced in anatomy.

In the foregoing remarks upon the very excellent book which Mr. Gray has submitted to the profession, we have criticised freely, because such a work can well abide criticism. As a practical aid to the student and surgeon, it will be found to have great value; and its author has evidently aimed at answering this end. He has well earned what he will doubtless receive—the thanks and the encouragement of his fellow-laborers in medical science. Such books are not merely published, they are used; they are incorporated with the literary *armamentarium* of the profession. And in this tacit acknowledgement of merit, employment for practi-

cal purposes by practical men, consists the most reliable of all endorsements.

For re-producing the volume,* in so handsome a style, the American publishers are entitled to the thanks of their medical countrymen.

BIBLIOGRAPHICAL NOTICES.

ART. VIII.—*On Poisons in Relation to Medical Jurisprudence and Medicine.* By ALFRED SWAINE TAYLOR, M.D., F.R.S., &c. Second American from the second and revised London edition. Philadelphia: Blanchard & Lea. 1859. 8vo. pp. 755.

The subject of toxicology is one that at present attracts to a great extent, not only the attention of medical men, but of all those who by education, interest, or profession, are brought into relation with the progress of science or crime. The high degree of perfection attained in the chemical manufacture of substances of terrific poisoning power, the low prices at which they are sold, the ease and certainty with which they may be used to accomplish death, and the consequent temptation thus held out to those of evil intent, are circumstances well calculated to awaken serious reflections in the minds of all those who have the good of society at heart.

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* Originally issued by Parker & Son, of London.

Far be it from us however, to pass censure upon chemists for giving to the world so many toxic agents. Let it always be remembered that if we owe the great majority of poisons to the advance of chemistry, we are also indebted to this science for most of our antidotes, and that the very substances used by man for his murderous purposes are, in the hands of the enlightened physician, among our most valuable remedies in the treatment of disease.

The science of toxicology has of late years become so extensive, and so thorough a knowledge of chemistry is required, in order to become an expert, that we doubt the expediency of physicians in active practice, devoting themselves in any great degree to its study. For with the constant demands upon their time to which they are subject, and the impossibility frequently of their obtaining the requisite apparatus and reagents for conducting toxicological researches, it is out of the question that they should attain to that complete knowledge of the subject, which would enable them, when called upon, to do credit to themselves or justice to the public. So far as a knowledge of the symptoms produced by poisons and of the antidotes are concerned, physicians of course should not be deficient, but it is far better in all casses of obscure or criminal poisoning, that the experienced analytical chemist, whose studies have led him to such investigations, should be selected to make the requisite examinations.

Of all the treatises on poisons which have appeared, we regard that of Dr. Taylor as one particularly well suited for the requirements of the medical profession, and deserving the careful study of every physician. The symptoms resulting from the administration of poisons, the morbid alterations caused by them in the tissues of the body, and the antidotes to their effects are so fully dwelt upon, as to leave little to be desired, and the tests for their presence are given with so much detail and clearness, that those who intend making a special study of toxicology will derive very great assistance from the information Dr. Taylor has collected. The work has however been so long before the profession, and is doubtless so familiar to our readers, that it would be superfluous

for us to notice it *in extenso.* In this, the second edition, the whole subject of toxicology has been brought up to the present time, and it may therefore be relied upon as a safe and ample guide for those who consult its pages.

It is nevertheless impossible to attain perfection, and we ought not therefore to be supprised at finding some evidences of the want of it in Dr. Taylor's work. For instance, we discover no mention of Wilson's treatment of chronic mercurial and lead poisoning, by the internal administration of the iodide of potassium—a method from which the most favorable results are obtained—and, when considering the antidotes to strychnia, no reference is made to camphor, which, in this country, has been used successfully several times, and which if not an absolute antidote, is certainly entitled to more confidence than any mentioned by Dr. Taylor.

Again, the subject of animal poisons is entirely omitted, although one of vast importance, especially in this country and in many of the colonies of Great Britain.

We might point out other omissions, and a few statements which are not altogether correct, but they are scarcely of sufficient importance to merit special attention.

We have already commended Dr. Taylor's treatise to the consideration of the physicians, but, there is another class of professional gentlemen, many of whom, we are sure, would be materially benefited by its perusal, and, would probably be more disposed thereafter, to treat those medical witnesses, who in the course of law are subjected to these examinations and cross examinations, with more courtesy and respect, than they generally receive under such circumstances. There are many members of the legal profession, distinguished at once for their learning, and honorable bearing, who, when a physician appears upon the witness-stand, act towards him with the delicacy and kindness, which should always govern gentlemen in their intercourse with each other, more especially under the conditions which exist in a court of justice; to these our remarks in nowise apply. But there is a class of lawyers who make up for the want of learning, by effrontery, and for good-breeding by insolence, who, when unfortunately

in charge of a case involving questions of medical science, deem it their office to brow-beat and insult men every way their superiors. To such we particularly recommend Dr. Taylor's volume, in the hope that with the increase in the amount of their erudition consequent upon its study, they may also experience some of those softening and refining influences, which always attend the acquirement of scientific information.

ART. IX.—*Woman: her Diseases and Remedies.* A series of Letters to his Class. By CHARLES D. MEIGS, M.D., Professor of Midwifery and Diseases of Women and Children in the Jefferson Medical College at Philadelphia, Member of the American Medical Association, &c., &c. Fourth edition. Revised and enlarged. Philadelphia: Blanchard & Lea. 8vo. pp. 706.

It is now nearly twelve years, since Dr. Meigs, yielding to the repeated solicitations of his classes, first published the well-known volume before us, whose contents are familiar to almost every practitioner in our country—for who has not enjoyed the pleasure of hearing its now venerable author discourse in his quiet, familiar, and unrestrained freedom of manner, in the crowded amphitheatre of the Jefferson, or read with a half-smile of comico-serious respect this quaint medical book, in whose pages comedy treads so closely on the heels of her half-sister tragedy, and where Esculapius appears sometimes with the cothurnus, often with a mask.

It was a bold, but has not been an unsuccessful stroke of Dr. Meigs to try the virtues of a pen not wholly blackened with the sable ink of scientific orthodoxy, and the popularity of the scribe is a fair reproof of the common scandal which has always loved to link the doctor and the undertaker together in owlish gravity of demeanour, and a stern solemnity of manners and dress. We do not always " bear about the

mockery of woe," and though the habit of our lives is of necessity moulded by the grave, sad duties of our daily work, we can sometimes laugh and "play the fool" with an abandon of mirth that even Comus would approve; nor is the laugh less hearty, because seldom indulged in, or the less to be approved of, because never out of season.

The style is in this instance eminently the man. The work is every line of it Dr. Meigs; the author's pleasant tones, and kindly manner, and earnest good feeling pervade the whole book from cover to cover. It is the professor talking as he always does, easily, frankly, impressively; never at a loss for words or ideas; overflowing with his subject, which is his idol, the god of his daily worship through a long, long life, and always mixing the useful with the pleasant in such honest proportions that the stomach is satisfied while the palate is tickled.

The volume as we have said is too well-known, has been too long before the public to need analysis, or recommendation. It has of course been improved by four successive revisions, but not very sensibly altered. As every body knows, it contains opinions and conclusions which are peculiarly the author's. Though not generally adopted, he has not therefore abandoned them, and right or wrong has stood by himself manfully.

The physiology is not always very recent, perhaps, partly from that reason, not always very correct; but the rules of the art here prescribed, the obstetrical opinions here expressed, the general directions and advice given and suggested, are, beyond any cavil, unexceptionably sagacious and prudent. They are founded on a large practice, have been tested by a long experience, and come from lips to whose teaching thousands have listened for many years, and never without profit.

The book might have been condensed, some of its matter is doubtless irrelevant—directly at least—to the subject in hand, and would bear elision without destroying any of its utility; but the letters would not have been the familiar talk which the author intended, the style would have been

less free, the expression formal and stereotyped, and the reader have been presented with a dry dull treatise instead of a work which amuses while it instructs, is pleasant as well as profitable. To use a professional simile, the pill would have been less palatable, and not the least bit more efficacious, because the sugar coating had been omitted as a superfluous elegance.

ART. X.—*Water, as a preservative of health, and a Remedy in Disease. A Treatise on Baths; including cold, sea, warm, hot, vapour, gas and mud baths; also on Hydropathy, and Pulmonary Inhalation; with a description of Bathing in ancient and modern times.* By JOHN BELL, M.D., Fellow of the College of Physicians of Philadelphia; formerly Professor of the Theory and Practice of Medicine in the Medical College of Ohio, &c., &c. Second edition. Philadelphia: Lindsay & Blakiston. 1859. 12mo. pp. 658.

The treatise before us is an enlarged and much improved edition of a work on Baths and Medical Springs, published by Dr. Bell in 1831. To those who are acquainted with the laborious habits, the learning and accuracy of its author, it is unnecessary to say, that within a brief compass it presents an excellent summary of every thing connected with the subject in all of its details. Until its appearance a comprehensive volume on this topic was strongly needed, and much of the information which had to be hunted down in scattered essays, pamphlets and journals, was here for the first time collected together in a compact and easily approachable form. The author has spared no trouble in acquiring information from every quarter, and makes special acknowledgements of the value of MM. Gaudet's, and Rapou's treatises on Sea Bathing, and Vapour Baths. While not attempting "to establish a dogmatic system of hydrotherapia," he has faith-

fully, and we think most successfully, endeavored to frame a dietetical and medical hydrology, from the vague hints, practical rules, and scattered facts which the periodical literature of the day abundantly furnishes.

The work, although made up of brief chapters, may be divided into four parts: the first treating of Baths historically, and being purely descriptive; the second hygienically; the third therapeutically, and the fourth medicinally, or as they affect the body when medicinal substances are suspended in the water or steam of cold and hot baths.

There is no doubt that the old Greeks and Romans were cleanlier than our *civilized* moderns—that high and low alike recognized the luxury, utility and necessity of frequent ablutions, whose daily practice undoubtedly first arose from the imperative exigencies of a hot climate, and in the latter days of the Roman empire public baths were as loudly clamored for by the populace, and as readily supplied by their tyrants, as the *panem et circenses* which were found to be such admirable soothers of popular turbulence.

Bathing was the most useful, and perhaps the least expensive, of all the many luxuries which these acquisitive nations imported from the East, where what was at first a religious observance, afterward degenerated into a sensual enjoyment; but we are not satisfied that the modern Greek, Italian, or Egyptian is still the perfumed exquisite his progenitor was. Our author ranks the English and American too low, we think, in the scale of personally cleanly nations. It may be true that among European and Asiatic races, the bath is esteemed a greater luxury, and more frequently used by the wealthier classes, but unless travellers grossly deceive us, the masses of the Russians, the Turks, and, perhaps, even the *nice* Gaul, still believe in the sanctity of dirt, and may yet lay claim to recognition as "the great unwashed."

We cannot discuss or analyze in detail a work like the one before us, without making very frequent and lengthy quotations—this is both unnecessary and unpermissible here, our object simply being to direct our reader's attention to the publication as one of large usefulness, and containing ample

directions for the employment of water in every form, with most judicious reflections upon its uses and effects.

Dr. Bell studiously avoids partizanship, and although he indulges in occasional panegyric of rather doubtful remedial applications, he nowhere falls into exaggerated praise of any special practice, is seldom misled by strongly reported cases of particular cures, and treats his subject throughout with the calm, impartial and earnest discrimination of a sound physiologist, and excellent practical physician.

We recommend the work, therefore, most heartily to the profession, and the public. To the former it would be superfluous to speak of the importance of the subject, and it is to be hoped, unneccessary to dwell upon the value of a thorough knowledge of it on his part, who is the chosen counsellor and guide in all matters relating to public and private hygiene and health.

Any intelligent layman, of ordinary education, would find no embarrassment in following its lucid and simple explanations, advice and precepts, and the subject is one which cannot be too strongly urged upon public notice, as underlying the general health, virtue and happiness of communities and individuals.

Liebig lays it down as an axiom, that of two countries with an equal amount of population, that may positively be declared the wealthiest and most highly civilized which consumes the greatest weight of soap. Whether political economists will so regard it or not, there is little doubt that in combination with water, both will materially elevate the standard of health and mayhap that of morals, by effecting a social if not ethical advance, in inducing that "cleanliness," which the apostle himself tells us, "is nigh akin to godliness."

ART. XI.—*The Clinical Study of the Heart Sounds in Health and Disease.* By AUSTIN FLINT, M.D., Buffalo, New York. Being one of the Essays to which a prize was awarded, at the Eleventh Annual Meeting of the American Medical Association, held in the city of Washington, May 4th, 1858.

In our Review of the Transactions of the above meeting of the American Medical Association, published in our last number, we were compelled for want of space to pass over this admirable clinical paper with a mere casual reference. It gives us pleasure to turn back to it now, at greater leisure, even although we can only indicate the conclusions at which that excellent clinicist, Dr. Flint, has arrived from a somewhat large and well collated experience.

In accumulating the data for the first division of this investigation, 25 persons, presumed to be free from cardiac disease, apparently healthy on examination, who had never suffered from acute Rheumatism or any grave affection of the chest, were selected. All were males, between the ages of 20 or 30, with few exceptions. The examinations were variously conducted, at different times, and always with the utmost care. The author divides the general elements of the heart sounds into two; that produced by valvular action he calls the *valvular element;* that produced by the impulsion of the heart's apex against the thoracic parietes, he terms the *element of impulsion.*

From a study of the heart sounds in health, Dr. Flint is of opinion, that the *first* sound of the heart is a *mixed* sound, when studied at its point of greatest intensity, over the apex of the organ; being composed of both the above mentioned elements.

As the study of this sound cannot be satisfactorily conducted by vivisections, it must mainly be pursued by clinical examinations, and the results obtained by the latter, it must be remembered, are absolutely true only so far as they are considered in reference to their end—as for example, wheth-

er the element of impulsion is produced by the friction of the heart against the thoracic parietes, or emanates from the muscular wall of the heart itself, the clinical relations will be unaltered, and it is therefore a question of little practical importance.

Skoda, among others, regards the first of these causes as the true one, but the London and Dublin Committees, and Drs. Pennock and Moore of this country favor the latter. The basis of their belief being the fact, that the first sound is heard with greater intensity when the heart is exposed, and the stethoscope placed directly over the organ. Even were it admitted that this sound is the same as that heard when the heart is covered by the thoracic walls, and *in situ*, the muffling effect of this muscular barrier should be taken into account.

The *first* sound is almost invariably accentuated at the apex. The element of impulsion predominates here constantly, and as a rule, over the body of the heart; the valvular element is more marked at the base, at the left border and the right border of the heart, and at all the points removed from the precordial region, where the first sound can be heard. This difference in diffusion, agrees with and supports the views already presented in regard to the cause of the two elements; and the conclusion is further strengthened by examinations in various positions, aiding and diminishing the force of impulsion, and by the variations presented in those morbid conditions (emphysema, pericarditis, pleurisy, &c. affecting the left side) in which the apex of the heart is removed from the thoracic walls.

The valvular element of the *first* sound is less intense than that of the second, not being heard at points to which the latter is transmitted. It also differs in *pitch* when studied over the inferior border of the heart near the ensiform cartilage, and at or near the left nipple. This may be due to the fact that in the latter situation the sound emanates from the mitral, and in the former from the tricuspid valves.

The *intensity* of the *first* sound not only varies widely in different persons, but in the same person within the limits of

health, as well as in disease. The variations chiefly affect the element of impulsion, the valvular element undergoing but little change, in this respect resembling the second sound of the heart.

The *interval* between the first and second sounds varies in different persons, according as the element of impulsion in the first sound is more or less marked. When this element is impaired the interval is lengthened. In those situations where the valvular element alone is heard, the interval is distinctly longer than when the two elements are combined, as over the apex of the heart.

The *second* sound has an unmixed valvular quality. It is best studied over the base of the organ, where its relative intensity is usually greatest, and its characters are best marked in the intercostal spaces between the second and third ribs, on either side of the sternum.

On comparison of the *second* sound on the two sides, at the points last named, a difference in pitch and otherwise is apparent. In connection with the anatomical relations of the aorta and pulmonary artery here, and clinical facts pertaining to disease, we may conclude that the character of the sound on the left side is due to the action of the valves of the pulmonary artery, and on the right side to the aortic valves.

The *second* sound presents the characters of that produced at the orifice of the pulmonary artery, most frequently over the inferior border of the heart, less often in the third intercostal space on the left side, and over the body of the heart. Elsewhere, wherever the *second* sound is heard, it presents the distinctive characters of the sound produced at the aortic orifice—the first intercostal space on the left side excepted.

The *second* sound is not subject to great variations, even as regards intensity, in the same person; maintains its character unaffected by causes which alter the movements of the heart within the limits of health, as exercise, &c., and remains unchanged under circumstances which affect the *first* sound considerably.

Though the relative intensity of the *second* sound is notably greater at the base than apex of the heart, the absolute

intensity is seldom less and often greater at the apex than over the base.

It follows from some of the above facts that the *second* sound produced at the aortic orifice has a greater intensity than that at the pulmonary orifice, and also, that the intensity of the valvular element of the *first* sound is due to the mitral, more than to the tricuspid valves.

We pass over the interesting portions of the essay, which treat of the nature of the sounds in disease, the characters of the murmurs, their different elements and the causes of these, their diagnostic value, relative importance, &c. These constitute the argument proper, by which the following conclusions are supported, and upon which they are based; but we cannot do more than direct attention to the clearness and ability which mark the author's style and method of treatment throughout.

The most important results of Dr. Flint's investigations concerning the heart sounds in *disease* are:

That the two elements which compose the *first* sound of the heart, viz: the valvular element, and the element of impulsion, may be affected conjointly but unequally, or separately.

Of the two sounds which unite to form the valvular element, viz: the mitral and tricuspid, the former may be diminished or extinguished, while the latter continues; this indicates disease of the mitral valves. An abnormal intensity of the tricuspid sound under these circumstances, denotes hypertrophy of the right ventricle.

To determine whether weakness or extinction of the mitral valvular sound be due to injury or destruction of the mitral valves, or diminished muscular action of the heart, this sound is to be compared with the element of impulsion. If the latter be well marked over the apex of the heart, while the valvular element, without the left nipple, be feeble or wanting, the mitral valves are more or less seriously impaired. This inference is not to be drawn if it be suspected that the mitral sound is inappreciable or slightly heard in

consequence of the intensity, roughness, or diffusion of a co-existing murmur.

If from any cause the *first* sound be so weakened as to extinguish the element of impulsion, but not the valvular element, this sound, in duration and quality, resembles the second sound. When this is observed over the apex, it does not follow that the heart is dilated. Weakness of the systolic movement is alone indicated, and the change occurs from hypertrophy, as well as dilatation, if the muscular power of the organ be impaired from any cause.

The *second* sound of the heart being constituted of a valvular element alone, is not subject to any important modifications except such as affect its intensity, and the degree of valvular quality. The aortic and pulmonary sounds, of which the second is composed, may be separately increased, diminished, or extinguished.

Abnormal weakness of the aortic sound denotes injury to the aortic valves, especially if an aortic murmur be present, provided this weakness be not due to mitral disease, or impaired muscular tone of the heart. The evidences of mitral disease we have already stated. Evidence of enfeebled action of the heart is obtained by observing the first sound, as well as by comparing the aortic and pulmonary sounds, the latter of which is feebler in health, than the first.

When the aortic sound retains its normal intensity in connection with a diastolic murmur, the latter is probably produced by the direct current of blood through the mitral orifice, and not by aortic regurgitation. When the normal intensity of this sound continues, in connection with a systolic murmur referable to the aortic orifice, it shows that although a lesion exists in this situation, it has not, as yet, led to serious change of the valve.

Exaggerated intensity of the aortic sound occurs in cases of hypertrophy, affecting the left ventricle, so long as the muscular power of the heart is not impaired; it is also exaggerated when the power of the heart is increased.

When the pulmonary sound is relatively more intense

than the aortic, it denotes that it is positively exaggerated, or the aortic sound abnormally weakened, or both. If the aortic sound is not abnormally weakened, greater relative intensity of the pulmonary sound is evidence of hypertrophy of the right ventricle. In connection with this exaggeration, the tricuspid portion of the valvular element of the first sound will be likely to be intensified. Whether a greater relative intensity of the pulmonary sound be due to a positive exaggeration, or to a diminished intensity of the aortic sound, or to both combined, it is generally observed in cases of mitral disease attended by either obstruction or exaggeration. This rule, however, is not without exceptions.

ART. XII.—*Urinary Deposits: Their Diagnosis, Pathology and Therapeutical Indications.* By GOLDING BIRD, M.D., F.R.S. Edited by Edmund Lloyd Birkett, M.D. A new American from the Fifth London Edition, with eighty Illustrations on Wood. Philadelphia: Blanchard & Lea, 1859. 8vo. pp. 382.

The late Dr. Golding Bird has been one of the most laborious and successful, as he was one of the earliest workers in the interesting department to which the book before us is so excellent a contribution. Commencing his investigations at a time, when organic chemistry was comparatively in its infancy, he patiently pursued them through all the difficulties incident to this condition, with a zeal, caution and energy which has forever linked his name with the object of his pursuits, as one of the most accurate and erudite scholars, as well as original students among the large number of his countrymen who have so ably illustrated the subject of urinary pathology.

Since the first appearance of this book, organic analysis has undergone many improvements, the facilities and accuracy of microscopical research been largely increased, and the sub-

ject considerably modified and extended by most able contributions to its literature from numerous quarters. But such has been the care and labor expended in its revision, that it has uniformly kept pace with these advancements, and this last edition is fully up to the day, thanks to the competent supervision of its well qualified editor, Dr. Birkett. The large and useful additions which he has admirably interwoven with the text, have much enhanced its value, without in the least impairing its original elegance or unity.

The Wood cuts are not very numerous but sufficiently well drawn to prevent mistakes. For the beginner especially something more is needed, and until he has become familiar with the different appearances presented by substances crystallizing in the presence of other bodies, he would be likely to be much confused and embarrassed without other assistance. This is matter of little consequence, since the atlases of Robin and Verdeil, and Otto Funke, can generally be procured, and it would have been impossible to introduce other and finer plates into the work, without raising its cost measurably. In every other respect it is worthy of all confidence and needs no recommendation to those who are likely to need or use such text-books.

ART. XIII.—*A manual of Elementary Chemistry, Theoretical and Practical.* By GEORGE FOWNES, F.R.S., Late Professor of Practical Chemistry in University College, London. From the Seventh Revised and Corrected London Edition. Edited by ROBERT BRIDGES, M.D., Professor of Chemistry in the Philadelphia College of Pharmacy. Philadelphia: Blanchard & Lea, 1859. 12 mo. 600.

Dr. Fownes' excellent work, has been universally recognized everywhere in his own and this country, as the best elementary treatise on chemistry in the English tongue, and is very generally adopted we believe as the standard text-

book in all our Colleges, both literary and scientific. The student who has thoroughly mastered its contents, will go into his laboratory with the means of experiment and practical research fully at his command, and prepared to study and comprehend any of the higher and more systematic works of English or European writers. Its contents are admirably arranged—founded as they are on the results of a long and successful career as a public teacher—the directions for preparing and using the various reagents and apparatuses, and the descriptions of the general working processes are clear and simple, and the text eminently lucid, direct and concise.

The author's object was to prepare "a convenient and useful class-book" for his pupils, and minuteness of detail, or lengthy description could not therefore be expected; indeed this would have defeated the very object in view, and most effectually impaired the qualities for which it is so highly prized—brevity, compactness and simplicity.

While the general aim however has been to render it as practical as possible, it will be found comprehensive enough to embrace the whole field of inorganic and organic chemistry. In this last department, many improvements have been introduced, both as regards additions and arrangement, rendered necessary by the progress of the science since the death of the author. This correction and revision has been thoroughly performed by the English Editors. Drs. Bence Jones and Hoffman, and the American Editor's supervision gives still further assurance of its accuracy and reliability. With such occasional emendations, as must from time to time be demanded, it will continue, we doubt not, to retain, for many years, the large favor which it has heretofore merited and enjoyed.

ABSTRACTS AND TRANSLATIONS

FROM FOREIGN AND DOMESTIC JOURNALS.

PATHOLOGY AND PRACTICE OF MEDICINE.

Typhoid Fever in Children. From the Second Edition of the Traité des Maladies des Enfants. By MM. F. RILLIETT and E. BARTHEZ. Translated for the Charleston Medical Journal and Review. By DAVID E. EWART, M.D., Newberry, S. C.

[Continued from Vol. XIV., No. 4. p. 540.]

ART. V.—COMPLICATIONS.

Typhoid fever sometimes passes through all its stages without any accident occurring to impede its progress. In other cases, different complications are superadded to the principal disease, changing its physiognomy, aggravating its prognosis, and carrying some patients to the grave when every thing gave the hope of a favorable termination. These complications may be divided into three principal groups:

1st. Those which are the result of the progress of lesions which properly belong to dothinenteritis.

2d. Those which depend upon the catarrho-inflammatory character of the disease or which are connected with its specificness.

3d. Those which, entirely foreign to the typhoid affection itself, can only be regarded as simple coincidences. We however do not pretend to deny but that this division is somewhat arbitrary, and that certain diseases which we place in the third category could, in some cases, be referred to the second.

A. *Peritonitis*, a consequence of perforation of the bowels or bladder, is the only complication belonging to the first class.

Intestinal perforation is a rare phenomenon; we have only

once observed it, (*.) M. Taupin met with it in but two of his cases. He cites two other cases obtained from MM. Barrier and Husson. In the case observed by us, the death was sudden and was caused by an attack of convulsions. The disease presented itself under a very grave form. In the cases which we have borrowed from M. Taupin, perforation took place in three of them, at a time when the patients were entering upon convalescence, after a very grave form of the disease, and in the other when the disease was light. M. Taupin noticed the following symptoms, (loc. cit., p. 246:) "The patients were suddenly seized with violent pains, at first limited to the right groin, but which soon spread over the whole abdomen; they gave vent to sharp cries, and their countenances became shriveled and covered with a cold sweat; in one case a peculiar odor was noticed *(odeur de souris)*. The diarrhœa was checked in one case; both had vomited abundantly a porraceous matter; the pulse was small, very frequent and irregular; the skin cold and clammy; the abdomen excessively painful on pressure; the respiration embarrassed. They both died in less than thirty-six hours."

We will place with the complications which are the result of lesions proper to typhoid fever, an example of perforation of the bladder; a probable result of ulceration. A circumscribed peritonitis, the anatomical characters of which we have elsewhere described, was the result of it. (See vol. 2, p. 5.)

This case occurred in a young girl of twelve years of age, affected with a very grave ataxo-adynamic typhoid fever, accompanied with abdominal pains, diarrhœa, and afterwards constipation, and the appearance of a tumour in the right side of the abdomen on the sixteenth day after the commencement of the disease. We did not see the patient until the twenty-second day; but we then discovered the existence of an ill-defined tumour in the right hypochondrium, which was tense and resistant; the abdomen was generally painful. The tumour disappeared by degrees; but delirium persisted, and the child died on the fifty-second day.

B. Complications of the second class may be, as we have said, either dependent upon the inflammatory character of the disease, as the phlegmasiæ, or upon its specificness, or the alteration of the blood, as hemorrhages and gangrenes.

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*One of us met with a second case of the kind at Geneva, in a child of twelve years of age.

1. Among the former, we will first notice bronchitis, pneumonia, enteritis and otitis, other inflammations being very rare and regarded as simply coincidences.

Bronchitis and broncho-pneumonia.—Bronchitis forms an essential part of typhoid fever, and constitutes a complication only when by its intensity it dominates over the real disease. In this case one will observe the different symptoms which we have elsewhere described, and the forms known under the names of general bronchitis and acute and sub-acute broncho-pneumonia, (see vol. 1.) This complication seldom ever takes place except in those cases in which the disease is already, of itself, grave or very grave. It generally takes place at an advanced stage of the disease, from the twentieth to the fortieth day, and when it occurs, increases the fever, and brings about a great alteration in the physiognomy of the patient. Broncho-pneumonia may be observed at all ages, but it is most frequent in the youngest. It is very dangerous and there are few patients who recover from its attack when to the bronchial irritation is added somewhat extensive congestive or inflammatory alterations of the parenchyma of the lung itself, and especially when the complication is developed in subjects who are much debilitated.

Lobar pneumonia.—The existence of lobar pneumonia in typhoid fever has been denied; yet we have had undoubted examples of it. It usually takes place also at an advance stage of the disease and in grave or very grave cases; though we have met with it once in hospital practice on the eighth day, and once in private practice on the tenth. It is certainly less unfavorable than general broncho-pneumonia.*

Gastro-entero-colitis and softening of the mucous membrane, gastro-intestinal catarrh.—Lesions of the stomach are rare in children, and do not generally differ from those attendant upon many other diseases of a very different nature. Of all the lesions observed, the most frequent is softening with thinning or even destruction of the mucous membrane of the great cul-de-sac of the stomach. We have also noticed at other parts of the organ a simple punctated redness; once only, as we have elsewhere stated, have we noticed ulcerations.

Enteritis, which in the adult is altogether a secondary complication, appears to play a more important part during

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*At the hospital, in five cases of lobar-pneumonia, we have seen three recover and two die, whilst in seventeen children affected with broncho-pneumonia, only one recovered.

childhood; for, whilst in the adult, inflammation of the intestines is only met with at an advanced stage of typhoid fever, in children it may be discovered when death has taken place early in the disease. We have discovered its anatomical characters on the seventh and ninth day.

Lesions of the large intestines are still more frequent than those of the small intestines; they are more general and more intense in proportion to the duration of the disease and the youth of the subject.

We noticed in surveying all the different lesions of the gastro-intestinal mucous membrane observed by us, that this membrane was perfectly healthy in only a fifth of those children who died, and by separating the lesions of the stomach from those of the intestines, that the latter were inflamed or softened in seventeen out of twenty-seven patients.

It is very difficult to recognize these complications during life. We have not observed vomiting to be more frequent in those who have had lesions of the stomach than in those who presented none. Enteritis is still more difficult to distinguish, since the principal symptom, (diarrhœa, enlargement of the abdomen, and pain,) are the same in the two diseases. The existence of an entero-colitis may be suspected however in the youngest children when the diarrhœa increases in intensity and persists for a long time, and, in the oldest, when severe abdominal pains exist.

This complication may affect children at any age; but it is certainly more frequent in the youngest, since in eight patients from two to four years of age who came under our observation, six had an entero-colitis or a considerable softening of the intestines. We think the treatment evidently appears to contribute to its production: this we have proved in another part of this work, (see hereafter *Treatment of Typhoid Fever*, and also vol. 1., page 745.) This lesion certainly renders the prognosis more favorable. In some of our youngest patients it exercised an unfavorable influence, and it is probably to it must be attributed the death of those children in whom the typhoid fever had every appearance of terminating favorably.

Otitis.—Inflammation of the auricular canal is a frequent complication: sometimes it is dry and consists only in very severe pains; sometimes, on the contrary, it is accompanied by an abundant fetid discharge. Otitis generally comes on at an advanced stage of the disease; we have never observed it before the nineteenth day, except in one case in which it was dry, and which appeared on the tenth day. The left ear generally commences to discharge first; sometimes it occurs,

in both ears, but successively. Its duration varies from ten to fifteen or twenty days.

It was principally in the years 1837, '38, and '39, that we observed this complication at the child's hospital; we did not meet with a single case from the first of April to the 30th of September, 1842, either in our private or public practice, at Paris and Geneva. Yet we met with some cases of incomplete and transient deafness.

Otitis is almost equally developed in light or grave cases; we have had only one subject to die from it. As it is met with, it is of rather favorable augury, and does not lead to any dangerous consequence. M. Taupin has already come to the same conclusion. At the hospital, otitis was noticed ten times in 107 children.

Parotids.—Disease of these glands though not a very rare complication in the adult, is much more so in children. The only time we have noticed this complication, it occurred on the seventh day of the disease in a boy of nine years of age, who died on the twentieth.

11. The different complications which we have just mentioned may be said to belong to the catarrhal or inflammatory character of the disease; hemorrhages and gangrenes pertain to its specificness.

Gangrenes are very rare in children as a complication of typhoid fever, though they frequently occur during eruptive fevers. We have never seen gangrene of the mouth as a complication of dothinenteritis. M. Legendre told us that he had met with one example of it; M. Tourdes has noticed that out of ninety-eight cases of gangrene of the mouth, seven succeeded typhoid fever. M. Boudet has published in the *Bulletins de la Societe Anatomique* (1840, p. 388,) a note on a case of gangrene of the pharynx which he observed on the seventeenth day of a typhoid fever occuring in a girl of six years of age, who died on the twenty-third. M. Taupin cites a case of gangrene of the lungs occurring during the declining stage of the disease. Dyspnœa, general debility, and fetid breath, were the symptoms which led to the recognition of this complication during life (loc. cit., page 246.) Constant's case is of the same kind (see page 383.) Gangrene of the skin is certainly rarer in children than in the adult. Our result in this respect entirely accords with M. Taupin's; we have noticed sloughs on the sacrum only six times out of 107 cases; they occurred between the seventeenth and fiftieth day in cases that terminated fatally. In one case there was a deep, perpendicularly cut ulceration having a yellow bottom. M. Taupin and M. Stœber have seen children die from the excessive suppuration succeeding the casting off of the slough.

Doctor Dufresne has communicated to us some very interesting notes on three cases of gangrene observed by him during an epidemic which occurred in an institute for young girls near Geneva. Out of twenty-two girls in whom the typhoid affection was more or less grave, three were affected with gangrene, a much greater proportion than we noticed in our private or hospital practice either at Paris or Geneva. One of these children, aged three years, was attacked with a gangrene of the mouth which brought on death at the expiration of six days. At the same time the gangrene took place, numerous petechiæ appeared over the surface of the body. There was also in the last stages of this case, epistaxis and bloody stools. Another of these girls, aged three years, had an extensive gangrene of the vulva (the labia majora were destroyed,) accompanied with petechiæ and bloody stools. The disease lasted forty days; the child recovered. Again, a girl of eight years, about the eighteenth day of the fever, presented two gangrenous spots, one on the right wrist, the other on the anterior part of the thigh of the same side, the sloughing of which lasted ten days; the case thoroughly recovered. There was no hemorrhage.

Hemorrhages.—Our observation (former and recent) joined to that of M. Taupin, shows that hemorrhages, with the exception of epistaxis, are rare.

We do not possess a single example of intestinal hemorrhage. M. Taupin has met with but one case of it, which occurred in a boy of fourteen years of age, affected with a very grave typhoid fever; death was the consequence of it, but, as is observed in the adult, this did not take place until several days after the blood had ceased to flow.

The epidemic influence may modify this very general rule, as is proved by the cases communicated to us by Doctor Dufresne. In fact, besides the two cases of hemorrhage coincident with gangrene which we have just cited, this physiscian has seen two other cases in which there was hemorrhage from the bowels. They both recovered.

Sometimes an autopsy reveals a hemorrhagic condition even when there was nothing during life to lead one to suspect it. Two of our patients who died at the hospital thus offered traces of lobar and general lobular apoplexy of the lungs. In one case, the lung presented no other lesion; in the other a hemorrhagic cavity was surrounded by a pneumonia; but this pulmonary lesion was developed during an attack of varioloid which succeeded one of typhoid fever, (see p. 289.)

C. Among the purely accidental complications we will class certain phlegmasiæ, anasarca, eruptive fevers, and tubercles.

1st. *Inflammatory affections.—Pleurisy.*—This is a rare complication; we have observed it only three times. (In making this statement however, we leave out of consideration certain cases of slight pleurisy, which, being coincident with pneumonia, were evidently dependant upon the latter affection.) In a case of typhoid fever occurring in a boy of fifteen years of age, who died on the twentieth day of the disease, a pleuritic inflammation so masked the real affection as to entirely absorb our attention. Upon the disappearance of the effusion, the patient left his bed and was walking about the hospital ward, when suddenly he was seized with quite an intense febrile movement, accompanied with an eruption of numerous sudamina, and in twenty-four hours afterwards he died. In another case, a pleurisy took place on the thirty-seventh day of the disease, and lasted twenty days; the patient recovered. In a third case, it occurred on the thirty-third day of the fever, afterwards became complicated with pneumonia, and finally the patient died of a rougeola on the fifty-second day.

Angina.—Laryngitis.—We saw four children affected with pseudo-membranous angina during the prevalence of an epidemic of diptheritis at the hospital in the year 1840. In three cases the disease was slight, the false membranes being in small patches, situated upon the uvula and parts of the pharynx. In the other case, the false-membranes were more abundant, at least so far as we could judge during life, for we made no post mortem examination of this case. One of the four patients was besides affected with a very severe coryza; and the one we made no autoptical examination of, had a laryngitis. In all four cases the fever was a grave or very grave form.

The angina appeared at an advanced stage of the disease, from the fifteenth to the twentieth day. Three of the patients died and one recovered. In the case in which the angina was attended with rather abundant pseudo-membranous formations complicated with laryngitis, it very probably had the effect of producing the fatal termination; but in the others it did not seem to have much influence in aggravating the chances of recovery. One child, in fact, died of intestinal perforation; and in the other, a coryza was in great part the cause of death.

A fifth patient was afflicted with a pretty intense erythematous angina; he at the same time had a pseudo-membranous laryngitis. In the two children who had laryngitis, the complication was developed nearly at the same time with the angina.

To these two cases we might add that of a boy of eight and a half years of age, who, on the thirty-second day of a typhoid fever, was seized with complete aphonia, which lasted ten days, accompanied with pains in the larynx. In this subject we did not observe any lesion in the posterior part of the throat, but four days after the disappearance of the aphonia, a pseudo-membranous patch was formed on the posterior part of the inferior lip. We are now convinced that this case was one of secondary pseudo-membranous laryngitis.

Finally, in a young girl of fourteen years of age who died on the twentieth day of the fever, we noticed that the trachea was red in its whole extent, having lost its smoothness and consistence at many points. A great number of yellow grains strongly adherent to the mucous membrane were observed lodged in small excavations which occupied its entire thickness. This lesion gave rise to no symptom.

Nephritis.—We have in three cases discovered after death the anatomical characters of albuminous nephritis, which during life did not manifest itself by dropsy; the urine was not examined.

Softening of the Spinal Cord.—We have only once discovered this lesion; it occurred in a child who succumbed to a chorea, which supervened during convalescence from typhoid fever.

M. Taupin has seen, in grave cases, *furuncle*, *anthrax* and *phlegmonous abcess*, occur as complications of dothinenteritis; he has observed that these inflammations were dangerous when they took place at the time the disease was in its progressive stage. Only two of our patients had abcesses; in one they were situated on the scalp, and in the other back of the ear; the former died, and the latter recovered.

2nd. *Anasarca* is not a very rare complication; we have observed it in nine cases. M. Stoeber has published the case of a boy of eight years of age, in whom first an œdema of the genitals, and afterwards ascites supervened.

Whether general or partial, serous infiltration into the areolar tissue is an accident which ordinarily occurs during convalescence, and should be considered as the result of the then existing anemic condition of the patient. In cases of this kind, it is observed to occur at an advanced stage of the fever, from the twentieth to the fifty-sixth day. We have, however, twice seen it take place soon after the commencement of the disease (the fifth day); in both these cases, the infiltration, which was abundant, was general, and lasted eight or ten days; the urine was not coagulable. After the

effusion had disappeared, the symptoms of typhoid fever persisted, and the disease followed its usual course; both cases recovered. The same was the case with all those in whom anasarca supervened during convalescence. In a case of ours, mentioned by M. Taupin, we noticed a considerable œdema of the left leg to occur coincidently with a diminished pulse, and pretty acute pains in the limb. Was there an inflammation of the artery in this case?

3rd. *Eruptive fevers.*—We have observed in children three examples of grave anomalous variola, to supervene during convalescence from typhoid fever. We have but one case of rougeola, which occurred at an advanced stage of a typhoid affection complicated with pleuro-pneumonia. M. Taupin has noticed rougeola four times, and scarlatina ten times; these complications nearly always occurred during convalescence. Those cases of rougeola, which were complicated with pneumonia, died; this was not the case with those afflicted with scarlatina, the eruption of which was always discrete (loc. cit. p. 245.)

4th. *Tubercles.*—According to M. Taupin, tuberculization sometimes follows typhoid fever. We have never observed at the hospital any case of this kind. Besides, seeing on the one hand, that this disease never attacks children seriously affected with tuberculization, and on the other, that when phthisis progresses rapidly, it sometimes so resembles typhoid fever as to be mistaken for it, we are forcibly led to conclude that the cases reported by M. Taupin were cases of tuberculization resembling typhoid fever in their outset. If this affection exercises any influence upon tuberculization, we would be inclined to think that it acted in the same manner as variola and scarlatina; that is, it would cause the tubercles to pass into a cretaceous state. In twenty-eight children who died of typhoid fever, we found tubercles in eleven. These accidental products were always in a very small proportion; some miliary tubercles were found in the lungs, and a few tubercles in the mesenteric glands. We once saw the mesenteric glands, etc., converted into a large mass of cretaceous matter. The case in which this occurred had died of a hemorrhagic variola that supervened during convalescence from typhoid fever. In the eleven children of whom we just spoke, four had cretaceous tubercles.

New facts which we have collected, confirm these results. In all the children treated by us in private practice, only two were afflicted with acute tuberculization during the course of dothinenteritis, and these occurred in Paris. More

than once, however, in seeing the fever prolonged, emaciation increase, the features become sunken, and frequent and fatiguing fits of coughing occur, at the same time that auscultation revealed to us all the signs of that general subacute bronchitis, which, in children, is so often the precursor or accompaniment of tuberculization, we have apprehended a pulmonary or general tuberculization. Our unfavorable presentiments, however, were nearly always contradicted by the progress of the disease, the gradual disappearance of the symptoms, and the complete re-establishment of the health. We could cite many cases of this character, but perhaps one will suffice.

One of us (M. Rilliet) was called in consultation to see a boy, aged five and a half years, afflicted with a typhoid fever which, on the tenth day, became complicated, first with a right lobar pneumonia, then with a double suffocating capillary bronchitis, and finally, on the twenty-second day, with a pleuritic effusion which gradually increased until the right side was filled. By degrees the child was reduced to an extreme degree of marasmus, and all the local and general symptoms appeared to combine to persuade us that a tuberculization had succeeded the typhoid fever. Resting upon our conviction, however, of the incompatibility of the two diseases, we repeatedly told our confrere "not to be uneasy, the child would not become tuberculous." But one day, our faith, strong as it was, came near being weakened. The child had now had, for six weeks, a pleuritic effusion which was constantly increasing, and all the symptoms of hectic were present, when suddenly the lung was perforated, and exit given to the pus, which was discharged in efforts to cough and vomit. This pneumo-thorax supervening without a cause in the midst of a general condition so alarming, inspired us with serious apprehensions, yet we did not lose all hope, and we were right in so doing, for from this time, the symptoms gradually diminished, and in two and a half months after the commencement of the pleurisy, the child was entirely cured. Seven years have now passed, yet no symptoms of tuberculization have manifested themselves.

Communications regarding some cases of Laryngeal Disease, examined by means of the Laryngeal Speculum. By DR. LUDWIG TUERCK. (Zeitschrift der Gesellssch. der Aerzte zu Wien, 1859, No. 11.)—*Brit. & For. Review*, for July.

We have on former occasions directed attention to the practical utility of the laryngeal speculum in the diagnosis and treatment of diseases of the larynx. The following are a few brief memoranda illustrative of the uses of the instrument, taken from the details given by Dr. Turk, in the above memoir:

1. A girl, aged thirteen, after recovery from lupus of the upper lip and left cheek, was attacked with temporary aphonia and renewal of the labial ulceration. The free edge of the epiglottis was seen to be much thickened, its mucous membrane much tumefied, and of a pink colour; at the anterior edge there was a loss of substance; the mucous covering of the arytenoid cartilage was much swollen, and the true chordæ vocales showed a white investment at their free edges.

2. A man, aged thirty-three, suffering from hoarseness and occasional aphonia. The chordæ vocales remained separated at their middle part, and did not show the tremulous movement seen in the healthy subject, when he pronounced A (ah); in coughing, the artenoid cartilages approach one another, and closed the false chordæ vocales, a condition which the author has often observed in aphonia dependent upon laryngeal catarrh. The autopsy revealed diptheritic (?) ulcers on the posterior surface of the trachea, on the parts of the larynx subjacent to the glottis, and between the arytenoid cartilages.

3. A pedlar, suffering from syphilitic aphonia, recovered from all his affections but the aphonia. The speculum showed no loss of substance in the epiglottis; the region of the left arytenoid cartilage was somewhat flattened. The true chordæ vocales did not join properly, especially at the posterior angle. when A was pronounced, nor when the patient coughed. On deep inspiration the cords did not separate properly; the true and false chordæ could only be distinguished on the right side.

4. A female, aged forty-seven, had been affected with complete aphonia for eight months. Frequent examination of the larynx never exhibited any catarrhal affection; in whispering A the chordæ vocales closed only at the anterior and posterior angle, and were wide apart in 1

their extent, and did not exhibit the normal vibrations; in making a more powerful effort at pronouncing the A, the space between the chordæ increased to from one to one and a half lines. In doing this the corpuscula Santorini at the apices of the arytenoid cartilages crossed one another, so that the right one came to lie in front of and below the other.

5. An artificial flowermaker, aged thirty, had suffered for many years from aphonia. At the anterior edge of the glottis the two chordæ vocales were of normal appearance; behind them there were on both sides a pale red excrescence, nearly two lines long; a narrow third excrescence was occasionally projected between them by coughing; another growth of a similar kind was attached to the left cord as well as to the right, and the two last were considerably separated when the attempt was made to pronounce A.

6. A case of œdema glottidis in a young man, following typhus, giving rise to great dyspnœa, exhibited a perfectly smooth surface in the place of the upper and lower vocal chords, and at the inner sides of the arytenoid cartilages were two transparent bladders, which only left a linear slit between them.

7. A female, aged thirty-eight, suffering for three months from hoarseness and dyspnœa, exhibited a translucent tumefaction almost covering the right cord as well as the ventricle; the inner surface of the right ary-epiglottic fold was also prominent; the same parts on the left side were normal.

On Cerebral Deficiency and Hydrocephalus. By Professor R. Heschl, in Cracow. (Prager Vierteljahrsschrift, xvi. Jahrg. 1859., Erster Band, p. 69.)

Professor Heschl has observed four cases of deficiency of the central gyri of the brain and of the adjoining parts. The first case occurred in an idiotic beggar, who died at the age of twenty-six, who had only had an imperfect power of executing the ordinary movements, and whose range of speech had been very limited. His left side was more powerless than the right. The right parietal bone bulged out, the most prominent parts being reduced to form one-half a line to one line in thickness, while the remainder of the cranial vault was from two to two and a half lines thick. The left cerebral hemisphere and its membranes were essentially normal, nor was any malformation perceptible in the cere-

bellum, medulla oblongata, and spinal cord. In the right cerebral hemisphere the whole middle portion of the centrum ovale, together with the convolutions appertaining thereto, both on the surface and at the Sylvian fisure, were absent, so that on removing the dura mater, one at once looked into the patulous lateral ventricle. The space was occupied by a large bladder with very thin parietes, containing about a pint of clear serum. This bladder evidently consisted of the distended internal cerebral meninges of the defective portion; it was in close proximity externally with the dura mater, and was intimately united to the arachnoid and pia mater of the remainder of the hemisphere. The right corpus striatum and thalamus opticus was smaller than those of the left side; the septum was perforated at two points. The tissue of the brain was otherwise healthy, and nowhere showed any extravasation or traces of inflammation.

It is unnecessary to give the further details of this case, or to specify those of the remaining cases, which in their essentials are identical with the first. The author sums up the conclusions to which he is led by the analysis of these cases as follows:

1. Idiots affected with hemiphlegia exhibit a congenital defect in the brain, which gives rise to a communication between the lateral ventricles and the sac of the archnoid. He terms this defect porencephalia.

2. This porencephalia is always associated with other defects in the formation of the hemispheres.

3. Porencephalia is probably not an arrest of development, but a true disease occurring during the growth of the brain.

4. It is sometimes associated with hydrocephalus, but as little as other cerebral defects explicable as the effect of intra uterine hydrocephalus.

5. Intra uterine hydrocephalus is probably the *consequence* and not the *cause* of numerous malformations of the brain.—*British and Foreign Review*, for July.

Raw Meat in Diarrhœa.—Our readers have, doubtless, not forgotten the interesting history of the two little twin daughters of a wealthy Mulhouse merchant, who had been reduced by unconquerable diarrhœa to the last gasp of life, and who, fed with the pulp of raw meat, returned, in a few months, to a state of perfect and robust health. Many facts have, since then, confirmed our confidence in [illegible] this Russian mode of treatme[illegible]

tunity to escape of recommending it, and of pointing out the best manner of rendering it both useful and acceptable.

The meat best adapted to the purpose is the fillet of beef; some patients, however, prefer the centre part of mutton chops. It should be cut fine, pounded in a mortar, and strained through a seive or cullender. The pulp, thus separated from the cellular texture of the muscular substance, is then gathered with a knife, and rolled in salt or powdered sugar, or mixed with currant-jam.

One of Mr. Trousseau's grand-children would take it only when mixed with racahout, a farinaceous compound of cocoa, ground rice, and potato-flour, sweetened and flavored with vanilla. Mr. Trousseau causes it sometimes to be rolled into small salted balls, of the size of a hazel-nut, or in little oblong gobbets, which may be administered in soup, to the number of thirty or forty, equivalent to four or five ounces of meat pulp. In grown persons, and particularly with ladies, the physician will probably meet with a repugnance, which he must overcome by concealing the repugnant character of the medication. For this purpose, some appearance of cooking may be imparted to the food, by exposing a thick slice of the meat, for twenty minutes, to the action of a brisk fire; its surface is thus roasted, the interior parts remaining raw, and being then treated as we have said. Mr. Trousseau has thus caused to be prepared by Mr. Mialhe (one of the principal apothecaries of Paris,) meat pulp combined with confection of roses, destined for delicate stomachs, which is taken without disgust, and even with pleasure, under the agreeable denomination of Damascene Preserve.

In children, the dose of raw meat, the first day, should not exceed 2½ dr. in four meals. It may be doubled on the second day, and on the third attain eight drachms; and so on, without any other additional food than albuminous water. It is easy to measure with precision the quantity administered daily, by means of a small balance and the current coins, the weight of which is well known—the franc being equivalent to one drachm, and the five-franc to six drachms. The dose may be carried as far as ten or twelve ounces, and the children gradually recover their good looks, their plumpness and spirits. At the end of a month or six weeks, when diarrhœa has entirely ceased, the quantity of raw meat can be gradually decreased, and broth or underdone eggs can be substituted, so as to reduce the dose of meat to three or four ounces daily.

It is necessary to be aware that, at first, when already the nature and abundance of the diarrhœa has undergone a favor-

able change, the motions are red and fetid. In one of the little Mulhouse patients we above referred to, this animal diet appeared to have occasioned the development of tape-worm, a parasite commonly met with in Abyssinia, where the natives feed on raw meat; but this kind of nutriment not being so long persevered in, generally, as was the case in the instance of the little girl alluded to, this circumstance must be considered exceptional, and cannot counterbalance the decided benefits yielded by the Russian mode of treatment, in cases of chronic disturbance of the bowels, and especially in the unconquerable diarrhœa which children are subject to in their second year.—*Dub. Hos. Gaz.*, April 15, 1859, from *Jour. Pract. Med. & Surg.*, Paris.

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Retrospect on the Use of Raw Meat in the Diarrhœa in Weaned Children. By J. F. Weisse, Director in the Children's Hospital at St. Petersburgh.

Seventeen years have now elapsed since I first directed the attention of the profession to this invaluable remedy in the above disease; but it was not until I had five years later treated of the subject at greater length, that it came into more general use. Soon after the publication of the latter paper, I received from the esteemed editor of the Journal just now quoted, Dr. Behrend, of Berlin, a letter containing the following passage: "You have no idea what interest your communication on diarrhœa ablactatorum and on the use of raw meat has excited; we now use the remedy extensively."

Not long after, Dr. Behrend inserted in the sixth volume of his Journal a letter of M. Marotte, Physician of the Central Bureau of the Parisian Hospitals, from which it appeared that this subject had attracted great attention also in the French metropolis. The author of this letter, which is addressed to Dr. Trousseau, has moreover had the kindness to suggest a theory explanatory of the results I have obtained. From this time the meat cure was generally received, and its utility admitted on all sides. Of the numerous favorable reports recently published, I cannot forbear literally transcribing that contributed by Dr. Eichelberg, because the author has given to the subject the appreciation it deserves. He says: "In consequence of the shortness of the time which has elapsed since this article of diet was first recommended, I have, it is true, only a limited number of observations (somewhat more than twenty) before me, but they all corroborate

the remarkable advantages of the plan proposed. It is only in exceptional instances that such children refuse raw meat —the great majority, in fact, consume it with manifest relish. I have observed two very striking cases were the children for several weeks readily partook of this food with the most beneficial results, and at the end of that time, suddenly refused it. Natural instinct seems in such examples to be unmistakable, as in the case of sick dogs, which eat grass. The want of osmazome made the children greedily consume the raw meat, but, with the cessation of the want, the desire for that principle disappeared.

As Herr Eichelberg, moreover, has expressly indicated the diarrhœa which sets in soon after the weaning of children (according to my observations usually in two or three weeks after that event), as the affection in which the raw meat cure is attended with certain success, so I have also, in recommending this mode of treatment confined myself to the same disease; and now, after nearly twenty years' experience, maintain that raw, scraped beef, to the exclusion of all other medication, is the true specific in this destructive diarrhœa. I therefore consider a remark made by Charles Hogg, in recommending the well-known "beef-tea," of the English, to be quite erroneous. Thus he says: "Beef-tea is an excellent, nourishing and easily digestible article of food, and completely replaces the juice of meat recommended by Weisse, of St. Petersburgh, obtained by scraping raw flesh." I have in raw beef discovered, not an article of food for children, but a remedy against the diarrhœa in question; nor have I spoken of the juice to be obtained by scraping meat, but the muscular substance itself must be given to the children, having, however, previously been sufficiently comminuted, either by scraping with a knife, or by means of a grater, in order that it may be swallowed without trouble. But the principal point is, that the muscular substance itself, and not merely its juice, should be conveyed into the digestive tube. The English beef-tea has as little beneficial effect on diarrhœa ablactatorum as Liebig's excellent decoction of meat. Both these fluid aliments appear, precisely because they are fluid, to pass too quickly through the intestinal canal; while the meat in substance remains longer in the tube, and by its mechanical irritation may stimulate digestion, and it may, perhaps, also neutralize the acridity of the gastric juice. Nor can I participate in Dr. Beer's sanguine hope that raw grated beef may be destined one day to dislodge codliver oil from the Materia Medica. Each of these excellent remedies has its definite sphere of medical action in the diseases of

children; raw beef in the diarrhœa ablactatorum, cod-liver oil in rachitic affections, with and without atrophy.

In St. Petersburgh, the meat cure in the affection of children under consideration has become, so to speak, completely naturalized; and this has taken place rather through oral communication, and in consequence of the favourable results of the treatment, than from any paper or essay, as I have never published anything in that capital upon the subject. Most of my colleagues have now for several years made use of it, and they all assure me that they have obtained very satisfactory results, even in cases where the employment of other established remedies appeared to hold out no hope of cure. I have myself seen this treatment adopted in about two hundred children, and, in the majority, with the desired effect, provided recourse was had to it at the proper time. I say, at the proper time, for if the disease has already advanced too far, and, particularly, if it has assumed the form of the so-called gastro-malacia, it is only in exceptional instances that we shall obtain a cure. But even in this case there is no other remedy so calculated to allay the most tormenting symptoms, the tantalizing thirst, and the vomiting, as the raw beef. This beneficial effect is produced even after a few meals.

But it has recently been stated, as I have already publicly remarked, that in many children saved by the meat cure, tape-worm, and it is worthy of note, always the tænia solium, that is precisely the species which is not indigenous in St. Peterburgh, has shown itself. A Dr. Braun has felt himself called upon to question this statement; two years later, however, an undoubted authority on this subject appeared in favour of the facts reported by me. Professor D. Von Siebold, of Munich, says. in the last page of his interesting work, "Ueber die Band und Blasenwüemer," Leipsic, 1854: "We can no longer be surprised, or consider their statements fabulous, when physicians report that tape-worms have been found in certain patients after the use of raw meat prescribed as a remedy;" and in the note upon this passage he adds: Compare on this subject Weisse's communications, which, notwithstanding Braun's objections, are worthy of all credit." Herr Von Siebold directs particular attention to the fact that in every instance it was the tænia solium which was passed; and he considers it probable that this species of the tape-worm, which is not indigenous in St. Petersburgh, may have been conveyed thither in the undeveloped state in the flesh of oxen, brought from Tscherkask and Podolia.

Only a few weeks before my departure from St. Peters-

burgh, in June of the present year, a tape-worm, more than four feet long, was sent to me by a colleague, to whom I had warmly recommended the meat cure in the case of a child, aged eighteen months, who had suffered from the diarrhœa in question, and was already very much run down, which worm was passed after the use of the ethereal oil of mail fern. This remedy was administered in consequence of the child, who had long ceased to get the raw meat, and was cured of the diarrhœa, having repeatedly passed joints of tape-worm. The attendant physician had already correctly diagnosed the worm to be the tænia solium; I found that it was voided with the head, on which the suckers were plainly distinguishable under the microscope.

I should not omit to state, that in the Children's Hospital under my care, in the diarrhœas of older children, into which the element of dentition no longer enters, and which so largely contributes to fill the lists of mortality, raw meat has been repeatedly and successfully tried. These cases of diarrhœa generally depend upon ulcerations in the intestinal canal.

Lastly, I may be allowed to call the attention of the meeting to as palatable a remedy as raw beef, in the lientery of adults; I allude to oysters. In two cases, an amount of experience, which, I must admit, goes for nothing, I saw the patients cured by the moderate use of these mollusca. From eight to twelve oysters were taken daily in two meals.—*Dublin Quart. Journ.*, Feb., 1858, from *Journ. für Kinderkrankheiten*, Jan. and Feb., 1858.

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On the occurrence of a blowing sound in the Pulmonary Artery. By Dr. J. Da Costa, Lecturer on the Practice of Medicine in the Philadelphia Medical Association.

In examinations of the chest, Dr. Da Costa has met with a blowing sound in the second intercostal space on the left side, the occurrence of which has only been imperfectly attended to. The sound is heard in the exact course of the pulmonary artery, and especially at a point corresponding to the bifurcation of its left branch; and there is every reason to believe that it must be referred to this vessel. Dr. Latham, long ago, pointed out the occurrence of a blowing sound at the upper part of the left lung in persons who were undeniably tubercular, or suspected of being so, and Dr. Da Costa's object is

chiefly to recall attention to the forgotten observation, to explain its cause. Dr. Da Costa relates eight cases, in all of which local signs of partial consolidation were met with, which consolidation, he thinks, might prevent the vessel from fully equally distending, and thus give rise to a murmur; and this explanation has every appearance of probability, for in no one of the cases could the murmur be referred to anæmia, or to the presence of local obstruction in the vessel, nor could it be supposed to be transmitted from the heart.

"The physical sign in question," says Dr. Da Costa, in his description of it, "may be stated as being a murmer attending the impulse of the heart, almost always soft and low-pitched, although occasionally harsher, of higher pitch, and simulating a sibilant râle. Its situation is in the second intercostal space on the left side, not an inch from the edge of the sternum. It may be audible higher up, or opposite the third rib, on the sternum.

"All blowing sounds in the heart or in arteries are due either to a peculiar condition of the blood, or to the presence of local obstructions; or, again, they may be transmitted. This latter supposition cannot be entertained, as there was no murmur anywhere to be transmitted, the heart in all the eight cases yielded perfectly normal sounds.

"The space it occupies is usually very limited, and can be accurately circumscribed with the stethoscope. It is not heard during a full inspiration, but very distinctly after inspiration, or with expiration. It takes the place of the first sound at the spot where it is heard, but is followed by a distinct second sound. When the patient is breathing quickly, and the heart's action excited, it is best distinguished. It is not always of equal distinctness or of equal pitch, but it is transitory, as it may be observed extending over a long space of time. The sounds of the heart are not influenced by it. They are heard with the usual clearness at the apex, immediately above the ensiform cartilage; at the third rib, or second intercostal space, on the right side; at midsternum; and even at the third costal cartilage and edge of sternum, on the left side. At this latter situation the second sound is distinctly heard; the occurrence of the first is more difficult to perceive and more doubtful."

In the course of this inquiry two points arise on which some light may be thrown by a comparison with phenomena in health. The blowing sound took the place of the first sound over the pulmonary artery. What are the natural sounds heard there, and how produced? Secondly, full in-

spiration prevented the sound from being heard. What is the effect of full inspiration on the sounds of the artery? In answer to these questions Dr. Da Costa proceeds:

"For the purpose of an accurate standard in examinations of cardiac affections, I have studied and compared with each other, with care, the heart-sounds in the different portions of the organ, and also the effect of the respiration on them. I shall insert here as much as is relevant to the subject of the consideration of sounds in the pulmonary artery, merely adding that the usual clinical positions have been selected—the second intercostal space near the sternum, on the right side, for the aorta; on the left, for the pulmonary artery; whilst the sounds at the apex have been studied a little above the left apex, and above the ensiform cartilage. Where the term apex merely is used, it is meant to apply to the apex beat on the left side of the heart, between the fifth and sixth ribs.

"It is seen, from an examination of twenty cases, that the first sound of the pulmonary artery is usually dull, of low pitch, and, in certain cases, more like a vibration than a sound, or so indistinct as hardly to be perceptible. Compared with the first sound of the aorta, it is equal in a certain number of instances (it was in six out of eighteen in the table;) but when it differs (in eleven out of eighteen,) it is noticed to be less distinct, less sharp, although it is in some persons a rather longer sound. Compared with the first sound at the apex of the heart, it lacks the weighty, prolonged, marked character of this sound. If the stethoscope be carried up from the apex of the heart to the second interspace, the difference is observed to be very marked; the change of sound occurs almost abruptly. It seemed to me, also, as if a decided change in pitch took place; but I do not wish to speak too positively on this point, as it is a very difficult matter to distinguish changes of pitch in sounds, both of which are dull, and one very indistinct. Compared with the first sound over the right ventricle (above the ensiform cartilage,) the first pulmonic is of a much duller character, and decidedly less sharp, and not of as high a pitch. In this respect, the difference—not merely in the above observations, but in others I have made—was noticed as much more decided than between it and the first sound over the left ventricle; the one on the right side being, although less strong and shorter, usually clearer, and of higher pitch even than this.

"These analyses, showing that the sounds of the heart, listened to in different positions, differ in character, are thus in favor of the view that the first sound of the heart, as heard

over the arteries, is not merely a transmitted sound, but is one which—to a great extent, if not entirely—is generated by the coats of the vessels themselves during their diastole. They would further tend to show that the first sound of the heart, as heard over the apex on the right side, is not transmitted from one to the other, but is formed by each ventricle separately.

"As regards the second sound in the pulmonary artery, as compared with the second aortic sound, it need only here be said that the latter is usually far sharper and more accentuated. Out of the eighteen cases compared, it was stronger in fifteen, equal in two, less marked only in one.

"A second point with reference to the blowing sound capable of being somewhat elucidated by a comparison with health was, why was it inaudible during a full inspiration, and so marked in expiration? The cases given, by exhibiting what effect a full inspiration has on the sounds of the heart in health, will explain it, especially if, in addition, it be borne in mind that the sound is sometimes of the same pitch as the inspiratory murmur, and that in expiration the heart's action is quickened. Ten young adults were selected, whose hearts, as far as the extent of percussion, dulness, rhythm, and impulse, proved, were perfectly normal.

"These observations, which have been repeated on other cases with similar results, show, then, the effect of a full inspiration on the heart sounds to be an almost entire disappearance of the sounds heard at the interspace between the second and third ribs on the left side, and a very great, but not as marked diminution of the aortic cartilage. At the apex, the first sound lessens very much, and becomes in some persons almost imperceptible; the second also is modified, but proportionately less than the first. Over the ensiform cartilage, the sound lessens least. Indeed, the second sound, in four cases out of ten, was distinctly increased; and in two cases, in which the first sound became much more indistinct, the second was heard almost unchanged. These changes were all noticed in those who were capable of taking a very full inspiration and holding it for some time, and were constant on repeated examination. I believe, although the proximity of the valves to each other renders it imposible to speak conclusively, that this accentuated second sound, heard at the spot mentioned, is that of the pulmonary valves, and has become more marked, owing to the fact that during a full inspiration, the heart's action is slower, more labored, and the circulation of the lung probably interfered with; thus the

pulmonary artery would be distended, and the backward stroke of the column of blood against the semilunar valves be more forcible.

"The fact that the heart-sounds are in a full inspiration less distinctly heard at points at which they were previously well perceived, may be explained by the lung being carried in front of the heart, which it does more on the left than the right side, and, to some extent, by the relative displacement which occurs during the act of inspiration. The apex, especially, is displaced; it moves down in some persons, by several inches, towards the pit of the stomach, and becomes almost imperceptible at its previous point of impulse. This displacement seems to me to be brought about, not only by the depression of the diaphragm, but by the pressure of the lung on the left side of the heart, a fact of which I have been convinced by other observations."—*Amer. Jour. of Med. and Sciences, Jan.*, 1859.

PHYSIOLOGY.

In the July number of the American Journal of Medical Sciences, Drs. Hammond and Mitchell, two of our best known and most promising American physiologists, publish a series of interesting experiments upon the effects of two new varieties of woorara, the South American arrow poison—corroval and vao.

The personal experiments of the authors are preceeded by an excellent historical account of woorara, its physical and chemical characters, and its physiological effects on the animal system; the investigations themselves are full, accurate, and well detailed.

The following are the conclusions to which they were led, in regard to the respective action of the two varieties above named. Of Corroval:

1st. That it differs essentially from any variety of woorara hitherto described, both in its chemical constitution and physiological effects.

2nd. That it acts primarily upon the heart, through the medium of the blood, producing an arrest of the action of this organ.

3rd. That the annihilation of voluntary and reflex movements is a secondary result of its action, depending primarily upon the discontinuance of the function of the heart.

4th. That it acts upon the nerves from the periphery to the centre, and abolishes both the sensory and motor functions.

5th. That it destroys muscular irritability.

6th. That it paralyzes the sympathetic nerve, this being one of its primary effects.

7th. That it is absorbed both from the intestinal canal and skin of frogs.

8th. That its poisonous qualities are due to an alkaloid hitherto undescribed.

1. Vao, either in a solid, or more quickly in a liquid form, can be absorbed from the areolar tissues of cold-blooded animals, as the frog.

2. It is also absorbed by the stomach, œsophageal mucous membrane, rectum, and skin, with a degree of rapidity which varies, and is rapid or slow as the animal is ill or well-supplied with water.

3. Warm-blooded animals absorb vao from the stomach and intestine when they are fasting, but suffer no ill effects when the vao is given during digestion. That this protection is not due to a mere mixture of the vao with the food of the full stomach, is shown by the fact that rabbits, whose stomachs are always more or less distended with food, are protected only when, owing to the entry of fresh food, digestion becomes active.

4. The demands of the system for water do not affect to any perceptible extent the absorption of vao from the stomach of the rabbit.

5. The circulation of the frog is arrested within from ten minutes to one hour by the introduction of vao under the skin. The same result obtains within from twenty-four to forty-eight hours, when the poison is swallowed in small doses.

6. The first effect of vao is to increase the force of the heart without increasing the number of its pulsations.

The next effect is a paralysis of the muscular tissues of the heart, so that the ventricle stops first, and the right and left auricles next, in the order in which they are named. In a majority of frogs poisoned by vao, the heart remained galvanically irritable for a certain time after the organ had ceased to pulsate.

The heart stops before the voluntary motions are at an end, in all cases of rapid poisoning. When poisoning occurs by absorption from a mucous surface, the phenomena march more slowly, and voluntary control and reflex power are sometimes lost before the heart has entirely ceased to beat.

7. Vao stops the respiration in warm-blooded animals by arresting the circulation, and thus paralyzing the nervous system, without which respiration is impossible, so that the checked respiration is a consequence and not a cause of the injury to the cardiac functions.

In the batrachia also, the respiratory movements cease before the heart has entirely lost the power to pulsate.

In the alligator poisoned by vao, the respiration is perfect some time after the heart is at rest.

8. The facts last quoted, and the inability of artificial respiration to restore or sustain the cardiac movements in warm-blooded animals poisoned by vao, proves sufficiently that the first effect of the poison is upon the heart, and that the appearance of asphyxia observed post-mortem in rabbits, cats, etc., are of secondary importance so far as concerns the cause of death.

9. The temperature of warm-blooded animals poisoned by vao falls with considerable rapidity, and does not undergo any elevation after death.

10. The nerves of sensation first lose their power to convey impressions—the motor nerves are next affected. The paralysis of the nerves extends from the periphery to the centre. The affection of the nervous system may be due to the sudden arrest of the circulation, and not of necessity to the direct influence of the vao. The irritability of the voluntary muscles in the frog is lost much earlier than is the case when the animal dies by decapitation.

11. The sympathetic nerve is paralyzed, at least in the upper portion of its distribution, before the nerves elsewhere have lost their functional power.

12. The ciliary motion is unaffected by the use of vao.

13. The blood of animals thus poisoned congulated as usual, and had not lost the power of changing color when exposed to oxygen or carbonic acid.

14. So far as we are aware, no true physiological antidote exists for vao poison, since even artificial respiration fails to sustain life in animals affected by it.

15. The vao poison closely resembles corroval in its physical, chemical, and physiological reactions. The alkaloids extracted from the two poisons produce in animals of equal size effects which cannot be distinguished.

We, therefore, are inclined to consider vao as merely a weaker variety of corroval, and to conclude that the apparent difference in the effects produced by the original extracts is due to a difference in their strength.

HABER: *On the Action of Curare on the Cerebro-Spinal Nervous System.* (Achiv. f. Anat. und Physiol., Jahrgang 1859, p. 98.)

VON WITTICH: *On Peculiar Muscular Contraction caused by the flowing of Water through the Vascular System.* (Virchow's Archiv., vol. xiii., p. 421, 1858.)

Haber bases the view that the contractions of the muscular fibres observed after poisoning by curare (see Sub. V.) are independent of the motor nerves, especially on the character of the muscular contractions, and on the manner of termination of the motor nerves in the tissue of the muscles, as elucidated by Reichert. This author found that the single fibres of the nerves are spread over a comparatively large space, thus comprising several muscular fibrillæ. "The whole peripheric distribution of the motor nerve fibres," he reasons, "the frequent ramifications of the fibres entering the muscle, manifest the principle to bring each nerve fibre in contact with as many muscular fibres as possible" From this distribution, Reichert inferred that the irritation, originated by a single nerve fibre, is not restricted to a limited number of muscular fibres, but acts on the whole muscle, or at least a great portion of it. Experiments show that the local irritation of a muscle of an animal recently killed, causes contraction of a considerable number of muscular fibres; while, on the contrary, each local irritation in animals poisoned by curare, causes only a very restricted contraction of fibres, in fact, only the fibres actually irritated, the other part of the muscle remaining perfectly motionless. It appears from this that the *independent irritability of the muscular fibres is at last firmly established.*

Wittich studied the nature of the convulsions produced, as E. Weber and Liebig already had observed, by injection of water through the aorta of animals soon after their death. By causing a stream of water to pass through the aorta of frogs, convulsions are occasioned, first in the muscles of the head and neck, then in those of the arms and abdomen, and at last when the blood is altogether expelled through the veins, also in the posterior limbs. Distilled water caused, of all fluids tried, the most powerful and long-continued convulsions; spring and river water less so; serum and saline solutions had scarcely any effect. Destruction of the nervous centres and section of the nerves had no influence on the appearance of the convulsions. Frogs poisoned by brucine

and curare exhibited, when distilled water was injected, powerful convulsions, while galvanic irritation of the nerves had ceased to excite the muscles. Wittich further convinced himself that distilled water alone, when applied on a frog's muscle deprived of skin, causes contractions. The author infers that pure water alone, without the influence of the nerves, causes the phenomena in question, and that water is a powerful excitant for the muscles, while it appears to be an indifferent agent with regard to the nerves. The endosmotic interchanges between the tissue of the muscle and the distilled water offer the probable explanation."—*Brit. and For. Med. Chirur. Rev., July*, 1859.

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On the Physiology and Pathology of the Nervous System. By Dr. BROWN-SEQUARD. ('*Lancet,' December 25th*, 1859.)

The principal points which Dr. Brown-Sequard endeavours to establish in connection with the physiology and pathology of the central nervous system are these:—

1st. Excitations of the anterior roots of the spinal nerves may be a cause of pain, because these roots, being motor, produce a *cramp*. The pain due to this cramp is what has been erroneously called *recurring sensibility*. Cramps, and several other kinds of painful spasms (of the uterus during parturition, of the sphincter ani in certain cases, &c.) are painful on account of a galvanic irritation of sensitive nerves accompanying muscular contractions.

2d. Our movements seem to be guided by the peculiar sensations we derive from the galvanic irritation of certain sensitive nerves of muscles, while they contract.

3d. The power of transmitting sensitive impressions exists in many parts which are not able to give pain or any other sensation when they are excited by our usual means of irritation; so it is with the gray matter of the spinal cord and with many parts of nerves, which, however, are conductors of sensitive impressions.

4th. Hyperæsthesia is a constant result of certain injuries or alterations of the posterior parts of the cerebro-spinal axis, from the tubercular quadrigemina down to the lower end of the spinal cord.

5th. The transmission of sensitive impressions, in the spinal cord, takes place chiefly through the gray matter, and partly through the anterior columns; but, before reaching the gray

matter, the impressions, in a certain measure, pass through the posterior columns.

6th. The conductors of sensitive impressions from the trunk and limbs decussate in the spinal cord, and not in the encephalon, as was universally admitted.

7th. Although the spinal cord is greatly altered or injured, sensibility, more or less diminished, may persist everywhere, on account of a peculiar arrangement of the conductors of sensitive impressions.

8th. The various kinds of sensitive impressions seem to be conducted by quite distinct nerve-fibres, in the nerves and in the nervous centres, and the place of passage of some of these conductors in the spinal cord seems not to be the same as that of the others, but none of them go up to the sensorium along the posterior columns.

9th. In the upper part of the cervical region of the spinal cord, near the medulla oblongata, most of the conductors of the orders of the will to muscles are in the lateral columns, and in the gray matter between these and the anterior columns.

10th. The voluntary motor conductors decussate at the lower part of the oblong medulla, and not all along the median line of the base of the encephalon.

11th. The posterior columns of the spinal cord have a great share in reflex movements, and this is the principal cause of the peculiar kind of paralysis so often observed in cases of alteration of these columns.

12th. The effects of excitation of the vaso-motor nerves consist essentially in a contraction of blood-vessels, which is followed by a diminution in the quainty of blood, in the temperature, and in the activity of nutrition. The effects of interruption of continuity of the vaso-motor nerves (*i. e.*, their paralysis) consist essentially in a paralytic dilatation of blood-vessels, which is followed by a greater afflux of blood, an increase of temperature, and a greater activity of nutrition.

13th. As a great many vaso-motor nerve-fibres go up to the brain and to the cerebellum along the spinal cord, the medulla oblongata and the pons Varolii, the diseases or injuries of the various parts of the cerebro-spinal axis, besides symptoms concerning sensibility and movement, present symptoms, depending upon irritation or paralysis of vaso-motor nerves; contraction or relaxation of blood-vessels, diminution of temperature, alterations of nutrition, of secretions, &c.

14th. Besides the influence of the nervous system upon nutrition, absorption, and secretion, through the vaso-motor

nerves, there is another, which seems to consist in changes in the elements of the tissues—changes producing various modifications in the quantity of blood attracted, and in the interchange of materials between the blood and the tissues.

15th. The absence of the influence of the nervous system on any part of the body is hardly a cause of other alterations of nutrition than atrophy, while the irritation of the nervous system is a most powerful direct or reflex cause of a great many morbid changes in nutrition, secretion, &c.

16th. The sympathetic normal and morbid changes of nutrition, secretion, &c., are reflex phenomena, the study of which shows how many diseases are produced by a reflex action, and how a rational mode of treatment might be arrived at.

17th. The loss of consciousness in simple vertigo or in complete attacks of epilepsy does not depend upon a disease of the brain, but upon a contraction of the blood-vessels of the cerebral lobes—contraction due to some irritation of the vasomotor nerves of these vessels, either by some direct cause irritating them in the base of the encephalon or the spinal cord, or by a reflex influence.

18th. Much more frequently than has been imagined, all the following affections may be produced by a peculiar kind of irritation starting from almost any centripetal part of the nervous system: epilepsy, the various forms of insanity, chorea, catalepsy, hysteria, tetanus, hydrophobia, &c.

19th. The medulla oblongata is neither the only nor an essential nervous centre for the respiratory movements.

20th. There are a great many nerve-fibres and nerve-cells in the medulla oblongata, the pons Varolii, and the other parts of the base of the encephalon, which are not employed in the transmission of sensitive impressions or of the orders of the will to muscles, and are endowed with the singular property of producing, after even a slight irritation, a *persistent* spasm in certain muscles, and especially in the neck. Rotatory convulsions very often depend chiefly upon the production of such spasms, and of changes in the blood-vessels of certain parts of the encephalon.

21st. The irritation of the auditory nerve may cause rotatory or simple clonic convulsions.

22d. The conductors of the orders of the will to muscles, of the sensitive impressions, and of the nervous influences to blood-vessels, decussating at different places in the cerebro-spinal axis, various symptoms are to be observed, depending upon either the irritation or the paralysis of these three kinds

of conductors, according to the part of a lateral half of the cerebro-spinal axis where an alteration exists.

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Adaptation of the Human Eye to Distances. By Mr. CHARLES ARCHER, Surgeon in the Bengal Army. (*'Proc. of the Royal Society,'* 1858.)

The following is a summary of the author's views:—

1. The eye is adapted to varying distances principally by an alteration in the fibrous arrangement of the lens itself. Moreover, that when the lens is removed after an operation this power is nearly lost, and can only be exerted within very confined distances.

2. The purpose of focalizing light at short distances is doubtless assisted, as suggested by Bowman, by the contractions of the ciliary muscle, in its antero-posterior direction, bringing forward the ciliary process.

3. As the posterior hemisphere of the capsule is firmly united to the hyaloid membrane, this portion must always remain quiescent, and therefore the antero-posterior contractions of the ciliary muscle must be very limited as regards the lens.

4. The ciliary muscle, being placed around the eye, and its fibres being of a somewhat plexiform character, the contractions of the muscle will relax those yielding portions of the lens placed within its circumference.

5. The relaxations of the ciliary processes will deprive the capsule of its firm support. It will be pressed forward by the lens, which will meet with no further resistance to the elongation of its short axis.

6. The lens itself, as microscopically described by Bowman and Kölliker, is admirably adapted to the varying changes which take place in the capsule.

7. The posterior capsule being firmly united to the hyaloid membrane, the alteration in the diameters of the cavity of the capsule must take place from the periphery of the lens to its centre, and from behind forwards, but not from before backwards, on account of the close union of the posterior capsule to the hyaloid membrane.

8. To allow such alteration to take place without endangering the achromatism of the lens, the alterations in the plane of its long diameter must be synchronous with the alterations in the plane of its short diameter. To allow of this, the mar-

gin of the lens is free in the canal of Petit; were this not the case, chromatic aberration would result.

9. The elasticity of the capsule of the lens and the ciliary muscle are antagonistic; that on the ciliary muscle becoming relaxed, the capsule of the lens is free to exert that elasticity.

10. By the pressure exerted by the anterior hemisphere of the capsule, by means of the polygonal cells of Virchow, on the anterior face of the lens, the organ is able to fulfil all the requirements for adapting it to receive focalized light from long distances.

11. The polygonal cells of Virchow are placed on the posterior surface of the anterior hemisphere of the capsule with the view before mentioned, and they are arranged with their long diameters in an antero-posterior direction, that pressure may not injure their transparency, which would be the case if placed laterally.

12. These cells are not found in other parts of the capsule.

13. The fibres of the lens are serrated for the purpose of uniting either to the other, so as to allow them freedom of motion without altering their ultimate relations to each other.

14. The ciliary muscle is very highly endowed with nervous matter to supply all these varying requirements.

15. By the above postulates, all the modern discoveries in the microscopical anatomy of the eye receive a distinct expression of their individual functions, and by so doing, adapt the organ of vision to the acknowledged laws of light.

SURGERY.

On the Treatment of Hernia by Electricity. By Dr. Clemens. (Deutsche Klinik, 1858, No. 34.)

In the first of a series of articles upon applied electricity, the fruits of ten years' investigation, Dr. Clemens gives an account of his employment of it in the treatment of hernia. He commenced its use in 1850, by endeavoring to produce a diminution in the size of the hernial apertures in a case of large double inguinal hernia. One of the poles of the battery, a massive metalic knob, was introduced deep into the canal, pressing a flap of skin inwards, and a moderately strong galvanic stream, increased daily, was passed along

this during six minutes. At the end of a week, the hernia protruded less easily, and the apertures had become narrower. When the examination was made before and after each *seance*, a great difference was always found in the accessibility and size of the sac—an observation since repeated hundreds of times. The application of the galvanism also has the good effect of increasing the peristalic movements, which in becoming more energetic, effect a favorable change in the position of the intestines, by altering the situation of the portion which had so long remained opposite the aperture, and had consequently become relaxed. A more complete evacuation of the contents of the canal is also brought out. The impaired vitality of the intestine, of the hernial canal, and of the abdominal coverings is always renovated through this application of electricity. When the hernia is recent, no means of treatment is so certain and so exempt from all danger; and even when the hernia long has protruded, it has often been returned under the influence of the galvanic stream or the electrical flask. Dr. Clemens has usually preferred friction-electricity to galvanism, as its operation is more rapid and its effects are more energetic. Among twenty-seven patients so treated, none have complained of the least unpleasantness; but, on the contrary, they have found many inconveniences disappear under its influence, and especially obstinate constipation. In very sensitive persons diarrhœa may follow an energetic *seance*. When a hernia has been recently produced, as by a fall, lifting, &c., the success of the method is often surprisingly rapid, and in marked contrast with the slow progress of treatment by trusses, &c. A double hernia thus produced was cured without any bandage in twenty *seances*, and has remained so now for two years—the treatment only commencing a week after the accident.

Dr. Clemens states that for large hernia, which can only be kept up imperfectly by any ordinary truss, he has contrived a galvanic truss, which operates with remarkable efficacy. It is constructed of copper and zinc plates, or pieces of copper and silver money, having felt or leather interposed, which is kept moistened by the saline solution necessary for the excitement of the pile. He again dwells upon the importance of exciting peristaltic action, not only in hernia, wherein it may ob[illegible] strangulation being produced, but in various [illegible] due to a sluggish movem[illegible] upon animals, [illegible] effects re[illegible]

New Mode of Relieving Retention of Urine. LANGSTON PARKER, Esq., Surgeon to Queen's Hospital, Birmingham (*Brit. Med. Jour.*, May 21, 1859.)

Dr. Parker states that he has very recently succeeded, in two separate instances, in relieving retention of urine in the following manner:

"A gentleman lately entered my consultation-room in great pain from retention of urine. He had not passed water for many hours; the bladder was much distended. He stated that ineffectual efforts had been made to pass a catheter, during which operations he had lost a considerable quantity of blood. I attempted to relieve him by the catheter, but failed to do so; I tried instruments of various sizes and various curves, but could not succeed in passing one into the bladder. I then took a No. 2 wax bougie, and inserted a small portion of potassa fusa into the end of it, after the manner proposed by Mr. Whateley, and practised by Mr. Wade, in the treatment of permanent stricture of the uretha. I well moulded the wax over all but the extreme point of the caustic, and passed it rapidly down to the point of obstruction; by pressing against this for a short time, it yielded, and I had the satisfaction of finding the bougie easily enter the bladder. I directed the patient to strain as I withdrew the instrument; a stream of urine followed, and the bladder was emptied. The retention did not again occur, and very little irritation accompanied or followed the proceeding. On the next day, the patient made water freely, but in a small stream.

"The second case was very similar. The patient had travelled some distance by rail. The bladder was much distended, the symptoms urgent, and a catheter could not be made to enter the bladder. A small wax bougie was armed as in the last case, passed down to the stricture, and firmly pressed against it. It yielded very quickly; the instrument entered the bladder, and a stream of urine followed its withdrawal. The patient had a second attack of retention two days afterwards, which was completely relieved in the same manner.

"A modification of this plan might be attempted by inserting a small piece of potassa fusa into the extreme point of a small gum-elastic catheter, and using it without the stilette. I am sanguine enough to hope that many cases of retention of urine might be easily and quickly relieved by the simple

means I have suggested, and more formidable and dangerous operations thus frequently avoided."

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Dislocation of the Shoulder Joint.—Mr. T. BRYANT, Assistant Surgeon to Guy's Hospital, has published (*Med. Times & Gaz.*, May 14, 1859) some interesting remarks on this subject, based on thirty-four cases which have come under his observation.

The following are his conclusions:

1. The necessity to make an early and correct diagnosis.

2. That the head of the humerus may be dislocated in any position downwards, backwards, and forwards, these directions being the only possible ones into which the head can pass.

3. That dislocation downwards is by far the most common form; and dislocation backwards the least.

4. That a direct blow upon the shoulder is by far the most frequent cause of dislocation.

5. That dislocation of the humerus is more common in old people than in young and middle aged.

6. That it is most frequent in men.

7. That reduction is comparatively an easy task under the influence of chloroform.

8. That spasm of the muscles is not the principal impediment to the reduction of the bone; but that the lacerated capsular ligament may be regarded as the chief obstacle.

9. That simple extension with the heel in the axilla, when the patient is under the influence of chloroform, is generally sufficient to reduce the bone; in many cases the treatment by manipulation as described will be followed by success.

10. That up to six or seven [illegible] reduction may, as a [illegible] be consid[illegible]

11. That r[illegible] may b[illegible] two months, [illegible]

12. That i[illegible] some weeks, [illegible] to attempt re[illegible]

13. That in [illegible] extension or wan[illegible] ly described of ste[illegible] of the bone, by the [illegible]

should be employed; as being simple it can do no harm, and may succeed in other cases as well as in the example given.

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Bismuth in Gleet and Leucorrhœa.—M. Gaby states that repeated trials have convinced him that injections of oxide of bismuth constitute the very best treatment of *gleety discharges.* Thirty parts are suspended in 200 of rose-water, and so injected as to leave as large a deposit of the salt as possible in the canal. Three injections *per diem* should be employed at first, and then fewer. He has collected forty-three cases thus treated with success, five of which he briefly relates. *Urethral discharges*, unconnected with gonorrhœa, as observed in certain diatheses, in masturbation, venereal excesses, &c., and increasing in quantity even after pure connection, have been treated by this means in three cases. *Balanitis* and *balano-posthitis*, and *herpes præputialis* yield rapidly to bismuth applied in powder after cleansing the part, and then covering with cotton. The various forms of *vulvar leucorrhœa* may be treated with bismuth. One of these is entirely confined to the vulva, whether appearing as a consequence of follicular vulvitis or without preceeding inflammatory symptoms. The latter is often met with in little girls. Pregnancy, want of cleanliness, masturbation, worms, or contusion, are among the exciting causes. After removing all complication the bismuth acts upon the discharge like a specific. In the leucorrhœa of girls, powdering with bismuth is an excellent remedy. In ordinary vaginal leucorrhœa, occurring in women otherwise healthy and having no other affection of the genito-urinary organs, the bismuth succeeds well. The cases of *urethro-vaginal* leucorrhœa are almost always of infectious origin. They have in some instances yielded to bismuth when resisting obstinately other remedies. It is to be remembered that all the cases in which the bismuth is useful are of the chronic description; and that pain and other signs of acute inflammation contra-indicate its employment. —*Med. Times & Gaz.*, May 28, 1859, from *Bull. de Therap.*, tome lv.

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Caries of the Spine, fatal by Hæmorrhage from the Aorta. Under the care of H. W. FULLER, Esq.

The following case is an example of a complication which

sometimes, though rarely, accompanies caries of the bones; viz: ulceration of one of the neighboring arteries. We have seen one of the branches of the carotid laid open in caries of the jaw, and the hæmorrhage from this vessel proved fatal. In the subjoined case, however, the original disease was so latent, and the history so imperfect, that no acumen in diagnosis could have led to the detection of the state of parts. It is quite clear that the extravasation had occurred after the patient's entrance into the hospital, and that the syncope noticed on the day subsequent to this was the sign of its occurrence. The man then rallied for a short time, the opening in the vessel being probably closed by a clot, which was again detached in the effort which he had been making just before his death.

James G., aged 30, was admitted on June 1st, under Dr. Fuller's care. He was a house-painter, said to be of dissolute habits, and had been ill since Christmas, suffering from pain in the loins and left hip. The pain was much increased by stooping. He complained of inability to empty the bladder, but not of pain in micturition. He had had rheumatism once, four years ago. He had been able to work till within a few days of his admission. At that time he seemed very languid and weak; his complexion was very sallow; skin cold and moist; tongue pale and furred. The pulse was 120, and very weak. The appetite was gone, and he had had but little sleep for some nights. He was ordered some opening medicine. Next morning, the bowels had not been open, so that the purgative was repeated. A swelling was now noticed on the left side of the abdomen. On getting out of bed to go to stool, the patient fainted, and complained of much pain on the left side. Next day, the swelling was carefully examined. It seemed to be on the left side of the middle line; was solid, smooth, and hard to the touch; and extended from the diaphragm to the level of the anterior superior spine of the ilium. It was solid, hard, and pulsated visibly; but no *bruit* was heard. The urine was scanty, and contained lithic acid. There was tenderness and pain in the region of the tumour, more especially in its lower part. No positive diagnosis of the nature of the tumour was arrived at; but it was thought possible that it might be an enlarged spleen, deriving pulsation from some vessel against which it rested. The blood was accordingly examined, and was found to contain an unusual quantity of white corpuscles. The bowels were opened by an injection, and a morphia draught was administered at night. Next day he was better.

There was not so much tenderness in the abdomen, but much flatus. At 9 P.M. he went to stool, and was getting into bed again, when the nurse's attention was called to him, as having suddenly fainted. He died in a quarter of an hour.

On *post mortem* examination, it was found that the anterior surfaces of the two last dorsal and two upper lumbar vertebræ were extensively carious; and in one of these bones a cavity, more than half an inch in depth, and of a rounded form, was found. The intervertebral discs seemed healthy. The tumour which was observed during life was found to have been formed by a large clot of blood, firmer in some parts than others, but not laminated or decolorised in any part. The greater part of it had about the consistence of currant jelly. A quantity of fluid blood was found in the cavity of the peritoneum, but the part where this membrane had given way was not discovered. In the greater part of its extent, it was stretched firmly over the front of the clot. The left kidney was embedded in the clot, and the pancreas and descending colon adhered to it.

The abdominal aorta was pervious throughout, and lay on the right side of the coagulum. Opposite the diseased vertebræ, there was an ulcerated opening in the coats of the aorta, about as large as a silver penny, around which the coats of the vessel were thickened, projected into the cavity of the vessel, and appeared of a red color, as though from the effusion of blood between them. It was observed that this opening corresponded exactly to the position of the openings of the lumbar arteries out of the aorta. There did not appear to be any psoas abcess; and if any pus had been in connection with the diseased bones, its presence was masked by the clot.

As a companion to this case, we append another case of internal bleeding, in this instance from injury, in which the same thing took place; a temporary check being soon followed by the re-opening of the vessel, and the recurrence of fatal hemorrhage. The injury in this latter case was a curious one, since the omentum was not adherent below; so that it is difficult to conceive why this moveable membrane should give way to the blow, in preference to the more fixed viscera with which it is connected.—*British Medical Journal*, July 9th, 1859.

Laceration of the Great Omentum from a Blow. Under the care of E. CUTLER, Esq.

John B., aged 44, was admitted on May 19th, under the care of Mr. Cutler, having been kicked by a horse just to the right of the umbilicus. Some brandy had been given him, after which he had vomited. When admitted, he rolled about in great agony, and would allow no one to touch the part where he had been kicked. There was no swelling. He vomited again soon after admission, bringing up the brandy and food he had taken. There was no blood in the vomit. His face was very pale; pulse slow and weak. Twenty leeches were applied to the painful spot, and he was ordered three grains of calomel, and one of opium, every three hours. During the day he continued very much collapsed, and in great pain. He retched much, but was not actually sick. The motions and urine were passed free from blood. Next day, his countenance was less distressed, and he was in less pain; still he could bear no pressure on the part. He had been frequently sick, but had brought up merely his food. The pulse was ninety, stronger. One dose only of the draught and of the calomel was given this day. Next day (May the 21st) the improvement continued. He had not been sick since the previous afternoon, though he still retched a good deal. The tenderness of the abdomen was less; the pulse fuller; the tongue clean. He had passed a fair night. As the bowels had not acted since just after his admission, a simple enema was given.

May 22nd. He was greatly improved. There was no more retching; the tenderness was much less; the bowels had acted four times naturally. During the evening, he had some griping pain about the injured part; but this was quite relieved by the administration of a grain of opium, with two of calomel, till next afternoon, at 2½ P.M., when he was seized suddenly with faintness, almost leading to fatal collapse, and pain in the belly. His pulse was scarcely perceptible, and the sweat poured off his body. Restoratives were given, but he continued to sink. Distressing vomiting came on, which lasted till his death on the following day, at 1 P.M. Calomel and opium were given, and he was kept alive by means of brandy; but nothing seemed to give him any real relief.

On *post mortem* examination, a large quantity of blood, partly solid, and partly fluid, was found in the cavity of the

peritoneum. The surfaces of this membrane did not look inflamed. On examination, a laceration was found at the root of the great omentum, near its attachment to the small end of the stomach. The omentum was here a good deal contused; and several small rents existed in it, both in the descending part, and in the part passing on to the colon. In one of these, a large artery was found laid open; and, by pressing on the part, grumous blood could be made to start up from the divided vessel. The viscera of the abdomen were all uninjured and healthy.—*Ibid.*

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Large Scrotal Hernia containing the entire Cœcum.—A rarity in the way of hernial tumours came under the care of Mr. Bryant, at Guy's Hospital. A young man was admitted with a very large scrotal hernia. It was as big as a child's head, and had been strangulated seven hours. There had been a tendency to protusion for seven years past. The symptoms were most acute, and vigorous attempts at reduction had been made prior to the man's admission. After a gentle endeavour to effect the taxis under the influence of chloroform, Mr. Bryant proceeded to the operation. It was found impossible to release the stricture without opening the sac. The latter measure having been adopted, between three and four feet of intestine were exposed, including the whole of the cœcum and its appendix. The cœcum was quite loose, and with it was a part of the commencement of the colon. The bowels acted about ten hours afterwards, and the man then seemed in a hopeful state. Symptoms of acute peritonitis, however, developed themselves, and he sank on the third day. The coils of intestines were found at the autopsy glued together by recently effused lymph, and the parts which had been in the sac were intensely congested.—*Med. Times and Gazette*, June 4, 1859.

EDITORIAL AND MISCELLANEOUS.

DEATH OF HUMBOLDT.

The recent notices of the gradual decline of this illustrious European philosopher have prepared the public for the intelligence of his decease, which is announced as having taken place in Berlin on the 6th of May. Had he lived but a few days more than four months longer, he would have completed the ninetieth year of his age. His fame belonged not only to Europe, but to the world, and in this country especially, probably no man who was known to us only through the medium of his scientific writings, was held in equal reverence and admiration. The simple record of his life forms the noblest monument to his memory. We need only gather up the successive facts in his career to show that the "man of the century" has been taken from the world.

Freidrich Heinrich Alexander von Humboldt was born in Berlin, Sept. 14th, 1769. His father, who was a man of military and civil distinction in Prussia, died when his son was but ten years old, but the latter still enjoyed the advantage of a singularly careful education. He studied at Frankfort-on-the-Oder, at Berlin, and at Gottingen, devoting himself chiefly to the natural sciences, to the Greek language, and to the application of science to the practical arts. Among his early instructors in the former were Blumenbach, Beckmann Lichtenberg. His university studies were varied by excursions to the Hartz Mountains, and to the vicinity of the Rhine, which occasioned the first fruits of his literary labors, in a work entitled, "The Basalt on the Rhine" (1790).

In the spring and summer of that year, accompanied by one or two congenial friends, he made a tour through Belgium, Holland, England and France. The acquaintance of Sir Joseph Banks, and the enthusiasm of one of his companions, who had made a voyage to the South Sea, awakened the desire for visiting tropical regions, and gave a coloring to his future life. On his return from England, he passed some time at a commercial academy in Hamburg, with a view of engaging in the pursuits of trade. But his inclinations strongly tended to the cultivation of physical science, and in 1791, he repaired to the School of Mines at Freiberg, where he received private lessons from the celebrated Werner, and enjoyed the friendship of Leopold von Buch, Freiesleben, and Del Rio. His researches in this locality led to the preparation of a treatise on the fossil botany of Freiberg, which was published a year or two later, (1793). A larger work on the physiology of the nerves and muscles (1797–99) attests the predominant tendency of his mind.

The death of his mother in 1796 removed one of the obstacles to his cherished purpose of a grand scientific expedition to the tropics. After pursuing an extensive course of study in the application of astronomy to geography, and engaged in meteorological observations with Von Buch, he went to Paris, where he first made the acquaintance of Bonpland, with whom he passed the winter of 1798 in Madrid. This was, in fact, the turning point of his life. Such a favorable impression did he make on the Spanish authorities, that he received permission to visit at pleasure any of the Spanish possessions in America or the Indian Ocean, with a guaranty of the free use of his astronomical instruments, and of no restraint in the collection of specimens in natural science.

In June, 1799, he embarked, in company with Bonpland, for South America, and arrived at Cumana in the middle of July. The next eighteen months were spent in an exploring tour through Venezuela; and in February they left the sea coast for the south, with a view of reaching the river Apure and the Orinoco. They passed over the cataracts of Atures and Maypures in Indian canoes to Fort San Carlos on Rio

Negro, about two degrees from the equator, and after a weary journey through the wilderness, returned to the Orinoco, of which they made the first exact scientific observation. From Cumana, at which they arrived in safety after their perilous excursion, they sailed to Havana, where they remained several months. They next went to Bogota, the vicinity of which they carefully explored, and in September, 1801, continued their journey to the south, arriving at Quito, January 6, 1802. Here they remained until the following June, pursuing their researches in the volcanic region, and ascending to heights which had never before been trodden by the foot of man. On June 23, 1802, they reach a height of 19,230 feet on the Chimborazo, which was more than 3000 feet higher than the point attained by La Condamine in 1738. Here they planted their instruments upon a narrow ledge of rock which projected from the vast field of unfathomed snow. A broad impassible chasm prevented their further advance; they were enveloped in thick fogs, and in an atmosphere of the most piercing cold; they breathed with difficulty, and blood burst from their eyes and lips. Only once has this elevation on the Chimboraza been surpassed, when Boussingault, in 1831, attained a summit of 19,600 feet, by a different path from that chosen by Humboldt.

Crossing the passes of the Andes, the travellers pursued their way to the upper valley of the Amazon, and made a thorough exploration of that portion of Peru. In December they sailed from Gallo to Guayaquil, and after a second tedious voyage, arrived at Acapulco, March 23, 1803. Thence they proceeded to Mexico, where they remained for several months, pursuing their researches in the volcanic regions, and making rich and valuable collections in natural history. In March, 1804, after exploring numerous localities on the Mexican coast, Humboldt sailed to Havana, where he remained for two months, gathering and arranging the materials for his "Political Essay on Cuba" (Paris, 1826.) He next directed his attention to the United States, and accompanied by Bonpland, visited Philadelphia and Washington, where he

was treated with distinguished attention by President Jefferson, and after a brief sojourn, sailed for Bordeaux, in July, 1804.

He now took up his temporary abode in Paris, where, in connection with Gay-Lussac, he devoted himself to chemical researches, chiefly on the composition of the atmosphere, until the following March. After a journey to Italy, he returned to Berlin in December, 1805. In 1807 he accompanied Prince William of Prussia on a political mission to France, and with a view to the publication of his works, made Paris his residence until 1827. The first portion of his great work, "Voyage to the Equinoctial Regions of the New Continent," was published in Paris in 1808. In the winter of 1827, he delivered a course of lectures in Berlin on the "Cosmos," which were the foundation of his celebrated work of that name.

In 1829, Humboldt was commissioned by the Emperor Nicholas to make an expedition to the Altai and Ural Mountains, with a view to examining the mineral treasures of those regions. The tour extended through Moscow, Kasan, Tobolsk and Barnaul, to the Chinese frontier. He was accompanied by his two friends, Ehrenberg and Gustav Rose. The journey, which occupied nine months, is described in "Central Asia" (Paris, 1843.) This journey furnished the most valuable contributions to the science of physical geography. New light was thrown on the volcanic agencies in Central Asia; many important facts were set forth on the soil, climate, and connection of the mountain chains; and owing to the interest which it awakened, a regular system of observations was established by the Imperial Academy of St. Petersburg, throughout the Russian empire, for the purpose of noting the changes in meteorological phenomena.

The political movements of 1830 gave the activity of Humboldt a political direction to a certain extent, but without diverting his attention from the pursuits of science. In May, he attended the Crown Prince of Prussia to the last Diet of Warsaw, and soon after was with the King at Teplitz; and on the accession of Louis Philippe, was commissioned by

Frederic William III. to acknowledge the new dynasty at Paris, and to forward political intelligence from that capital to Berlin. He received many similar commissions within the course of the next twelve years, which required him to spend a considerable portion of his time in Paris.

Since 1842, Humboldt has resided at Berlin, engaged in the preparation of "Cosmos," which sums up the observations of a long life on what may be termed the "Harmonies of the Universe," receiving visits from strangers of every nation, who were attracted by his scientific fame, carrying on an extensive correspondence with men of learning in various departments of research, and cherishing the curiosity of youth, with regard to every new discovery and phenomena in the realms of nature.

Humboldt was remarkable for combining the fruits of his own observations with the exhibition of whatever had been accomplished by other investigators in the field of physical inquiry. In this comprehensive department he was equally eminent as an explorer and a historian. As he detected every phenomenon with wonderful sagacity, so he recorded every discovery with unparalleled fidelity. This is not the place to give an account of his contributions to science. His positive additions to the treasures of human knowledge would fill many volumes. But what will ever distinguish Humboldt from the mass of physical inquirers who had preceded him, is his study of the universe as a harmonious whole, and his search for the laws of order, beauty and majesty beneath the apparent confusion and contradictions of isolated appearances.—*N. Y. Tribune.*

IDIOSYNCRACIES.

Mr. Munn, Assistant Surgeon to the Middlesex Hospital, says—"Instances of the poisonous effects, on certain constitutions, of drugs ordinarily of moderate action, are not unfamiliar in medical practice. Ipecacuanha is, perhaps, one

of the best examples of a drug that, even in a state of the minutest subdivision, is capable of producing, idiosyncratically, extreme results. But some articles of diet also are, to individuals, poisonous; a numerous class of persons might be easily found, the members of which could not take, without inconvenience, one or other of the various alimentary substances. The object of this communication, is, however, not to discuss the physiological questions which naturally arise in the consideration of such a subject; it is rather to offer an instalment of facts.—J. M., an occasional patient, cannot eat rice in any shape without extreme distress. From the description given of his symptoms, I believe spasmodic asthma to be the cause of his discomfort. On one occasion, when at a dinner party, he felt the symptoms of rice-poisoning come on, and was, as usual, obliged to retire from the table, although he had not partaken of any dish ostensibly containing rice. It appeared, on investigation, that some white soup, with which he had commenced his dinner, had been thickened with ground rice.—A gentleman who, as in the preceding case, could not eat rice 'without being suffocated,' took luncheon with a friend in chambers. The fare was simple—bread, cheese and *bottled* beer. On the usual symptoms of rice-poisoning seizing him, he informed his friend of his peculiarity of constitution. The symptoms were explained by the circumstance of a few grains of rice having been put into each bottle of beer for the purpose of exciting a secondary fermentation.—A gentleman, some time since under my treatment for stricture, informed me that he could not eat figs without experiencing a most unpleasant formication of the palate and fauces; and that the fine dust from split pease produced the same sensation, accompanied by a running at the nose. The father of this gentleman suffers from hay-fever at certain seasons.—Mr. P., himself a gentleman of a peculiarly nervous temperament, states that his father cannot endure the sensation produced by handling a russet apple. He also communicated to me that of Mr. T., who cannot remain in a room in which there is a cooked hare, on account of the peculiar effect produced on his sys-

tem. Miss ———, after eating eggs, suffers from swelling of the tongue and throat, accompanied by an 'alarming illness.'—Miss ———. In this case, somewhat similar effects follow the taking of honey of any kind, and especially honeycomb, into the stomach—viz., swelling of the tongue, frothing of the mouth, and blueness of the fingers. The following is an extract of a note received by me from a lady, who says: 'I had on three mustard plasters—one on the throat, one on the back of the neck, and another under the left shoulder. They remained on half an hour. Cotton wool was applied on their removal. About thirty hours afterwards, a painful stinging sensation commenced in the back of tho neck, followed by violent twitching of the muscles of the face, arms, and legs, which continued in regular succession through the whole night. It yielded, after about twelve hours, to hot fomentations of poppy-heads, applied to the back of the neck.' It cannot be ascertained that any medicine containing strychnia was taken.—A gentleman, member of the medical profession, with whom I am well acquainted, suffers from nettle-rash after eating veal.—Veal has the reputation of being particularly indigestible. The above instance of the production of urticaria from its use, is, doubtless, not an uncommon one.—I have been informed of a lady who cannot remain in a room in which there is a cat. Although the cat may be concealed, the lady's peculiar sensations immediately declare to her the presence of the animal.—A patient under my care, since dead of cancer, was invariably thrown into a state of nervous excitement by the exhibition of the compound infusion of orange peel.—A personal friend of my own suffered from erythema nodosum after eating shrimps, although these were perfectly fresh. I believe shell-fish generally is particularly liable to excite unpleasant consequences."

DISCOVERY OF THE ANÆSTHETIC EFFECTS OF CHLOROFORM.

A writer in the Household Words gives the following description of the discovery of the powerful anæsthetic properties of chloroform:

To Professor Simpson, of Edinburgh, belongs the distinguished credit of introducing chloroform, which has nearly superseded all other anæsthetics. Possessed with the notion that something better than ether existed in the chemical world, the professor set about deliberately to examine any volatile substance which afforded a promise of revealing the required properties. Various gases and liquids were experimented upon; and at last chloroform—a ponderous liquid which provoked no great expectations; and only known as a chemical curiosity in the laboratory—was brought to the trial. Doctor Simpson, with his two assistants, sat down late one night, after an arduous day's toil, and when most physicians, as well as patients, were wrapt in sleep, began to inhale various substances which had been collected. A small bottle of chloroform had been raked up out of some obscure corner, and was to take its turn with the rest. Each experimenter provided himself with a tumbler or finger-glass, a portion of each selected fluid was poured into the bottom of it, and the glass was placed over warm water to favor evolution of vapor. Holding the mouth and nostrils over the vessel, these votaries of science courageously explored this *terra incognita* by inhaling one vapor after another. At last each charged his tumbler from the small bottle of chloroform, "when immediately," says Professor Miller, "an unwonted hilarity seized the party; they became bright-eyed and very happy, and conversed with such intelligence, as more than usually charmed other listeners who were not taking part in the proceedings. But suddenly there was a talk of sounds being heard like those of a cotton-mill, louder and louder; a moment more, then all was quiet, and then—crash.

"On awaking, Doctor Simpson's first perception was men-

tal: 'This is far stronger and better than ether,' he said to himself. His second was to note that he was prostrate on the floor, and that his friends were confused and alarmed. Hearing a noise, he turned round, and saw his assistant, Doctor Duncan, beneath a chair, his jaw dropped, his eyes staring and his head half bent under him; quite unconscious, and snoring in a determined and alarming manner. More noise still, and much motion, and then his eyes overtook Doctor Keith's feet and legs making valorous efforts to overturn the table, or more probably to annihilate every thing that was on it.

"All speedily regained their senses, and from that day, or rather from the middle of that night, dates the discovery of the marvellous properties of chloroform."

APPOINTMENT AND RESIGNATION OF MEDICAL PROFESSORS.

Dr. George B. Wood has resigned the posts he has filled for many years as Professor of Theory and Practice in the University of Pennsylvania, to take effect at the close of the next lecture term, and of Physician to the Pennsylvania Hospital, where Dr. F. G. Smith has already succeeded him.

The Faculty of the Medical Department of Pennsylvania College have resigned simultaneously, and the Faculty of the Philadelphia Medical College have been elected to occupy their places.

Drs. C. R. Gilman and B. F. Barker have resigned their respective chairs as Professors of Obstetrics and Diseases of Women and Children, the former in the College of Physicians and Surgeons, and the latter in the New York Medical College. Dr. Geo. T. Elliott, jr., has been appointed to the vacancy occasioned by Prof. Gilman's resignation, and Dr. E. R. Peaslee to fill Prof. Barker's chair.

Dr. Markoe, clinical lecturer in New York Hospital, has been appointed Adjunct Professor of Surgery with Prof. Willard Parker.

Dr. Austin Flint, jr., the able editor of the Buffalo Medical Journal, has been appointed to the chair of Physiology and Microscopic Anatomy in the University of Buffalo, and Dr. S. Eastman to the chair of Anatomy.

Prof. J. G. Howard has retired from the chair of Anatomy in Savannah Medical College, and Dr. W. R. Warring has been appointed his successor.

Dr. L. P. Yandell.—This gentleman has tendered his resignation of the chair of Physiology in the University of Louisville, and has accepted a similar position in the Medical School of Memphis, Tenn.

VALUABLE SURGICAL DISCOVERY.

PARIS, July 28, 1859.

A medical discovery of much value, destined to effect a great amelioration in the treatment of ulcers, abscesses, flesh wounds, &c., has lately been made by two former *internes* or house surgeons of the Hospice de la Charite, and by them generously offered to the world without fee or reward. At the last sitting of the Academy of Science, M. Velpeau demanded permission to make an important communication, and announced that the two young practitioners in question, Messrs. Crome and Demeaux, had paid him a visit for the purpose of presenting to his notice their discovery and explaining to him its results. Messrs. Crome and Demeaux have found a process for the complete and instantaneous disinfection of animal matter. The action of the disinfecting agent arrests the progress of decomposition, and effectually prevents the generation of insects. The substance, prepared for use, costs here about one franc for a hundred pounds, and the expense in America would probably be still less. The following is the formula, as given by the inventors themselves:

Plaster of commerce, reduced to a fine powder, 100 parts; coal tar, one to three parts. The mixture of the two sub-

stances is effected with ease by the aid of a mortar, or by any other appropriate mechanical means. The application of this composition to the dressing of sores or wounds requires a particular preparation. A certain quantity of the powder, prepared according to the formula, is diluted with olive oil to the consistency of a paste or ointment. This species of paste or salve is of a dark brown color, has a slightly bituminous odor, and may be kept in a closed jar for an indefinite period. The oil unites the powder without dissolving it, and the composition has the property of absorbing infectious liquids the instant it is applied to the sore which produces them. The application may be mediate or immediate. In the latter case, that is to say, placing the composition directly in contact with the sore, no pain whatever is produced; on the contrary, the salve has a detersive action, cleanses the sore and favors circulation.

In the course of his remarks, M. Velpeau mentioned the case of a patient at the Charite, to whom the new process had been applied, with perfect success. This person was afflicted with a frightful abscess in the thigh, from which exuded a purulent matter of a most infectious odor, rendering the operation of the surgeon both painful and difficult. This matter mixed with a powder held in readiness by the two experimentalists, was disinfected in one minute, touched with impunity by the spectators, and applied beneath their noses, without leaving a trace of unpleasant odor.

As has been seen, the elements of this composition are of the simplest character, and though intelligence of the discovery could not have reached the medical faculty of the United States in advance of this letter, your own surgeons will doubtless receive, by the same mail which carries this, every corroborating particular. My desire is to make known the event throughout our country, and I sincerely hope this paragraph may be widely copied by your exchanges. As M. Velpeau himself observed at the close of his observations before the Academy, too much publicity cannot be given to so valuable a discovery, as well as the disinterestedness of its authors. In their own report, Messrs. Crome and Demeaux

state that the composition may be applied in the form of a poultice, or on cotton, and laid on the wound. They demonstrate that their mode of dressing possesses the double property of disinfecting morbid products and of absorbing their liquids. This last circumstance entirely obviates the necessity of lint—which is one of the most important features of the discovery.—*N. Y. Express.*

SPIRIT RAPPING.

At the meeting of the Imperial Academy of Science on the 18th April, M. Jobert de Lamballe read a very curious paper, the subject of which is highly interesting both to the physiologist and the pathologist. Some time ago this distinguished surgeon was called upon to visit a young girl about fourteen years of age, who for six years had been suffering from involuntary movements of the peroneus brevis muscle of the right leg. The cause of this peculiar affection could not at the time be accounted for; the movements were characterized by pulsations or knocks, presenting all the regularity of a pulse; each pulsation, or rather knock, being distinctly heard at some distance as proceeding from behind the external malleolus. A similar affection, in a less intense degree, declared itself shortly after at the same point in the left leg. Pain, hesitation in walking, and a tendency to fall were some of the effects attendant on the malady. On extending the foot, and on applying pressure to certain points along the course of the muscle, the patient could arrest the pulsations; but these manœuvres invariably produced pain and fatigue in the limb. The parents of the girl had begun to entertain the notion that the peculiar sound or knocks which proceeded from the limb were the result of some supernatural agency, and it was only after a careful and scientific investigation, conducted by M. Jobert, that their minds were disabused of this idea. He found that the peculiar sounds were produced solely by the rising and falling of the tendon of the peroneus

brevis muscle during the action of the latter, and at the part where the tendon passes along its osseous groove. The involuntary character of the movements, he attributes to some peculiar functional trouble of the muscular fibres, or of the nerves supplying them. Although in the case in question the pulsations or knocks were altogether independent of the volition of the individual, still he believed it possible that by a little practice they could be produced at will, and that it is to the possession of this peculiar power that the entire secret of mediums and spirit-rappers is due. He moreover stated that the muscles and tendons, both of the shoulder and of the leg, are equally capable of producing these peculiar sounds; and he related the case of a lady who could give rise to them at the hip-joint by assuming a particular position. Certain conjurers had been known who could even produce a kind of harmony by a succession of knocks, thereby imitating the tune of a dance, or a military march. M. Jobert terminated the history of this interesting case with a few remarks on the treatment which he adopted. The medical man in attendance, and at whose request he had been called to see the girl, had failed to do her any good, although he had employed a great variety of remedies, such as leeching, blistering, continued pressure over the part, etc., together with the internal use of mineral waters. M. Jobert proceeded as follows: By means of the subcutaneous section he completely cut across the body of the peroneus brevis of both legs, and afterwards, by means of a suitable apparatus, put up the limbs so as to secure their perfect immobility. On reunion taking place the girl recovered the complete use of both members, and no trace of the affection has since appeared. The cure was complete. On continuing his remarks on this case, M. Jobert observed that a German physiologist, M. Schiff, whose attention had been attracted to this curious subject, made a discovery some three or four years ago, as to the seat and origin of these peculiar sounds (which are so commonly attributed to supernatural intervention) almost identical in result with the conclusions he himself had arrived at. Remarking that the sounds proceeded invariably from the foot of the bed of individuals who pretended to be influenced by spirits, M.

Schiff began to entertain serious doubts of their supernatural origin, and was not slow in concluding that they were the result of natural causes existing in the body itself. His knowledge of anatomy led him to think that the seat of these sounds might be the peroneal region, where there exist a bony surface and two tendons running in a common groove. A series of experiments made on himself confirmed him in this belief: he found that the exact seat of the sounds was behind the external malleolus, and in the bony canal along which pass the tendons of the peronei muscles. By practice he was enabled, in whatever position he placed himself to imitate all the tricks and prodigies of spirit-rappers—in short, he established the fact that these peculiar noises originate in the tendon of the *long* peroneus muscle, and that they are depedent on a diminution in the thickness of the sheath, or in the total absence of the sheath in this muscle. M. Jobert, while agreeing with M. Schiff as to the seat of these sounds, differs with him, as we have already seen, as regards the particular muscle producing them. The one maintains that it is the peroneus brevis, the other is of opinion that it is the peroneus longus which is mainly concerned in their production. There is also another point on which they differ, viz: that while the sounds or knocks observed by Schiff were purely physiological, and due to movements dependent on the will; in the case of M. Jobert's patient they were the result of movements of an involuntary nature, painful, and consequently morbid. Altogether the subject is one of much interest, and well meriting a further investigation on the part of anatomists and physiologists. By enlightening the world on the purely physical character of these sounds much may be accomplished in the way of dispelling the absurd superstition connected with spirit rapping.—*Med. Times and Gaz..*, May 14, 1859.

MEDICAL JOURNALS.

In the past few months some of our familiar exchanges have undergone important changes, some altered their places

of publication and others been altogether suspended, in every instance save one, from want of sufficient support, and from shameful negligence on the part of their subscribers to pay the honest dues of Editorial toil. Our profession should from very shame look to it, that this latter event occur no more; since it necessarily brings reproach upon the good faith—to say the least—of all its members.

Dr. Austin Flint, jr., has removed to New York city. He still retains the Editorship of the Buffalo Medical and Surgical Journal, whose second number of Volume XV., bears the lengthened title of The New York Monthly Review of Medical and Surgical Science, and Buffalo Medical Journal.

The Savannah Journal of Medicine, is now conducted by Prof. Juriah Harriss, as Senior Editor; Dr. J. S. Sullivan having resigned that position, held by him for the last two years. In making the announcement, the present Editor remarks, "Our association as Junior Editor with him, has been most cordial and friendly, indeed such intercourse as a gentleman of high tone and intelligence, will ever guarantee to others in the Profession." Prof. R. D. Arnold will continue as Associate Editor of the Journal, while Dr. Sullivan is promised, as still a contributor to its pages.

The Nashville Monthly Record of Medical and Physical Science, has also undergone a change in a part of its editorial corps. Dr. Richard O. Currey has resigned his position of Associate Editor, while Prof. Daniel F. Wright will be hereafter assisted by Professors John H. Callender and Thomas L. Maddin, of the Shelby Medical College.

The Chicago Medical Journal has, during the past year, undergone several changes. Its January and February numbers present the names of Drs. N. S. Davis and W. H. Byford, both of the Rush Medical College. In the March number, we find that the Journal is again conducted by its former able Editor, Professor Daniel Brainard, Professor of Surgery in Rush Medical College, and the same issue informs us of the resignation of Professor Davis and Byford, not only from the Journal, but in company with Professor Johnson, from their places in the Faculty also. The June number is impress-

ed with the names of Dr. Edward Powell, Demonstrator of Anatomy in Rush Medical College, as assistant to Dr. Brainard. While we deeply regret the loss of Drs. Davis and Byford, we cordially welcome Dr. Brainard and Dr. Powell, into the ranks of our fraternity.

The Medical Chronicle of Montreal, the only Medical Journal of the Canadas, has been abandoned by its able and heretofore indefatigable Editors, Drs Wright and McCallum, for the want of support.

The Maine Medical and Surgical Reporter. This Journal begun in March of the present year, under the Editorial management and proprietorship of Drs. W. R. Richardson and R. W. Cummings.

The Philadelphia Medical and Surgical Journal, and The Louisville Medical Gazette, are each suspended, very properly and justly disheartened, that faithful, long and arduous labors, brought no adequate reward.

EASY METHOD OF EXTRACTING FOREIGN BODIES FROM THE EYELIDS.

Dr. Leon Renard, in a note to the Editor of the Union Medicale, describes the following method of extracting small substances which have become lodged in the groove formed by the reflection of the conjunctiva from the upper lid to the sclerotic, and which often cannot be seen, even when the lid is inverted. The lid being seized at its angles between the thumb and forefinger of each hand, is to be gently drawn forward and downward, as far as possible, over the lower lid, and retained there for about a minute. On allowing the upper lid to return to its normal position, the flow of tears will carry off the foreign body, which will usually be found on the lower lid, or one of the lashes, or on the cheek. The writer states that he has often found this simple method of the greatest utility and convenience.

CORRESPONDENCE FROM THE SEAT OF WAR IN ITALY.

BY J. J. CHISOLM,

Professor of Surgery in the Medical College of South Carolina.

MILAN, July 21st, 1859.

The Hospitals in Milan, which number twenty-four, and all of which are filled with wounded from the bloody battles of Magenta and Solferino, offer a rare opportunity for the study of gun-shot wounds and also, for the comparison of the various modes of treatment, instituted by civil and military surgeons. All of these buildings are not equally ventilated; many of them were built for Hospitals and in their time were considered patterns of such institutions. The majority have been pressed into service from the force of circumstances. The facilities for transportation by rail road are so great, that the wounded, taken from the field, were immediately sent to the neighbouring villages and from thence to the large cities, where the conveniences for treatment could be more readily obtained. Milan of course came in for a very large share, and in a few hours all of its hospital accommodations had been disposed of, Convents and Barracks were soon teeming with sufferers; friend and foe mingling their cries of agony and expressions of sympathy, so suddenly had their condition changed their previous thirst for blood. The medical profession of the city at once volunteered their assistance, and the wounded without delay received surgical aid. At this late day, one month and even more since the wounds were received, much of interest connected with the active treatment has been lost, and the relative importance of the same can no longer be traced, but still the full wards offer so much material for instruction, that our visit was extremely satisfactory.

We first visited the great Civil Hospital, built in due oblong form, and containing a series of open courts. The building possessed three thousand beds. Many of the wards

were very long and the number of the wounded necessitated two, three and even four rows of beds, with a very narrow passage between them. Notwithstanding this crowded condition, no offensive odor could be preceived from the suppurating wounds. Cleanliness pervaded the entire establishment. The appearance of the patients was bright and indicated a progressive convalescence. This Hospital was filled with French soldiers, who bore their suffering well and were chiefly annoyed by their compulsory inactivity. The bright appearance of the wards was partly attributed to the fact that the serious cases had been thinned out and their places supplied by those of a milder character; still many very serious ones remained, as the recital of individual cases will show. The wounds were nearly all healthy; a few were cleaning off after an attack of acute gangrene, which at one time had been very rife in the Hospital and to which a great many of the amputated had succumbed. Even now, with rare exceptions it was in the stumps that the ravages of gangrene were seen. Under this combination nearly all the amputations of the thigh were fatal. Those of the upper extremity were more successful, although equally attacked by gangrene as the exposed bones indicated. In one of the Hospitals which we visited, formerly a barrack, with rooms accommodating about ten beds, surrounded by long and spacious corridors, hospital gangrene was epidemic. In this institution were now collected one thousand wounded Austrians, whose worn frames and gaunt visages indicated a fearful combat with disease. They had been led to believe that if taken prisoners they would as surely be put to death. Therefore all who had the strength to crawl along after they were wounded had hid themselves in the ditches or among the thick undergrowth; many remaining on the battle field two and even three days, living upon ditch water if they were so fortunate as to be in its neighbourhood. When you take into consideration their moral dejection, we may nearly say annihilation; their lymphatic tendencies; their irregularity of living, for scarcity was the common condition of the Austrian camp, and before the battle of Magenta it was reported that the Austrians had

not tasted food for fifty hours, and then their wounds much more grave than those of the French, as they were made by a much larger ball;* and add to all this the mistaken abstemious diet which was pursued in this hospital, you may understand readily the condition in which we found these unfortunates. The physician with whom we made the visit, believing in the contagiousness of the disease, had isolated these cases in separate wards and some of them were fearful; thighs transfixed, in the depth of which, you could thrust your fist, &c. He stated to us that he had seen patient after patient attacked who were lying in contiguous beds to a case of gangrene, thus clearly proving to him its infectious character, the tendency to attack diminishing with the space of interval. Yet, I saw this physician thrust his dirty unwashed finger, which one minute before had been bathed in the pus of gangrene from one of the worst cases I ever saw, directly into a healthy wound, to show us the position which the ball had taken. I shuddered at this utter recklessness and did not wonder at the fearful mortality which had befallen his practice. In the better regulated hospitals and especially among the victorious troops, many gun-shot wounds healed with surprising facility and comparatively few were accompanied by much inflammatory re-action. The majority of these now chronic wounds had no induration around them, and a few appeared to have healed by quick union. Most of the wounds were in the extremities and very few of the trunk; the majority of the latter had been left on the field of battle. Most of the cases which had survived the day, had been lost before our visit. Judging from present appearances the lower extremity was more frequently wounded than the upper. In the greater number, the ball had transfixed the limb and escaped, so that very rarely was it necessary to remove a foreign body. I saw but two cases in which a ball had been removed by a counter opening. It appeared

* The rifle of the French chasseurs carries a conical ball of over two oz. weight.

to me that the conical ball which is now coming into universal use, not only moves with much greater velocity, but cuts its way to a greater extent than did the round ball. The wounds made by them have not that mangled appearance, nor are they so tortuous, with eccentric courses. Perhaps we may account in this way for the mild degree of inflammatory reaction and absence of general excitement, which was noted in so many cases. Even where fingers had been amputated by shot, the stumps looked well, the interval of a month no doubt materially assisting. From the same causes secondary hemorrhage was rare.

It was very interesting to examine the course of balls through the most intricate portions of the body, the neck, the axilla, the groin, in the very midst of the most important blood vessels and nerves, without leaving any permanent injury. The accounts of the soldiers were, that they bled profusely, losing quarts of blood, and were picked up exhausted, yet these injured vessels did not require ligatures, nor did secondary hæmorrhage occur. These wounds of arteries must have healed with great facility. In many instances the entrance and exit of the ball was such, as to warrant the belief that the main arteries could not have escaped; yet after the first profuse hæmorrhage, the wounds would heal without further trouble. I saw an aneurism of the popliteal vessel, produced by a ball, operated upon very unscientifically by the head surgeon of this great hospital. After the incision through the skin, for which alone he used the knife, he continued and completed the dissection with a scissors, making necessarily a most tedious operation; Scarpa's triangle was selected as the seat of ligature. In the early treatment of all the wounded, cold water had been freely used, and appears to have been the general plan pursued. When suppuration had set in, this treatment gave way to forcing beds of lint, which was the universal practice. This bedding was continued until suppuration ceased, and the wound cicatrized. Nimia diligentia was every where conspicuous. In many of the hospitals two and even three dressings were daily made, the wounds being thoroughly washed, as if ex-

cessive cleanliness was the *sine qua non.* The wound was subjected to this scouring from within as well as without, and I heard one of the surgeons exclaim with evident satisfaction, when the water injected into a wound came away as clean as when thrown in. I saw some wounds which were so far healed that the new cuticle had actually formed over it, under a thin scab. This was torn off so as to give the favorite charpie some thing to do. Whilst all this covering up was going on two and three times a day, the surgeons were complaining of the hot summer weather, with its injurious effects upon the wounded. How consolatory is the thought that nature has endowed us with strong constitutions, so that we may have enough and to spare; otherwise woe to many. Several cases showed very forcibly the pertinacity of human life. They were chiefly chest wounds. Not those in which the ball in striking a rib had followed the circumference of the chest, so as to appear serious to a trivial examination. Many examples of such were in the wards. I refer to those in which the chest had been completely transfixed with its contained lung. In one of these cases, the ball had entered at the posterior edge of the scapula, midway between the lower angle and the spine, and had, after transfixing the chest, escaped behind the anterior border of the axilla, through the outer edge of the great pectoral muscle. The patient had spit up largely of blood, which continued for some days, but had now altogether disappeared. The posterior wound was still discharging—the anterior had healed. By pressure no pus escaped; but during the act of coughing, the diaphragm, assisted by the thoracic muscles, pumped out ounces of frothy pus from the pleural cavity. He was very much reduced by this drain which had been going on for three weeks, but had strength enough to enjoy his pipe, which we saw him using as we returned by his bed. In another case, the ball had perforated the left side of the chest, on a level with the third rib, and about two and a half inches from the middle of the sternum. Its exit was at the lower portion of the shoulder blade. This patient had had hæmoptisis for several days, followed by

violent inflammation of the lung and coverings. By repeated blood lettings, the hæmorrhage as well as the inflammation had been controlled, and the patient was ready to be discharged. How the large blood vessels at the root of the lungs escaped injury, is an enigma. Would not transparency of the tissues develop curious facts in pathological anatomy? Thoracic wounds were comparatively common, whilst very few intestinal wounds existed in the hospital. Most of these had terminated fatally, either upon the reception of the injury, or very soon afterwards. One case of great interest was shown, in which a ball had obliquely entered the abdomen just above the symphisis pubis, escaping near the anterior superior spine of the ilium. The iliac wound had healed quickly. On the second day, after the receipt of injury, some coffee which he had taken, escaped from the pubic wound; this is his account of himself, and from that time his food has continued to discharge from the opening. In examining the parts, the intestine which appears to have the lining of the upper portion of ilium, has become intimately adherent to the skin, its mucous surface protruding above the general level. At times it exhibits a lively vermicular motion, an inodorous, colored discharge, consisting of digested food escaping freely at such periods. This case with others which have occurred, would show either the paralyzing effect of injury upon the motions of the intestine, or the very limited motion which normally exists in the same; otherwise how can we explain the rarity of fœcal extravasation in intestinal wounds? It is proposed to reestablish in this case the continuity of the intestine, by destroying the septum when the external orifice will close spontaneously. In a case of interest, the entire body of the lower jaw had been crushed by a cannon shot, which carried away the chin. A large orifice was formed through which the entire lower jaw with the exception of the rami, escaped in broken fragments. The tongue hardling, lost its support, and protruded. The wound, two inches in diameter, now looked healthy, and was gradually contracting. The tongue was retained in the mouth by pledgets of

lint. With the exception of the frightful deformity, for which there will be no remedy, the patient will do well. In another case the ball had perforated the root of the neck in the posterior triangle, grazing the external border of the sterno—cleido mastoid muscle, having its exit on the back; profuse hæmorrhage followed the injury. Now after an interval of four weeks, the wounds having perfectly healed, paralysis of the arm exists, with the exception of those fingers supplied by the ulnar nerve, which retain perfectly their sensibility. One soldier was shot in the face, in such a way that the ball entered at one malar, and escaped through the other, passing across the floor of the orbit. The pupils of both eyes were dilated, and vision much impaired; the wounds had perfectly healed.

But few cases of tetanus had occurred. I saw one convalescent, in which the muscles were still rigid. This person had been shot through the calf, the ball in its passage fracturing the tibia. From a large opening, pus was freely discharging. In this case I saw the head surgeon, who had used the scissors so freely in the morning, further dilate the wound, and for fifteen minutes make continued efforts to remove a piece of dead bone which was not yet detached, and which was embedded finger deep in the calf. It would be a curious instance, if this excessive additional pain which the tetanic patient was compelled to endure, without the soothing aid of chloroform, quiets the nervous excitement under which he was laboring.

The ladies of Milan are constantly engaged at the hospitals, supplying and manufacturing lint and dressings for the use of the wounded. Not only do they liberally bestow comforts, but give their individual assistance. Many of the wounded were taken to their dwellings, where every attention was lavished upon them. I saw several of these, during convalescence, riding out with their refined hostesses, a striking contrast between the rough soldier of the ranks, and the polished courtisane. Their unremitting kindnesses soothed many a pang, and gave that solace which is the peculiar prerogative of women.

SO. CA. MEDICAL ASSOCIATION PRIZE.

At a meeting of the Executive Committee of the South Carolina Medical Association, held August 17th, the following resolution was adopted:

Resolved, That with the view of promoting the interest in its meetings, and increasing the value of the essays presented, a prize of one hundred dollars be offered by the Association for the best essay on a subject in any one of the departments of medical science.

The essays must be handed in to the Corresponding Secretary of the Association, in Charleston, by the 1st of January next, 1860, in order that sufficient time be given the Examining Committee to report at the annual meeting to take place in Charleston, in February next.

A motto to be written both on the essay competing for the prize, and on an accompanying sealed note, as usual in such cases.

All essays thus sent to become the property of the Association.

J. P. CHAZAL, Corresponding Secretary.

SANITARY CONDITION OF CHARLESTON, S. C.

Our city has enjoyed such a season of perfect and uninterrupted health this summer, as, within there collection of our oldest practitioners, has no parallel. Not only have we been spared any epidemic visitation, but not even a single case of Yellow Fever has been brought into our harbor. Moreover, the mortality from all causes has been unprecedently low, and as the summer is now far advanced, and our city in a tolerably satisfactory condition, as regards all the supposed agents or adjuvants of pestilential disease, we may reasonably hope, with the blessing of God, to preserve this happy condition.

RETURN OF DEATHS IN THE CITY OF CHARLESTON
FOR JUNE, 1859.

WHITES.	
Accident,	1
Apoplexy,	1
Ascitis,	1
Brain, Congestion of	2
Bronchitis,	2
Cholera Infantum,	3
Consumption,	6
Croup,	1
Croup, Membranous	1
Debility,	1
Drowned,	1
Dysentery,	1
Fever, Typhoid	1
Fever, Scarlet	1
Gastro Enteritis,	1
Hydrocephalus,	1
Intemperance,	2
Liver, Disease of	1
Lungs, Congestion of	2
Marasmus,	3
Old Age,	2
Pericarditis,	1
Peritonitis,	1
Teething,	3
Trismus Nascentium,	1
Uterus, Ulcer of	1
Males 29; Females 13. Total,	42
AGES.—Under 1 year,	5
1 to 5,	10
5 to 10,	2
10 to 20,	2
20 to 30,	5
30 to 40,	1
40 to 50,	7
50 to 60,	6
60 to 70,	1
70 to 80,	1
80 to 90,	1
90 to 100,	1
Total,	42
Natives of Charleston,	23
Natives of United States,	5
Foreigners,	14
Total,	42

BLACKS AND COLORED.	
Abscess of Liver,	1
Anæmia,	1
Apoplexy,	2
Brain, Disease of	1
Catarrh,	1
Caries of Vertebra,	1
Cholera Infantum,	2
Cholera Morbus,	1
Convulsions,	3
Consumption,	1
Delirium Tremens,	1
Diarrhœa,	1
Dropsy,	5
Drowned,	1
Enteritis,	1
Epilepsy,	1
Fever, Typhoid	1
Fever, Catarrhal	1
Fever, Remittent	1
Fever, Worm	1
Fever, Scarlet	1
Hydrothorax,	1
Liver, Congestion of	1
Lungs, Congestion of	1
Marasmus,	5
Old Age,	6
Peritonitis,	1
Rectum, Disease of	1
Sore Throat, Membranous	1
Teething,	2
Trismus Nascentium,	4
Unknown,	1
Want of Vitality,	3
Males, 30; Females, 26. Total,	56
AGES.—Under 1 year,	22
1 to 5,	8
5 to 10,	2
10 to 20,	3
20 to 30,	2
30 to 40,	2
40 to 50,	5
50 to 60,	4
60 to 70,	1
70 to 80,	4
80 to 90,	2
90 to 100,	1
Total,	56
Natives of Charleston,	49
Natives of United States,	7
Total,	56

J. L. DAWSON, M.D., *City Registrar.*

RETURN OF DEATHS IN THE CITY OF CHARLESTON
FOR JULY, 1859.

WHITES.		BLACKS AND COLORED.	
Birth, Premature	2	Accident,	1
Burn,	1	Anasarca,	1
Cholera Infantum,	5	Bowels, Spasms of	1
Convulsions,	3	Brain, Congestion of	1
Consumption,	2	Cancer of Rectum,	1
Coup de Soleil,	2	Catarrh,	1
Cystitis,	1	Cholera Infantum,	4
Debility,	1	Convulsions,	4
Diarrhœa,	3	Consumption,	8
Drowned,	1	Coup de Soliel,	1
Dysentery,	1	Croup, Membranous	1
Fever, Typhus	1	Diarrhœa,	2
Fever, Intermittent	2	Dropsy,	2
Fever, Congestive	2	Enteritis,	2
Hydrocephalus,	2	Fever, Continued	2
Hydrothorax,	1	Fever, Typhus	1
Intemperance,	1	Fever, Congestive	1
Lungs, Congestion of	1	Gastro Enteritis,	1
Marasmus,	2	Heart, Disease of	1
Spasms,	1	Hepatitis,	1
Teething,	6	Hydrocephalus,	1
Trismus Nascentium,	4	Hydrothorax,	1
Want of Vitality,	2	Lungs, Congestion of	1
		Marasmus,	4
Males, 33; Females, 14; Total,	47	Old Age,	6
		Sore Throat, Membranous	1
		Teething,	9
		Thrush,	1
		Trismus Nascentium,	7
		Males, 31; Females, 37; Total,	68
		Ages.—Under 1 year,	21
		1 to 5,	14
Ages.—Under 1 year,	18	5 to 10,	1
1 to 5,	13	10 to 20,	3
5 to 10,	0	20 to 30,	8
10 to 20,	0	30 to 40,	2
20 to 30,	3	40 to 50,	3
30 to 40,	6	50 to 60,	4
40 to 50,	4	60 to 70,	5
50 to 60,	0	70 to 80,	1
60 to 70,	1	80 to 90,	1
70 to 80,	2	90 to 100,	5
Total,	47	Total,	68
Natives of Charleston,	33	Natives of Charleston,	57
Natives of United States,	5	Natives of United States,	10
Foreigners,	9	Foreigners,	1
Total,	47	Total,	68

J. L. DAWSON, M.D., *City Registrar.*

METEOROLOGICAL ABSTRACT FOR JUNE, 1859.

BAROMETER.

Maximum, 30.440, - - Minimum, 29.960, - - - Range, 00.480

THERMOMETER ATTACHED.

Mean.

7 A. M., 76.63, - - - 2 P. M., 80 43, - - - 9 P. M., 78.40

THERMOMETER DETACHED.

Mean.

7 A. M., 74.43 - - - 2 P. M., 82.13, - - - 9 P M., 76.60

Highest Degree.

7 A. M., 80, - - - 2 P. M., 88. - - - - 9 P. M., 81

Lowest Degree.

7 A. M., 62, - - - 2 P. M., 72, - - - - 9 P. M., 67

REGISTER THERMOMETER.

Mean.

Night, 72.60, - - - - - - - - - - - Day, 85.83

DEW POINT.

Mean, - - - - 72.53

WINDS.

Sun Rise—NE 7 days; SE 9; SW 11; E 2; W 1. Prevailing SW.
4 *P. M.*—NE 4 days; NW 1; S 1; SE 8; SW 11; E 4; W 1. Prevailing SW.

WEATHER.

Fair, 13 days—Cloudy, 5 days—Rain, 12 days

QUANTITY OF RAIN.

Inches, - - - 6 44.

JOHN L. DAWSON, M.D.

METEOROLOGICAL ABSTRACT FOR JULY, 1859.

BAROMETER.

Maximum, 30.452, - Minimum, 30,012, - - - Range, 00.440

THERMOMETER ATTACHED.

Mean.

7 A. M., 78.09, - - 2 P. M., 83.32, - - - 9 P. M., 80.61

THERMOMETER DETACHED.

Mean.

7 A. M., 75.51, - - 2 P. M., 85.00, - - - 9 P. M., 79.22

Highest Degree.

7 A. M., 82, - - - 2 P. M., 94, - - - - 9 P. M., 86

Lowest Degree.

7 A. M., 64, - - - 2 P. M., 71, - - - 9 P. M., 68

REGISTER THERMOMETER.

Mean.

Night, 73.93, - - - - - - - - - - - Day, 86.09

DEW POINT.

Mean, - - - [illegible]

WINDS.

Sun Rise—NE 9 days; [illegible]
4 *P. M.*—NE 5 days; [illegible]

TO CORRESPONDENTS AND PUBLISHERS.

Communications have been received from Drs S. W. Mitchell, W. A Hammond, W. M. Cornell and T. P. Bailey, which will appear in our next.

The following Books and Pamphlets have been received:

Gray's Human Anatomy; Blanchard & Lea, Philadelphia.

Althaus' Medical Electricity; Trubner & Co., London.

Headland on the Action of Medicine; Lindsay & Blakiston, Philadelphia.

Contributions to Midwifery, with a Report on the Progress of Obstetrics, &c., &c., for 1858. By Drs. Noeggerath and Jacobi, Bailliere Brothers, N. Y.

Reeves' Treatise on Enteric Fever; Lippincott & Co., Philadelphia.

Sixteenth Legislation Report of Massachusetts.

Medical Communications of the Massachusetts Medical Society.

Ayre's Memoir on the Treatment of Epidemic Cholera.

Wynne on the Importance of the Study of Legal Medicine.

Churchill on the Cause and Treatment of Tubercle.

Proceedings of the 67th Convention of the Connecticut Medical Society.

Meigs, J. A., on the Methods of Studying and Teaching Physiology.

An account of a Secret Society in New-York entitled the Kappa Lambda.

Galt's Lecture on Idiocy.

Hutchison's Inaugural Address at the Brooklyn City Hospital.

Hamilton's Address on Hygiene.

Bell's Address on Medical Heroism.

Logan's Introductory at the Atlanta Medical College.

Pennsylvania College--Medical Department.

NORTH STREET, BELOW LOCUST, PHILADELPHIA.

Session of 1859-60.—Faculty.

B. Howard Rand, M.D., Professor of Chemistry.
Henry Hartshorne, M.D., Professor of Practice of Medicine.
Lewis D. Harlow, M.D., Professor of Obstetrics, &c.
William S. Halsey, M.D., Professor of Surgery.
Wm. Hembel Taggart, M.D., Professor of Materia Medica.
James Aitken Meigs, M.D., Professor of Institutes of Medicine.
Wm. H. Gobrecht, M.D., Professor of Anatomy.
Theodore A. Demme, M.D., Demonstrator of Anatomy.

The Session of 1859-60 will commence on Monday, 10th of October, and continue, without intermission, until the 1st of March. The commencement for conferring degrees will take place early in March, causing as little detention of the Graduating Class, after the close of the Lectures, as possible.

There will also be an examination of candidates for graduation, on the 1st of July; the degree in such cases being conferred at the ensuing commencement in March.

The rooms for Practical Anatomy will be open early in September.

The College Clinic will be conducted on every Wednesday and Saturday throughout the session.

The Register of Matriculants will be open in the College Building, early in September. The Janitor will always be present at the College, to give every necessary assistance and information (as regards board, &c.) to students, on arrival in this city.

Matriculation (paid once only), $5; For each Professor's ticket, $15; Dean, $10.

LEWIS D. HARLOW, M.D., Dean,
No. 1023 Vine, below 11th Street.

MEDICAL COLLEGE OF VIRGINIA.

AT RICHMOND.

SESSION OF 1859-60.

The regular course of Lectures will commence on Monday, the 3d of October, and continue until the 1st of March.

Charles Bell Gibson, M.D., Professor of Surgery and Surgical Anatomy.
David H. Tucker, M.D., Professor of Theory and Practice of Medicine.
Beverley R. Wellford, M.D., Professor of Materia Medica and Therapeutics
Arthur E. Peticolas, M.D.. Professor of Anatomy.
Levin S. Joynes, M.D., Professor of Institutes of Medicine and Medical Jurisprudence.
James H. Conway, M.D, Professor of Obstetrics and Diseases of Women and Children.
James B. McCaw, M.D., Professor of Chemistry and Pharmacy.
Marion Howard, M.D., Demonstrator of Anatomy.

The study of Practical Anatomy may be prosecuted with the most ample facilities and at a very trifling expense.

The Infirmary, under the same roof with the College, is at all times well filled with Medical and Surgical cases, and furnishes peculiar facilities for Clinical instruction.

Many Surgical Operations are performed in presence of the class, and the Students being daily admitted to the wards, enjoy, under the guidance of the Professors, unusual opportunities for becoming familiar, not only with the symptoms and diagnosis of disease, but with its daily progress and treatment.

Students also enjoy the Clinical advantages afforded by the Richmond Almshouse.

FEES.

For the entire Course of Lectures, - -	$105 00
Demonstrator's Fee, - - -	10 00
Matriculation Fee, - - - -	5 00
Graduation Fee, - - -	25 00

A Prize of ONE HUNDRED DOLLARS, offered by Dr. Thomas D. Warren, of North-Carolina, will be awarded to the member of the graduating class who shall present to the Faculty the best Essay on any medical subject.

Board may be obtained in the city at prices ranging from $3.50 to $5 per week. The catalogue, containing fuller information concerning the Institution, will be forwarded to those who apply for it, or specific enquiries will be answered by letter.

L. S. JOYNES, M.D.,
Dean of the Faculty, Richmond, Va.

Course on the Diseases of the Heart & Lungs.

BY J. DA COSTA, M.D.

Philadelphia, 212 South Eleventh Street.

Dr. DA COSTA will commence his next practical course on the Diseases of the Lungs and Heart, on the 20th of October. The Clinical meetings are held twice a week. Numerous patients are introduced to familiarize the student with the physical signs of thoracic diseases.

Three courses are delivered annually. One commencing early in April, one in August, and one in October. Each course includes clinical demonstrations, and a series of lectures.

CHARLESTON

MEDICAL JOURNAL AND REVIEW.

VOL. XIV.] CHARLESTON, S. C., NOV., 1859. [No. 6

ART. I.—*An Experimental Examination of the Toxicological Effects of Sassy Bark, the Ordeal Poison of the Western Coast of Africa.* By S. WEIR MITCHELL, M.D., Lecturer on Physiology in the Philadelphia Medical Association, and WM. A. HAMMOND, M.D., Assistant Surgeon U. S. Army. Read before the Biological Department of the Academy of Natural Sciences, Philadelphia, June 29th, 1859.

The study of new and curious poisons, is one which has a two-fold value; firstly for the light which it sometimes cast upon obscure physiological questions, and secondly, because agents which in large doses are potent for evil, may, under proper management, become invaluable additions to the Materia Medica.

The substance which we are about to consider, seemed likely to prove interesting for the latter reason, and not less so from its association with the strange and barbarous custom of trial by ordeal, which prevails to so great an extent in Africa.

So far as we are aware, no proper toxicological examination of Sassy Bark has, hitherto, been made. Prof. Christison, in an interesting paper upon the ordeal bean of old Calabar, refers at some length to the Sassy* or Red Water

* On the Properties of the Ordeal Bean of Old Calabar, Western Africa. By Robert Christison, M.D., V.P.R.S.E., etc., read before the Royal Society

Ordeal, and regrets that he has not enough of it to make an examination of a subject so curious. We have, therefore, the highest authority for considering it as deserving the close investigation to which we have submitted its characters.

Sassy bark, red water bark, etc., for it is known under many names, is the bark of a tree belonging to the genus Erythrophleum* (Prof. Afzelius). In the Gard. Dict. vol. 11, p. 424, of Mr. George Don, it receives the specific name of E. Guineënse. It has been thought by Dr. Hooker and Mr. Bentham to be identical with the "Filda Suaveolens," described and figured by MM. Guillemin and Rerattet in their "Flore de Sénégambie," and well known as the ordeal poison of the native tribes on the river Gambia. It is improbable, according to Dr. Christison, whom we quote, that the two trees are identical, since E. Guineënse, has a purely astringent taste, and "F. Suaveolens" causes slowly an intense numbness and tingling on the tongue when tasted. Prof. Procter, who was not aware that E. Guineënse had received a specific name, described it under the name of E. Judiciale, in reference to the use to which the natives apply the bark.†

It is unnecessary to enter into detail regarding the botanical characters of this tree, since they have been fully treated of by Prof. Procter, with his usual skill and judgment.‡ The bark used by us was a portion of that which originally belonged to Prof. Procter, and which came from Cape Palmas, Liberia.

The term sassy bark is probably a corruption of Cassa bark, another species of the same genus, and also used in Congo for the trial by ordeal. It is proper to add, however,

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of Edinburgh, Feb. 5th, 1855—Pharm. J. & Tr. V. XIV. 1854–55, London, p. 470.

* From the red color of the bark.

† On Erythrophleum Judiciale. By Wm. Procter, Jr., A.M. J. of Pharm., July, 1852.

‡ See the Am. Jour. of Pharm., July, 1852, for the botanical statement, and Oct., 1851, for an elaborate chemical examination of the bark. By Prof. Procter.

that the word is sometimes spelled Saucy,* and that our friend, the Rev. J. Leighton Wilson, believes the word sassy to be a corruption of the English term "saucy," which is in common use along the coast, and is employed to express ideas of strength or power. Thus the surf on a stormy day is described as saucy. On account of the supposed efficacy of the ordeal bark, and its power in exposing the guilty, the term "saucy," as expressive of its energy, is applied in the corrupted shape of "sassy" to the bark in question.

The African method of trial by ordeal is extremely curious, and resembles, to some extent, the trial by bitter water, described in the Old Testament, Numbers, V., 18–31. Although the custom of ordeal is mentioned by numerous authors, and undoubtedly prevails throughout a great extent of country, and among tribes otherwise unlike, the material employed is not in every case the same. On the Gambia and Congo rivers red water or sassy is used, in Northern Guinea nkazya is employed, and in old Calabar they use the nut described by Prof. Christison. This must be a far more potent substance than sassy, unless the specimen of the latter examined by us had lost some of its activity, which we can see no reason to suspect.

The mode in which sassy bark is used in the ordeal trial, is described by several African travelers with more or less detail. They differ somewhat, however, in regard to the effects induced by sassy or red water, and we shall see that some of them have been misled by hearsay statements, or that there are varieties of red water bark possessing analogous but not identical properties.

We quote, without present comment, several of the best authorities. Beecham† thus describes the trial by red water ordeal:

"The oath chiefly administered in criminal cases is Adóm. The draught is an infusion of the bark of a tree, called by

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* C. A. Santas. The Am. Jour. of Pharm., April, 1849.

† Ashantee and the Gold Coast. By John Beecham. 1841.

that name, in about half a gallon of water. The person charged with crime is required to drink the nauseous mixture; and it is believed, that if he be innocent his stomach will reject it, but if he be guilty, it will not make him sick. It is supposed that the fetish goes down with the water into the man's stomach, and looks about in the heart for the guilt; that if he discovers the prisoner to be innocent, he comes back with the draught which is vomited up again; but that if he finds the alleged guilt, he remains in the stomach with the water to inflict merited chastisement upon the culprit."

Winterbottom* also gives a very interesting account of this ceremony. He describes the red water as the infusion of the bark of a tree, called by the natives kwon, okwon, or millee. This infusion is emetic, and sometimes purgative. In some cases it has proved immediately fatal. which he thinks leads to a suspicion that occasionally some other addition must be made to it, especially as it does not appear that the delicate are affected by it more violently than the robust. We shall see, however, that red bark is narcotic as well as emetic, and hence that the deaths, which sometimes follow its use, may be explained in other ways than by presuming that stronger poisons are added to the draught, when a dangerous person or a rival is to be disposed of. Just before the infusion is given, the accused is obliged to eat a small quantity of rice in the grain, and is allowed no other food during twelve hours before the trial. The red water, says our author, is administered to him in a calabash, capable of holding about half a pint, which he empties eight, ten, or a dozen times successively, as quickly as it can be filled. It probably now begins to exert its emetic powers, but he must, notwithstanding, persist in drinking until the rice or kola be brought up, which is easily seen upon the plantain leaves spread below. Should vomiting not be caused, and the medicine produce purgative effects, the

* An Account of the Native Africans in the neighborhood of Sierra Leone, etc. By Th. Winterbottom, M.D., etc. 1803.

person is condemned immediately; or if it be suspected that the whole of what he has eaten has not been brought up, he is permitted to retire, but with this reserve, that if the medicine produces no effect upon his bowels, until next day at the same hour, he is then, and not before, pronounced innocent, otherwise he is accounted guilty; when the "red water" proves purgative, it is termed "spoiling the red water." The utmost quantity which may be swallowed is sixteen calabashes full; if three have not the desired effect, the prisoner is not allowed to take any more. When neither vomiting nor purging are produced, the "red water" causes violent pains in the bowels, which are considered as marks of guilt. In some instances the person has died after drinking the fourth calabash. When the accused leaves the tripod, upon which he sat during the ordeal, he is ordered to move his arms and legs, to show that he has not lost the use of them, and immediately runs back into the town, followed by all the women and boys shouting and hallooing. People who have undergone this trial and escaped, acquire from that circumstance additional consequence and respect. The author afterwards speaks of the red water as frequently causing death upon the spot.

Livingstone,* the latest authority, thus describes the custom in another locality, and where perhaps the plant is not the sassy or red bark:

"As we came away from Monina's village, a witch doctor, who had been sent for, arrived, and all Monina's wives went forth into the fields that morning fasting. There they would be compelled to drink an infusion of a plant named 'goho,' which is used as an ordeal. This ceremony is called 'muavi,' and is performed in this way: When a man suspects that any of his wives have bewitched him, he sends for the witch doctor, and all the wives go forth into the field and remain fasting till that person has made an infusion of the plant. They all drink it, each one holding up her hand

* Missionary Travels and Researches in South Africa. By David Livingstone, LL.D., D.C.L.

to heaven in attestation of her innocency. Those who vomit it are considered innocent, while those whom it purges are pronounced guilty, and put to death by burning; the innocent return to their homes and slaughter a cock, as a thank offering to their guardian spirit. The practice of ordeal is common among all the negro nations north of the Gambesi. This summary procedure excited my surprise, for my intercourse with the natives here, had led me to believe, that the women were held in so much estimation that the men would not dare to get rid of them thus. But the explanation I received was this. The slightest imputation makes them eagerly desire the test; they are conscious of being innocent, and have the fullest faith in the 'muavi' detecting the guilty alone, hence they go willingly, and even eagerly, to drink it. When in Angola, a half caste was pointed out to me, who is one of the most successful merchants in that country; the mother of this gentleman, who was perfectly free, went, of her own accord, all the way from Ambaca to Cassange to be killed by the ordeal, her rich son making no objection. The same custom prevails among the Barotse, Bashubia, and Batoka, but with slight variations."

One of the latest and probably the most reliable account is given by the Rev. J. L. Wilson,* whose acquaintance with the manners and customs of Western Africa is most thorough, and whose statements in regard to them are entitled to the utmost respect, from the fact of his having resided for many years among the natives. He says:

"But terrible as witchcraft is in either of these aspects, there is a complete remedy for it in the 'red water' ordeal. This has the power, when properly administered, not only to wipe off the foulest stain from injured innocence, but can detect and punish all those who are guilty of practicing this wicked and hateful art. And [illegible] results of this ordeal, there is and [illegible] acknowledge[illegible]

* Western Afric[illegible]
Leighton Wilson. [illegible]

of gainsaying or questioning the correctness of its decisions. The red water is a decoction made from the inner bark of a large forest tree of the mimosa family. The bark is pounded in a wooden mortar, and steeped in fresh water until its strength is pretty well extracted. It is of a reddish color, has an astringent taste, and in appearance is not unlike the water of an ordinary tan vat. A careful analysis of its properties, shows that it is both an astringent and a narcotic, and, when taken in large quantities, is also an emetic. A good deal of ceremony is used in connection with the administration of the ordeal. The people who assemble to see it administered, form themselves into a circle, and the pots containing the liquid are placed in the center of the inclosed space. The accused then comes forward, having the scantiest apparel, but with a cord of palm-leaves bound around his waist, and seats himself in the center of the circle. After his accusation is pronounced, he makes a formal acknowledgement of all the evil deeds of his past life, then invokes the name of God three times, and imprecates his wrath in case he is guilty of the particular crime laid to his charge. He then steps forward and drinks freely of the 'red water.' If it nauseate and causes him to vomit freely, he suffers no serious injury, and is at once pronounced innocent. If, on the other hand, it causes vertigo, or he loses self control, it is regarded as an evidence of guilt, and then all sorts of indignities and cruelties are practised upon him. A general howl of indignation arises from the surrounding spectators; children and others are encouraged to hoot at him, pelt him with stones, spit upon him, and in many instances he is seized by the heels and dragged through the bushes, and over rocky places, until his body is shamefully lacerated, and life becomes extinct."

"No particular quantity of 'red water' is prescribed, and the amount administered always depends upon the state of feeling in the community toward the accused. If they are indignant toward him, and are intent upon his destruction, they compel him to swallow enough of the 'red water' to endanger life, even if it had no poisonous qualities. If he

die after drinking the usual quantity, it is a clear evidence of guilt." The author also refers, p. 398, to a still more curious ordeal poison called nkazya, used in northern Guinea. If it act as a diuretic it is a mark of innocence; but if as a narcotic, causing dizziness or vertigo, it is a sure sign of guilt. Small sticks are laid down about eighteen inches or two feet apart, and the suspected person, after he has swallowed the draught, is required to walk over them. If he has no vertigo this is an easy task, but if his brain be affected, he imagines they rise up before him like great logs, and he is then apt to step awkwardly and fall down, whence he is at once concluded to be guilty.

It is not a little difficult to reconcile the various opinions expressed in these quotations and elsewhere, unless we suppose that the substances knwon under the names "sassy," "saucy," "gidu," "red bark," "goho," "kwon," "millee," and "adum," are not always one and the same material.

All of the authorities agree as to the emetic property of sassy. Livingstone and Winterbottom consider it to be purgative when it is not vomited, but all unite again with Wilson in the statement, that in some instances it causes death. Mr. C. A. Santas, who first analysed the sassy bark, when speaking of its effects, says:—"No detailed and accurate account of its influence upon the human economy has as yet come under the observation of the writer, but it is said to produce vomiting, redness and glazing of the eyes, and loss of power of contracting the muscles throughout the body, so that when the poison has fairly commenced its action on the system, the sufferer is incapable of standing or walking, and the head rolls heavily about the breast and shoulders. When given to inferior animals, a decoction of the saucy bark caused alternate dilatation and contraction of the pupils, appearance of delirium, violent retching, vomiting, symptoms of tetanus, etc., finally death." Mr. Santas does not refer to his authority for these descriptions, and we are, therefore, at a loss to know how much weight they should have. We shall see, however, when we come to the detail of our own researches, that some of the seeming incon-

sistences of these various statements may be reconciled, and we, therefore, pass at once to our own study of this interesting substance.

Physical characters. Sassy bark occurs in pieces four inches and upwards in length, and half an inch in thickness. It has a deep red color, a slight odor, and a marked astringent taste due to tannic acid. The dust, when drawn up the nose, causes violent sneezing and distress about the eyes and forehead. Its decoctions and alcoholic tintures are of a deep red color, and contain all of the active principles of the bark.

Chemical characters. The quantity placed in our hands was not more than enough for experimental purposes, and we have, therefore, been unable to offer any new chemical observations in regard to its active principle.

Mr. Santas states that he obtained a crystalline substance by the action of quicklime on a decoction of the bark, reduced to a syrup by evaporation. The crystalline matter was extracted by alcohol. He obtained a similar material by passing the tincture through animal charcoal, which was then washed, dried, and boiled with absolute alcohol. The crystalline substance was obtained by evaporation. In both instances it is said to have proved deleterious to animal life and to have produced symptoms analagous to those which were observed when the bark was employed.

Prof. Procter, who examined the bark with great care, was unable, on repeating Mr. Santas' processes, to obtain any crystallizable principle. The organic substances which he detected in the bark were tannin, an insoluble apotheme analogous to that in krameria, and a combination of these, which constitutes the red coloring matter of the bark, gallic acid, gum, resin in small quantities, fatty matter, and a peculiar substance precipitable by tannin, and soluble in alcohol and chloroform, but which is not the active principle.

Prof. Procter gave three grains of the aqueous extract to a cat, which recovered after suffering great prostration, frothing at the mouth, moaning, dilatation of the pupils, and total indisposition for food.

The preparation used by us in the following researches was, the dry extract made by evaporating a tincture containing one part of alcohol to three of water, and the tincture itself.

As is usual in such examinations, frogs were first employed to test the peculiar characters of the poison.

Experiment. A large frog received under the skin of his back one drachm of the tincture, previously warmed to drive off the alcohol. At the close of two hours he had become very sluggish, and indisposed to move. He continued in this condition during twenty-four hours, and then gradually recovered.

Experiment. A frog received a similar dose in the same manner. His heart was then exposed, and attentively watched. It became more feeble as the frog became sluggish, but no alteration in the number of its pulsations was observed. In the course of two or three hours the frog became extremely torpid, but was still able to move when roughly irritated. A second frog, which received 1ʒ of the evaporated tincture in his back, and three grains of the extract in his stomach, became inert like the others, but at the close of forty-eight hours was again active, and on being replaced in water, was well two days later.

Still larger doses were tried upon other frogs, with so little result of interest, that we turned at length to warm-blooded animals for more satisfactory and definite conclusions.

Experiment. A large rabbit took internally twelve grains of the dried extract. Within an hour he became languid. At the close of two hours he would remain in any strange position in which he might be carefully and gently placed. He could be laid upon his side, or seated on his gluteal muscles in the corner, without an attempt to escape or resist. If disturbed while in these strange postures, or if placed in them suddenly and roughly, he instantly recovered his activity, and sought to escape, although his efforts were characterized by a certain languor and difficulty which brought him to rest again almost immediately. There was, at this period, no change in the pupils. His sensibility was

thought to be diminished, since his skin could be pinched sharply without eliciting any expressions of pain. Six hours later no change was perceptible. He was motionless, unless disturbed, when he moved a few steps and then stopped again, and remained with his head couched on his fore paws. He declined food, but drank a little water. Seventeen hours later he was found dead. His urine contained neither sugar, albumen, nor the coloring matter of the sassy. His stomach contained a mass of hay, lettuce, etc., as is usual with rabbits, whose stomachs are never quite empty. The coloring matter of the sassy seemed to have disappeared, and there was not the least evidence of inflammatory action in the stomach or intestines. The heart was large and relaxed, the right cavities containing most blood. The blood vessels of the meningies of the brain were considerably injected, but no other abnormal appearances presented themselves.

Experiment. A small doe rabbit received under the skin of her back eighty drops of the tincture of sassy, previously warmed until it scarcely retained any odor of alcohol, and was thick and turbid. At the same time forty drops were given internally. At the close of half an hour, she was sluggish and unwilling to move, although able to do so when pushed. This torpor gradually increased, the skin became insensitive, the eyes remained half closed, and the limbs appeared feeble, especially the hind legs. When roughly shaken, the torpor passed off for a time, and the rabbit even ate a small quantity of lettuce. In a few minutes, however, the animal again sought the corner, and relapsed anew into the state of stupor above described. If carefully handled, it was then possible to place the animal on her side, or even to suspend her, head downwards, without the least show of resistance or discomfort. During the continuance of these symptoms, the heart beat feebly and the respiration was labored, and sometimes jerking in character. Eighteen hours after receiving the sassy, the rabbit was more active, but not free from a marked languor or difficulty, which appeared in all of her motions. Twenty-five hours after the poisoning, the stupor seemed to have deepened

again, and was so profound that the eyes remained nearly closed, and the head, resting on the fore paws, rolled to one side or the other. The rectal temperature was now 102¾° F., or about one to two degrees below the normal standard. So insensible was the rabbit at this time, that she did not appear to feel the passage of the thermometer bulb through the sphincter, although the operation is usually resisted by all animals with great energy. Within ten minutes she slept again, notwithstanding that the thermometer was still in the bowels. Fifty hours from the date of the first dose, the animal was again reviving. At this time she received 1ʒ of the tincture in the stomach, and 1ʒ in the rectum, where it was confined by suitable means. In twenty minutes chewing motions of the jaws were observed, the head fell and was caught up again, and at last reposed on the fore paws, the pupils contracted, the heart became slow and irregular, the respiration quick and labored, and at length, in one hour from the last dose, death occurred with general convulsions and sudden dilatation of the pupils. Upon examination, post mortem, the membranes of the brain were found to be highly congested, but no other appearance of specific value presented itself.

Experiment. A small cat received internally five grains of dried extract of sassy. At the close of two hours she was sluggish, and, half an hour later, vomited with great violence. The matters rejected had none of the color of the extract. Another dose of four grains was vomited up within an hour, and apparently caused great discomfort, due perhaps to the nausea caused by the drug rather than to pain. The disposition towards stupor, which was seen after the first dose, appeared to be relieved by the emesis. The cat survived, and next day was none the worse for the ordeal.

Experiment. A large pigeon received, under the skin of the belly, eighty gtt. of the partially evaporated tincture of sassy. At the same time twenty drops of the tincture were given internally. Within half an hour the pigeon sought a corner, and assumed the usual sleeping posture, the eyes

closing and the respiration being rather uneasy and laborious, Now and then it awoke and staggered about, only to sleep again in a few minutes. When thoroughly aroused by rough shaking, it exhibited more activity. Fifty-five minutes after receiving the sassy, the pigeon vomited very freely. Occasionally the sleep, into which it always relapsed, became so profound that it staggered and fell down. At the end of two and a half hours the sleep had become comatose, and no agency had power to break it. Meanwhile the sensibility of the surface was impaired, the respiration suffered, the temperature fell to 97½° F. in the cloaca, and death took place, without convulsions, three hours and a half from the time the first dose was given. A second pigeon, similarly treated, except that the doses were smaller, survived twenty-four hours and exhibited no symptoms of importance which have not already been noted, except that at the time of death very slight convulsive movements were seen.

It was apparently plain, from the above detailed observations, that the sassy bark contained a narcotic, or at least a stupefying principle, and it was also probable that it posessed emetic activity. This, however, could scarcely be inferred from the effect upon the cat or the pigeons, because these animals vomit upon very slight provocation. The experiments upon animals had, however, convinced us that we were dealing with an agent which could be safely used in large doses, and which was certainly not a potent poison. Moreover, it was possible that the emetic effect, observed during its liberal use in half pint doses for ordeal purposes, might be due to the bulk of the infusion, which the person suspected was forced to take. Should this have proved correct, and its emetic powers be really feeble when taken in moderation, we should posess a drug in which co-existed narcotic and astringent activity. Such a remedy would have had a certain value, and, considering the poverty of our pharmacopœia in good narcotics, could not have been overlooked. We were also aware that Dr. Thomas Savage, formerly a practising physician on the coast of Africa, had

made use of sassy bark in dysentery, but with what effect we were then uninformed.

With these considerations in view, we resolved to test still further the properties of sassy bark, by experimenting upon ourselves. Accordingly one of us took, fasting, three and a half grains of dried extract of sassy. Four hours later he took forty-five drops of the tincture. No effect was perceived, and fourteen hours later he took, fasting, one hundred and twenty drops of the tincture. These repeated doses had no effect, except to interrupt the regular daily action of the bowels. Again, in five hours, another dose of one hundred and twenty drops was taken. Up to this time no symptoms were developed which could be referred with confidence to the effect of the sassy. On a second occasion the same individual took, at one dose, one tablespoonful and a half of the tincture of sassy bark. This large dose was taken two hours after a light meal, at about 9 p. m. Half an hour afterwards, while engaged in writing, he experienced some little giddiness and slight frontal pain, with a sensation of fullness, chiefly at the front of the head. None of these symptoms were of marked intensity. About ten o'clock the tendency to sleep became so apparent that he ceased writing, in which occupation he had been previously engaged, and left the house in the hope that a brisk walk would lessen the effect of the dose. This proved to be the case, but as the narcotic influence faded, a rapidly increasing nausea took its place, and resulted in violent emesis at 1 a. m., five hours after the tincture was employed. In both instances the pulse fell about fifteen beats below the normal standard.

The effects of the sassy upon another of the authors of this paper is recorded in the following statement:

At 11 a. m. he took four drachms of the saturated tincture of sassy. At the time his pulse was ninety-one per minute. He experienced no unusual sensation till about 12 m., when slight dizziness ensued. At this time his pulse was e three. The narcotic influence was not increased, and p. m. the pulse beat ninety-three. At this hour he took

additional drachms. At 2 p. m. his pulse had fallen to eighty, and the sedative effect was very sensibly experienced. Repeated the dose. The pulse continued at from eighty to eighty-four till 5 p. m., when it began to rise. At 7 it was ninety-three. After the third dose the narcotic effect was well marked, but was not greater than would have been produced by a grain of opium. The principal feeling was a pleasant lassitude, a desire for quiet, and an indisposition for either mental or physical exertion.

When the first dose was taken his bowels were slightly loose, and there was some little griping. Both, however, were relieved, and the following morning the bowels were somewhat constipated.

Two days after the foregoing experiments he took, at 10 a. m., one ounce of the tincture. Its effect, marked by a decline in the rapidity and force of the pulse, and the feeling of lassitude above mentioned, was experienced in half an hour. The narcotic was well marked, and was accompanied with headache and slight nausea. The pleasant feelings, experienced with the smaller doses, did not accompany the action of the larger quantity. The pulse remained at from eighty to eighty-five per minute, till about 4 p. m., when the effects began to wear off, although the face was unusually flushed as late as seven o'clock the same night.

Upon careful consideration of the foregoing experiments, we are disposed to conclude:

1. That sassy bark is a feeble narcotic.
2. That it is nauseant and emetic.
3. That it has a marked astringent effect, and that we have seen nothing justifying the belief in the purgative powers ascribed to it by some writers.

Since the forgoing paragraph went to press, the authors have received the following additional remarks from their friend Dr. Savage. It will be observed that Dr. Savage differs somewhat from Prof. Procter in his views of the botany of the subject:

THE "SASSY WOOD" OF WEST AFRICA.

It belongs to the Linnæan Class and Order *Decandria Monogynia*, and falls under Section 5, Ovary Inferior; Flowers complete, of Loudon, in his Encyclopedia of Plants. But to no one of the six Genera under that section can it be referred, so that, I have given it the provisional name of Tulleia in honor of Professor Wm. Tully, M.D., who, besides being one of the early and most accurate Botanists of our country, has done much towards enlarging and correcting its Materia Medica. For a specific name I have adopted that of *Africana*. It is reduced then, to its place in Classification in Loudon thus:—

Decandria Monogynia.

§ 5.—Ovary Inferior—Flowers complete.

Tulleia africana.

Generic characters—Flowers many, racemose; racema spiked, axillary, sometimes terminal:—Calyx 5, parted; petals 5; filaments unequal, 4, long—Stigma enlarged and strongly villous; capsule many seeded.

Specific Characters.—Stem large, lofty and branching; leaves ovate, smooth, alternate; bark rough and furrowed, libernum red, juicy and astringent, leaving a prickly sensation and temporary subparalysis of the parts of the mouth when chewed.

Remarks.—In the recent state the inner bark is quite juicy, presenting a pinkish hue which, on exposure to the air, becomes a bright red, the color of the infusion. This fact has obtained for it in other parts of the country the name of "Red-water."

On chewing the bark, especially in the recent state, an astringent effect is produced in the fauces attended by a prickly sensation, similar (though less in degree) to that from the *Prickley Ash*, of the United States. Nausea then follows with a subparalysis in the parts affected. If a strong infusion, or decoction, be taken and repeated, these effects will be increased, followed by stricture across the forehead, severe headache, coma and death.

Medical properties.—Astringent, acrid, narcotic and cholagogue, also diaphoretic, perhaps indirectly a good substitute for Sulph Quinine in intermittents, hence in the old language of the Mat. Medica, a Febrifuge.

Therapeutics.—Intermittents:—

The Intermittent fever of Africa, which with *its sequelæ*, causes the greater proportion of deaths among the whites, under the vague and deceptive name of "*African Fever*," deserves to be called the *scourge* of that country—a good preparation of Sassy, judiciously administered, I believe to be efficient in the case of Intermittents.

Dysentery, Diarrhœa and Colic:

These three diseases are the only ones in which I know that the natives administer the "Sassy-Wood" as a remedy, and but rarely is it done in these. The discovery of its powers arose from the fact that persons having these diseases when called to the *ordeal*, escaping death, were found afterwards to be cured or greatly benefitted. Their mode of administration, as a remedy, is by enemata only.

Cases.—The first trial was made on myself. One evening, about 5 p. m., symptoms of "Chill and Fever" arose—"aches in the back," slight sensation of chilliness, with coldness of extremities, languor and lassitude, sure precursors of "an attack" in Africa. Of an extract made by infusion, or decoction, I took one grain, repeated every hour for four hours. This was followed by a profuse perspiration, which on being checked by a current of air, was easily restored by a little hot water applied to the feet — freedom from the above unpleasant symptoms succeeded. Resumed the next morning the extract, in same doses, every three hours through the day—at 5 p. m. had a slight headache, which, on suspension of the remedy, ceased in two hours—next day repeated the Sassy as before, at 11 a. m., and had return of the headache which ceased on suspension of the dose as before—no return of the Intermittent.

I caused it to be tried in Dysentery and Diarrhœa to a greater extent, and with satisfactory results. Mr. Appleby, a teacher in the Mission, having been freely supplied with

the tincture, and instructed as to its administration, reported as follows:

"Sunday, November 1st.—First case of Dysentery—continued desire to go to stool—pain in the lower bowels; stools, mucus mixed with blood—gave the tincture of sassywood in teaspoonful doses every three hours. At night, pain had subsided, and stools became much less frequent.

"Second and third cases.—Man and his wife, were treated as above, except that the woman, who, from recent confinement, was much worse, and attacked with vomiting, etc., was treated by epispastics of red pepper over the abdomen. By night the stools and fever subsided, and great relief from all the symptoms was expressed.

"Fourth case.—A man past middle age--stools very frequent—treated as above—experienced great relief from the first dose.

"Monday.—Visited all the cases twice to-day, and found all better but the last. The old man presumed on the relief obtained and eat a supper of crabs.

"Tuesday.—Could not see the sick to-day, but several others with the same complaint have come to my house, and have been treated with tincture of the extract of sassywood with decided benefit. It seems to act like a charm in Diarrhœa and Dysentery.

"Wednesday.—Called to-day to see one of the worst cases I ever saw. The native doctors had given him up to die. Gave the tincture as before. All the cases mentioned on Sunday are much better; two are walking about.

"Many new cases have been treated, and all with like success.

"Thursday.—The very bad case of yesterday is yet doubtful—stools though frequent to-day are free from blood—applied a fly blister to the abdomen, but as soon as it began to draw, he became so restless that it was removed. All the other cases are much better. The chief complaint now is the want of something to eat. The woman has suffered from want of proper attendance. In all cases the sassywood has been the chief remedy used.

"Friday, Nov. 6th.—Found the man who had been given up by the doctors, better—number of stools much less. From bloody mucus they have become yellow or bilious. To-day, from 1 p. m. to 6, (five hours,) has had no stool.

"Saturday.—He has passed the night well, having had no stool till this morning.

"Monday, Nov. 9th.—One of the head men, 'Old Neah,' applied to me for medicine, having the same disease severely —stools very frequent and of bloody mucus.

"Wednesday, Nov. 11.—Neah slept well all night, had no discharge, and feels well, Had one stool this morning free from blood or mucus.

"The severe case above mentioned, as having been given up by the *Doctors* to die, has been removed through their jealousy. When the man left, he was said to be much better, and it was supposed would recover.

"I have treated about twenty cases of Dysentery with the tincture of the extract of sassywood. All have recovered but one, who manifested a high degree of inflammation in the bowels, having taken a severe cold when I saw him. He would not permit continuous applications to the abdomen, and little else could be done. I do not see how he could survive under the circumstances."

Ordeal Uses.—Individuals are supposed to possess the power of witchcraft, a very vague notion. To "witch one," is to inflict an injury of some kind, generally, however, to cause disease. This may be done by an "evil eye," *i. e.* causing evil through the eye, a notion not confined to Africa. It may be done by mixing poison with food. Should it be stealthily thrown into the boiling pot, it is said that "the devil is in the pot." In these and other ways may the power of witchcraft, it is believed, be exerted among them. If one be accused of witchcraft, he must assert his innocence by promptly demanding trial by sassywood. Should he hesitate or evade it, this is proof in the community of his guilt. As a test in this particular, it is taken in the form of decoction. The recent bark is bruised in a mortar, and boiled down to a concentrated state, and thus administered in large and repeat-

ed draughts. It is said, however, that when the death of an individual is determined on, the doctors, who prepare it, mix in stealthily subtle poison which surely does their work.

The decoction often acts as an emetic. Should it do so at the outset, it is received as an evidence of innocence. In most cases this occurs, and the accused is led forth by his friends in triumph, to be the happy recipient of their greetings, and well deserved honors.

ART. II.—*Surgical Cases.* BY T. P. BAILEY, M.D., North Santee, S. C.

Case 1.—*Necrosis of Fibula.*

Betty, a negress, aged about twenty, has for several years past suffered from pain and inflamation of the fibula of left leg, with sanious discharge mixed at times with minute portions of bone. The leg is misshapen and enlarged, with two or three cloaca in the line of the fibula, having indurated edges. She is at times totally incapacitated for work, the irritation being so great as to cause fever and painful swelling of the lymphatic glands of the groin. On examination with the probe, I found a sequestrum several inches in length, detached and moveable. Her general health being good, I resolved as soon as fever and inflammation subsided to operate.

Operation.—On the 9th February, patient being under the influence of chloroform, I made an incision along the line of fibula until I reached the involucrum, when the soft parts with the periosteum were dissected. The wound was carefully dilated by assistants and by the use of a small gouge and mallet, I broke up the partitions of bone between the three openings when the sequestrum was discovered. The grove being too small to allow the necrosed portion to be lifted out, I enlarged the same and having divided the imprisoned bone, succeeded with a pair of forceps in drawing out a portion of necrosed bone about five inches in length, The limb was then dressed

with lint and a roller bandage applied, with directions if she was in much pain to give an opiate.

10th. Patient suffered some pain last night which was relieved by anodyne. Wound looks as well as can be expected. Continued dressings.

11th. Wound healthy; continued dressings. From this time granulations began to spring up, and I found it necessary to touch occasionally with nitras argenti, but in a few weeks I had the satisfaction of finding patient able to resume her accustomed duties. I omitted to mention that along with the local treatment 4 grs of iod: potassium was administered three times a day.

Case II.—Hydrocele, Operation, Cure.

Robert, a mulatto, aged between thirty and forty, had contracted gonorrhea on two or three occasions. For some time past there has been a gradual swelling of the right scrotum and testis, and at times much pain, aggravated by riding on horseback. He now suffers much from weight and pain about the loins. On examination I find right scrotum much distended and detect fluctuation and every indication of collection of serum in tunica vaginalis.

Operation.—Patient being previously prepared by avoiding all excitement and the administration of a saline aperient, on the 1st March, I punctured the scrotum and drew off about 8 oz. of clear serum, after which I injected about 3 oz. diluted tinc. iodine of the strength of one part of the tincture to two parts water.

March 2d. There is much pain and swelling this morning with some excitement of pulse; apply cold water to scrotum occasionally.

3rd. Complains of much pain and heat. Had to take anodyne to procure sleep. There is much tumefaction of the parts. To have dose of sulph. magnesia. From this time his symptoms gradually abated. About eight days after the operation, I punctured the scrotum and drew off about an ounce of fluid after which he rapidly improved. He has since suffered no inconvenience.

Case III.—Amputation of the leg—Gangrene of the Stump—Second Amputation—Recovery.

I was summoned in haste on the 8th April to see Hector, a negro aged about forty years; a miller in the Santee pounding mill. While engaged in greasing the journal, &c.. of a large wheel in the mill, being opposite the south window and having on a loose over-shirt, it was blown upon the wheel and caught, when he was dragged in and lashed upon the shaft, which at the time was performing eighty revolutions to the minute, and the computation is, that he was in this position for four minutes before he was discovered and the mill stopped: 80 x 4—320, number of revolutions.

The following are the injuries received: The right foot from being knocked violently upon the beam above was crushed, and two of the tarsal bones picked up several feet from the scene of accident. The articular extremities of tibia and fibula crushed and protruding, the rest of the foot hanging by tendo achillis. A deep gash in the heel of other foot, one rib broken, with several bruises. Pulse feeble and skin rather cold. I examined carefully the crushed leg and the rest of the calf, and saw no reason why I should not attempt to save the knee joint as the limb appeared perfectly healthy at point of election, and the negro was very valuable to his owners. I accordingly resolved to amputate at the point above named. Assisted by a friend, and two or three negroes—the patient being previously under the influence of chloroform—I operated by the circular method. There was very little hemorrhage, which I attributed to the general shock and contusion the limb received. I was necessitated to tie but one vessel. The stump was dressed in the ordinary way by adhesive straps, bandage, etc. A compress and bandage was applied around the chest to confine the broken rib, and the edges of the wound of heel brought together by adhesive straps, bandage, etc. The patient was left as comfortable as his unfortunate situation would allow, with directions that ¼ gr. sulph. morphia should be administered, and, if necessary, repeated in the course of the night. Dur-

ing the operation his pulse was at times so much depressed that he had to be sustained with brandy.

9th. I am surprised to find my patient so comfortable this morning—he is cheerful and hopeful. Pulse has fully reacted. Suffered some pain last night, which was relieved by morphine, and obtained some sleep. Continue morphine to-night. Let his diet be light.

10th. Much the same as yesterday. Complains of pain about contused ankle and foot. Applied cold water dressings. Stump looks healthy. Re-applied straps, etc.

11th. Has fever this morning, is restless, and there is a gangrenous odor from stump. Upon examination I find some portions of flap in a sloughing state. Dressed with a solution of pyroligneous acid and lint.

12th. Much the same as yesterday. Continue the same.

13th: Much the same—discharge from stump not so fœtid—inflammation from ankle and foot has somewhat subsided. Let diet be more nutritious.

14th. Removed some sloughing integuments. I fear there is a tendency in the gangrene to extend. Pulse debilitated. To have a little porter every day.

15th. Not so much fever to-day. Considerable fœtid discharge from stump. Slept badly last night. Continue morphine.

16th. Rested better. Stump looks more healthy. Discontinue porter as it tends to disturb his bowels.

From this time until the 19th the gangrene rapidly extended, and the patient was becoming much emaciated. I saw the necessity of a second amputation, and was impressed with the truth that "if it were done, then it were well if it were done quickly." I was nevertheless startled by the possibility of my patient dying under the knife, but determined not to shrink from what duty pointed out as the only possible chance for the unfortunate sufferer. Accordingly, assisted by a friend and the owners of the negro, I determined to amputate through the lower half of the femur. The patient being under the influence of chloroform, I operated by the circular method, and found it necessary to

secure five vessels besides the femoral artery. The pulse was much depressed after the operation, but with the aid of stimulants the patient reacted, and I was much gratified after dressing the stump, to leave him with a cheerful heart and a determination " to get well."

To avoid prolixity, I would simply state that the ligature on the femoral artery came away on the 4th of May. I had some trouble with the heel and contused ankle, the wounds having assumed an unhealthy appearance, but by touching occasionally with the nit argenti, healthy granulations were made to appear, and with the aid of tonics and a generous diet, the patient has entirely recovered.

This case is remarkable as showing the influence exerted by the mental over the physical organization. At the time of the accident, and ever after, the patient exhibited a presence of mind and strength of will that was wonderful. He remarked, that during the rapid revolutions of the wheel, " he dodged his head to prevent his brains being knocked out," and accommodated himself to his unnatural position, which no doubt prevented his being mangled and killed on the spot. He was always hopeful, and exhibited a patience and cheerfulness under suffering that was worthy of all admiration.

Although not visible at the time, there was, no doubt, a laceration of the muscular substance near the popliteal space, which prevented the success of the first amputation, as considerable traction must have been exerted upon the tendo achillis, from which the foot was hanging after the accident.

Case IV.—Dislocation of the Hip Joint—Reduction by Reid's method.

Simon, a negro man, aged about forty, was engaged on the morning of the 26th July, in assisting to raise a flat, when he was knocked down by one of the levers slipping, and striking him in the vicinity of the hip joint of the left side. He is in much pain, and unable to move the limb. On a careful inspection, I discover shortening to the

extent of about an inch and a half, the toes resting on the opposite instep, and the knee turned inwards upon the other, the limb can be adducted, but I find it impossible to carry it outwards without excruciating pain on my making the attempt, all of which symptoms indicated a dislocation of the head of the femur upwards on the dorsum ilii. I resolved to attempt the reduction by manipulation. Chloroform being administered until relaxation was produced, the patient was placed in the usual position, a band passed around the upper part of thigh, so as to bear upon the perineum, and held by an assistant. The leg was then flexed upon the thigh and the thigh upon the abdomen, when I proceeded to adduct the limb, at the same time giving it a rocking motion, after which it was abducted, bringing the thigh firmly up against the abdomen, and describing the arc of a circle outwards. The head of the bone was felt distinctly to change its position downwards and forwards with a slight rubbing sound. Two more trials were made in like manner, and I began to despair of reduction by this method, when a distinct snapping sound was heard, and the head of the bone slipped into the acetabulum.

I directed the patient to be kept quiet, and ¼ gr. sulph. morphia to be given, also the vicinity of joint to be rubbed with ung. saponis.

28th.—I examined patient to-day—he complained of much pain with muscular stiffness and soreness, and has not been able to sleep without morphia. His limb is somewhat swollen, but he is better in other respects. He stood up in my presence, and made every motion that I directed. The limb, with the exception of tumefaction, compares in all respects favorably with the sound one.

At the time I write (Aug. 4th) patient still exhibits some swelling about the hip, which is caused very much by the violent blow he received at the time of the accident, but he seems to be doing well in all other respects.

ART. III.—*A Case of Epilepsy.* By WILLIAM M. CORNELL, M.D., late of Boston, Mass., now of Philadelphia.

In my last communication for your journal, (vol. xiii., p. 289,) I promised you another article on the treatment of that species of *Epilepsy* and *Epileptiform convulsions*, which occurs monthly in young females; and which arises from the nervous excitement dependent upon *suppression* or deranged menstruation. These cases are numerous, and every practising physician has a greater or lesser number of them. Having made the treatment of *nervous* diseases, for many years, a *speciality*, and, more especially, those connected with epilepsy, many of this class have been thrown in my way.

Let me premise, at the commencement, that all the cases arising from difficulties of this kind are *not curable* by medicine, any more than all cases arising from other causes are. Fidelity requires that, in reporting *cases treated*, those not yielding to treatment should be stated, as well as those in which treatment has been crowned with returning health. As stated in my former paper, many of the class of patients here referred to have recovered under treatment, to the great joy of friends and their own comfort and usefulness.

In the case which will occupy no small part of this paper, the recovery was not complete, when the patient left my *sanatorium* for her home at a distance.

Oct. 22d, 1858. Miss ———, aged fifteen and a half years, was brought to me by her father, for medical treatment, and to remain in my institution at least for several months, or as long as should seem advisable.

She was somewhat less than the ordinary height, fair complexion, light blue eyes, and light hair. The muscles were lax, and the condition of the whole system indicated considerable debility, and a *scrofular diathesis*. When fourteen years old, she menstruated, and was "regular" for six months, though the quantity, her mother said, was less than usual. At the end of six months from the first appearance of the catamenia, she had [illegible] "flooding," without any

assignable cause. Indeed, so profuse was the discharge, that her mother became alarmed and sent for the "family physician." He prescribed for her, and the flooding ceased. From that time till she came to me there was no appearance of the catamenia.

During that year she took a variety of domestic and professional medicines for the purpose of bringing on the monthly term. Some of the remedies were represented as being very *powerful*, and those prescribed by her medical attendants were unknown to her parents. Her mother's representation was, "that every thing had been tried that the ladies or he doctors could think of, but all to no purpose."

Some three months after the last appearance of the catamenia, and the *flooding* above mentioned, she began to grow *nervous*. Soon *spasms* came on, and from the description given of them, I judged they were of an *epileptiform* character. She sometimes had three of these convulsions in one night. She never had any in the day, nor when she was awake. They were always "night visitors," and always came on when she was asleep. She rarely escaped during a night, and often when she went to sleep in the day-time she had them. There was little or no amendment, and no perceptible improvement during the year, whatever the remedies administered.

When she came to me, the following were her symptoms: She was pale, but not *chlorotic*. The appetite was tolerably good. Food was relished. Her spirits were generally good. Perhaps rather better than ordinary, as from representations from some others of my former patients, who had fortunately recovered, she was impressed with an idea that such would be her lot. The mucous membrane of the throat and glands about the neck were secreting a fluid very viscous and tenacious, and more adhesive than any thing of the kind I had ever seen. It was with much difficulty that her throat could be kept clear of this secretion during her waking hours. Her nose was full of little ulcers, and when they became healed, a considerable quantity of *scabs*

were taken from the anterior nares. One eye was also inflamed, and very troublesome. Her abdomen was not flaccid nor tumid, but natural. From a child she had been afflicted with tenderness of the *urethra*, the cause of which may appear in the sequel. Upon a vaginal examination by myself, and at another time by a skillful physician of this city, no vestige of a *hymen* could be found. The vagina was unusually large for a young lady of her age and circumstances, a little inflamed, and considerable tenderness was found at the *os uteri*. The *neck* was bent a little anteriorly. These were all the discoveries made by such an examination of any thing that was abnormal.

Almost immediately upon her coming under my care, inquiry was made whether she had ever been addicted to *self-abuse*, to which question a negative answer was returned. Still the skillful surgeon who examined her with myself, from the appearance of the organs, was led to believe that such a practice had existed; and, though he said nothing to her upon the subject, suggested to me his suspicions.

From a child she had been subjected to that pernicious custom, which abounds with too many good mothers, namely, constant drugging of their children. Under an impression that she had *a humor* that must be purged off, she had taken various forms and kinds of physic, which, I doubt not, had contributed in no small degree to prostrate her health. She said, "she had taken about all the *patent* medicines that had ever been invented." When will mothers, and fathers too, learn the lesson of common prudence on this subject! This is a practice against which, I am well aware, it is useless for physicians to inveigh; and still, when I call to mind the many, *very* many, constitutions ruined by it, and slight ailments aggravated till they have resulted in incurable diseases, among many patients, in my practice of *chronic* cases chiefly, I can scarcely keep my pen from dwelling upon the evil effects of such a course. But I will not be led off into a harrangue against quackery.

From such treatment he

a natural peristaltic action; and this tended to aggravate all the other difficulties in the case.

The first night after she came to my institution, I gave her one grain of pulverised opium made into a pill. She had no ill turn, but slept quietly the whole night. Perhaps, it should be added, she was very much fatigued from a long journey. The next night she took no medicine, and soon after, falling asleep, had a convulsion. It was a nearer approach to *epilepsey* than I had imagined from the description of her case.

She gave a shriek, or uttered what has well been called "an unearthly sound." In a moment I was at her bedside. She was drawn down in the bed, so that she was what is usually called "double"—feet and head being nearly in contact. She was considerably convulsed—the right hand and right foot, being thoroughly stiffened. Her respiration, though not *stertorous*, was very much obstructed. The left hand and left foot were in constant motion, together with the head. As soon as she was moved, so that the air could enter its natural passages, the convulsive movements immediately ceased, and with a long heavy snore she became still, though insensible and unable to speak. She had no more convulsions during the night. The next morning she made no complaint—was quite bright and cheerful. She was wholly unconscious of any attack during the night.

I was satisfied that her case approached *epilepsy* near enough to call it so, and informed her father of my opinion.

I did not think this a proper case for the administration of *fox-glove*, of which I gave you an account in a former paper, and which, as then stated, has in many cases proved successful in my hands. I did not deem this called for in the present case, because the *digitalis* is debilitating, and it has no *emenagogue* effect; at least, I have never seen any.

I put her upon five grain doses, three times a day, of *ferro-citras et quinia*, the English preparation, (for, though we manufacture some medicines quite as well as the English, yet, I do not believe this to be one of them.) It is a very

valuable medicine. Indeed, I do not know another, among the gentle tonics, so valuable, and fulfiling so many indications.

It is thought by the profession, that *quinine* alone is *emenagogue*, and so far as my experience goes, it loses none of its emenagogue property by combining it with the citrate of iron.

I kept her upon this preparation one month, and restricted her diet, cutting off almost entirely her *suppers*. Under this treatment, she lost strength rather than gained, the convulsions, in the mean time, continuing about the same.

I then thought it advisable to modify the treatment, and changed it to the *tincture of strammonium*, of the strength of the officinal preparation, commencing with ten drops at a dose, three times a day; and gradually augmenting the doses, till the *specific* effect of the plant appeared. Under this medicine, she was kept another month, and during this period, her bowels were more regular, and the number of the convulsions decreased; so that she passed twelve nights without one. Still, there were no symptoms of returning *menstruation*. With this improvement, I was not so well satisfied as I now think I should have been. But I then thought, and am of the same opinion still, that the convulsions were dependent upon the obstructed catamenia, and that little would be accomplished towards a permanent cure, till this function should be restored.

It should, also, be mentioned that, for some two months preceding and succeeding her coming under my care, she had *epistaxis*, about the *same time* monthly. This I considered a kind of *vicarious* menstruation. But during the last eight months that she remained with us, she had no hemorrhage of any kind, except, occasionally, raising a very little blood, which I think came from the rupture of some small vessel in the convulsion. She remained with me, in all, ten months.

It appeared to me that the immediate cause of the convulsions was the adhesive character of the secretion from the mucous membrane, and follicles of the throat and mouth.

One reason for this opinion was found in the fact that, when she was awake, she could keep the throat clear by spitting up the secretion. But when the attention was taken off in sleep, it was allowed to accumulate, and seemed to *glue* down the epiglottis, thus preventing the admission of air into the lungs. This kind of strangulation, I have often known to produce convulsions. Indeed, it is an established fact that convulsions often result from spasm of the glottis, or the exclusion of air from the lungs. Upon this principle was based the well known recommendation of Marshall Hall, to perform *tracheotomy* for the cure, or the relief of the epileptic.

Another reason for diagnosticating the epilepsy as being caused in this way, was found in the fact, that invariably, as soon as she was taken hold of, and so moved that air entered the trachea, the convulsion ceased. The *screech* was faint, and, if I arrived at the bed-side soon enough to move her head, before she became convulsed, the spasm directly passed off without any convulsive movement. As I lodged near her, this was often accomplished.

But, supposing this adhesive secretion to be the immediate cause of the convulsion, the question still arises, what caused the *secretion?* Was it wholly or in part the effect of *scrofula?* Was it wholly or in part the result of the suppressed secretion of the *catamenia?* I confess, there are some difficulties attending the *pathology* of the case, and I make these inquiries with the hope that some of the profession who may have had similar cases, will throw some light upon the subject.

Whenever the throat was more than usually obstructed with this viscid secretion, the convulsions were more severe. But the throat was thus troublesome, and the convulsions severe only once a month, for two or three nights in succession—thus evincing that the suppressed menstruation was the cause of the difficulty; or, at least, that there was more than ordinary excitement in the system *once* a month, and at the *same time* of the month.

The third month she was put upon a combination of *strammonium* and *zinc*. The tincture of strammonium was used as before, in sufficient quantity to produce its *specific* effect, with a solution of *oxide of zinc*, amounting to from three to five grains of the metal, a day. If it be asked, why I resorted to this compound, or, what particular condition of the patient indicated it, I cannot say; the treatment was dictated from *experience* merely. I had given it in numerous cases to patients, whose cases, in many respects, resembled this one, and while taking it they had generally improved, and several of them recovered. In the present case, there was no perceptible change. She did not pass as many nights in succession without a convulsion as she did while taking the strammonia alone, though I think the convulsions were less severe than they had been during the two preceding months. Before the month closed, her countenance began to present the creamy look which I have often seen when zinc has been taken for a considerable time.

It was now the end of the *third* month of treatment, and it could scarcely be said that any perceptible progress had been made towards the removal of the disease. She had lost rather than gained strength, though she took such *gymnastic* exercise every day (as all my patients in the sanatorium do) as she was able to bear, and also rode out a part of each day.

I now began to suspect that I had curtailed her diet too much. This was to be ascribed to the fact, that in the greater number of cases of epilepsy I had found a *spare* regimen, a *diet* consisting chiefly of *boiled mutton and stale bread*, the best that could be adopted. I have still no doubt but that, in nine cases out of ten, epileptcis take *too much* food—that at least ten are injured by *repletion* to one from *inanition*. But the present seemed a peculiar case in this respect and I resolved to put her upon a more generous diet. The result proved the correctness of this determination, as while the convulsions were not increased, either in frequency or severity, she gained in flesh, strength, and spirits. She partook freely of meats and vegetables, bread and butter, fruits,

and tea, (having always drank cold water previously, which, by the way, I am satisfied is not the best drink for that *debilitated* class of persons who become epileptics,) but pork and hot bread she left untouched. Her *suppers*, however, were light, This generous diet was continued till she returned to her home. As one eye continued inflamed, I took her to one of our best occulists. He thought the affection of the eye was dependent upon the condition of the general system, and recommended no very active treatment for it—nothing, save a weak lotion of *alum* or *sulph. zinci.* The doctor's opinion was in harmony with my own.

The next month I put the patient upon a feruginous treatment, together with *Gum Guiac*, (Dewees' Préparation). She held her own during the month, and very little change was perceptible, save that the convulsions were less frequent and milder in their character.

It was about the fifth month of treatment, that in conversation with her about her final recovery, she said: " When you spoke to me about *self-abuse*, I did not answer you as I should have done. I have been addicted to that practice from seven years old up to some time after I came here. Nor did I ever know that it was wrong or injurious, till I read about it in one of your books. I was taught it by an older girl. I never heard my mother, or any other person, say it would injure me, and have done it in total ignorance of its effects. I now think this has been the chief cause of my suffering. Oh! how I wish I could get well."

Thus was developed a new feature in my patient's case. One not altogether unsuspected, and yet one of which there had previously been no positive proof. I was, however, now satisfied that the whole secret of her disease was before me

Still there seemed but little need of any change of treatment, except to caution her against any repetition of the habit. This was done, and the most intense feeling was manifested on her part, and, I believe, a resolution formed never again to repeat the practice.

I was once quite skeptical as to the habit of *self-abuse*

abounding to the extent that some had represented; and also as to its pernicious consequences, especially in females, dwelt upon by some writers. But an experience of many years, in a class of diseases where its effects are very apt to be exhibited, has led me to the conclusion, that neither its frequent occurrence nor its deleterious results have been overstated.

Here was a young lady, in a very worthy and pious family, if not affluent at least having all the comforts of life, her father and mother both professors of religion; and what is more, she herself, not only reared under all the moral training of such parents, but also a member of the church. Yet despite all these moral influences and restraints, she had been unremitting in the practice of a habit which had ruined her health, and induced one of the sorest maladies that "flesh is heir to."

If, in a communication of this kind, it were meet to moralize, what a theme is here presented. Her parents had never spoken to her upon this subject. None of her physicians had intimated a suspicion of such a habit. She had lived to be over fifteen years of age, and had practised it more than half her life, and still asserted that she never knew it to be wrong or injurious.

If I do not greatly err, a great error in this matter is committed by parents; by *mothers* especially, so far as daughters are concerned, and by *fathers*, as respects sons. But the subject is said to be a *delicate* one, and considered among those "not to be named" in the hearing of children. This might be [illegible] if there were none in the community [illegible] them. But wh[illegible] by demon[illegible] the ver[illegible] parent[illegible] such m[illegible]

May I [illegible] are doing [illegible]

The physi[illegible]

man — above suspicion — never capable of rudeness or indelicacy—assuredly avoiding every thing that would lead to an impure thought. If, as the family adviser in their private and delicate relations, he mentions matters of this kind to others, he is not merely unfit for the station he holds, but deserves to be expelled from the profession and driven from society.

Still, can he be faithful to his patient—can he be "the beloved physician," and preserve silence upon this subject, when he believes, or has reason to suspect even, that such a habit lies at the foundation of the malady he is called to relieve? The inquiry, of course, should be made with great tenderness, and the patient and friends should understand that it is made for no other purpose than to be the better able to point out the only course which can result in restored health. When the inquiry is *thus* made, I do not believe one patient, or friend of such patient, in ten thousand, would object. On the contrary, they would thank the physician for his fidelity, and feel the warmest gratitude for his anxiety in their behalf.

But suppose they should object — suppose they should become angry and say: "Such an inquiry is a vile insinuation against the honor and integrity, the morality and standing of the family"—and suppose, further, that they should be so devoid of reason, that no just and candid explanation of the motive prompting the inquiry could satisfy them, would not the physician feel a consciousness of integrity in the discharge of his duty which would compensate a thousand times for the loss of such a patient, or a whole community of such families?

I believe more nervous irritation and debility, more epileptic convulsions, more idiocy and insanity arise from *self abuse*, than from any one, perhaps, from all other causes combined. The reports from the physicians of our almshouses, asylums for the insane, and superintendents of idiotic schools, will bear me out in this opinion. I am satisfied this is the case in New England, and presume it is so throughout the Union. If so, what is to be done? If a *reform* in this matter is ever to be accomplished, physicians must take an

active part in it. While parents, teachers, clergymen, legislators, may all do something, it seems to me, *physicians* have a special call to this work. I am aware that physicians, in the vast amount of their *gratuitous* practice, do much more for suffering humanity, than any other class in the community. But, still, I think this is their plain duty, and I doubt not, many of them are ready and willing to aid in the reform now under consideration.

The patient was treated with tonics, such as iron, zinc, bark, quinine, and almost the whole catalogue known to pharmaceutists, till the last month of her sojourn with me. But their entire effect was simply to keep up her strength, and mitigate somewhat her convulsions.

Cases produced in the manner here spoken of, have proved with me most difficult of cure. The reason, I suspect, lies in their very cause. The nervous system has been so long in a state of excitement, that it has received a shock which time alone can remedy. A few months can scarcely be sufficient to eradicate what has taken years to produce. There was however in this case a marked general improvement, and, as I have reason to believe, that the habit which originated the disease, was abandoned, I trust a cure may be ultimately accomplished.

REVIEWS.

ART. IV.—*A System of Surgery: Pathological, Diagnostic, Therapeutic, and Operative.* By SAMUEL D. GROSS, M.D., Prof. of Surgery in the Jefferson Medical College of Philadelphia, etc., etc., Illustrated by nine hundred and thirty-six engravings. In 2 vols. 8vo., pp. 1162–1198. Philadelphia: Blanchard & Lea, 1859.

This work is probably the most voluminous one that has ever been issued in this country, in any single department of medical literature. Its author has for many years occupied a promiment position among his professional brethren, as an industrious and able laborer in the field of surgery; his opportunities for experience and observation have been large, and he has turned them to good account. His former publications are so well known, that the present one challenges the attention of all who take an interest in its subject.

Our task in noticing this very extensive treatise is by no means an easy one, unless we should overrun the space here available to us. We shall try, however, to give our readers some idea of the manner in which the author has dealt with his subject, or at least of the impressions afforded us by a necessarily hasty survey of his work.

Dr. Gross's first volume comprises 1,162 pages, his second 1,198, forming an aggregate of 2,360 closely printed pages. His subject-matter is divided into two parts; the first occupies 676 pages, under the head of General Surgery; the second, entitled Special Surgery, extends over 1,645 pages; the remaining 37 pages are devoted to a very full and carefully prepared index.

Part first, on General Surgery, contains nineteen chapters. These treat of irritation; sympathy, and idiosyncracy; congestion; inflammation; terminations, and results of inflammation; textural changes; congenital malformations; tumors, or morbid growths; scrofula; wounds; effects of injuries upon the nervous system; syphilis; general diagnosis; minor surgery; operative surgery; plastic surgery; subcutaneous surgery; amputations in general; excision of the bones and joints; anæsthetics, or the means of averting pain.

Dr. Gross' views on some pathological subjects are well known to be at variance with those of most authors of the present day. But he says in his preface, "If, upon certain points of doctrine, I have been obliged to differ from colaborers of acknowledged authority and of the highest professional eminence, it is because I have found it impossible to do otherwise. As Luther said at the Diet of Worms, 'heir etche ich, ich kann nicht anders,' so I may declare that what I have here written, I have written under a solemn conviction of its truth, though certainly not without a strong sense of my fallibility and shortcomings." Views put forth in such a spirit must at least be received with respect.

After two chapters devoted to irritation, congestion, etc., the great subject of inflammation is taken up. This phenomenon, with its results, are discussed in the third and fourth chapters, which together occupy 188 pages. A few points only can be here alluded to by way of comment.

Dr. Gross says, (p. 667,) "We should, therefore, I think, not make any exception, as it respects the possibility of the occurrence of inflammation, even in regard to the scarf-skin, the hair, and nails." Now such an anatomical structure as is presented by these parts, renders the phenomena of inflammation—heat, redness, pain, swelling, and disordered function—impossible. In what way then is the inflammation to be manifested? In fact, if we look closely into this matter, inflammation is a process which concerns the vessels and nerves of the part involved; the other constituents of the tissue are only secondarily affected, because their nutrition is altered or cut off. This position we regard as altogether

tenable; it is one of the results of microscopy, and is theoretically important.

We are very glad to find that Dr. Gross makes only two terminations of inflammation, viz.: restoration to health, and death; all the other so called results being either collateral effects, or properly falling under one of these heads.

The local and general symptoms of inflammation receive ample notice in this chapter; and the intimate nature of the process is discussed at some length. Concerning this latter point, we entertain views differing somewhat from those expressed by our author.

When a mild irritant of any kind is applied to a part, it acts upon the nervous filaments distributed in it. These excite the actions of the part, and blood flows into it in an increased quantity, to answer the increased demand. Let the irritation exceed a certain degree, and the over excited part ceases to act, ceases to demand blood, ceases, therefore, to draw the current which forms the *vis a tergo* of the capillary circulation—and we have stagnation. The walls of the vessels are, according to this view, passive; changing their condition simply by reason of the changes in equilibrium between their contents pressing outwards and the adjacent tissues pressing inwards. To account for all the visible phenomena of inflammation, we think that this theory need only be followed out; it involves less mystery than the doctrines of debility or active expansibility of the capillaries, and is in accordance with the instances daily seen of the filling and emptying of the vessels under nervous influence.

The treatment of inflammation, constitutional and local, is very fully and systematically described; and the chapter concludes with a survey of the chronic form of the disorder.

Chapter IV., on the terminations and results of inflammation, is of course a very important one. "Lymphization," as Dr. Gross not inappropriately calls the effusion of plastic-matter, is discussed at some length, but the mode of formation of vessels in the reparative tissue is, in our opinion, too hastily disposed of; no allusion being made to any of the theories upon the subject. Suppuration, hœmorrhage, gan-

grene in all its varieties, ulceration, granulation, and cicatrization, are the topics to which the remainder of this chapter is devoted.

Ulcers are divided by Dr. Gross into the common and the specific; the former class, the scope of which evidently, includes two varieties, the acute and chronic; the chronic are again divided into the indolent and the irritable forms. Specific ulcers are said to "owe their origin to the operation of some peculiar virus, as the poison of syphilis, small-pox, glanders, malignant pustule, scirrhus, encephaloid, tubercle, or melanosis." With all this we agree, except that we do not know of any *tuberculous* ulcer—none such is described by Dr. Gross—and that the ulcers which co-exist with the different forms of cancer, are not to our knowledge different from one another.

In Chapter VII, the important subject of tumors is discussed. It is a hard matter, in the present unsettled state of opinion as to the real distinction between benign and malignant growths, to give the uninitiated clear ideas of what is and what is not known. Dr. Gross does not seem to us to have overcome this difficulty; his expressions are sometimes inexact, as for instance when he says of enchondroma: "It is also probable that it may occasionally become the seat of carcinoma, resembling, in this respect, some of the other benign formations." The general description given of malignant tumors is quite incorrect in some points; thus it is stated that "they are all deficient in plastic material, while they contain an unusual amount of albuminous substance, or the protein principles of the blood. The consequence is that they are less organizable than the ordinary lymph-products, and also less capable of maintaining a long existence in the parts among which they are deposited." The inaccuracy of these remarks requires no demonstration.

Five forms of malignant tumors are described by Dr. Gross: scirrhus, encephaloid, epithelioma, colloid and melanosis.

Of these, the first two are invariably and beyond all doubt malignant; the last is frequently associated with them, some-

times indeed almost or quite overshadows them; while either epithelioma or colloid may constitute, or help to constitute growths of an altogether benign nature.

On the other hand, there are many fibrous, cartilaginous, and polypoid tumors, to say nothing of other forms, of which malignancy is clearly predicable.

Hence, we think that Dr. Gross' clinical teachings must of necessity sometimes differ from his formal propositions in reference to these points; but we must hasten on to other, and perhaps more practical matters.

Scrofula is made the subject of Chapter VIII. Our author insists upon the identity of this affection with tuberculosis; an opinion in which we fully agree with him. But we do not believe, as he is well known to maintain, that tubercle is a product of inflammation—a view which modifies his therapeutics materially. That impress of *type* which gives an individual a large frame and powerful muscle may be, and often is, conjoined with the indefinable and unknown taint which finds its manifestation in what is known as tuberculosis. Nor does it seem to us that this latter state could ever be induced, except indirectly, by inflammation, which may however be excited by the local irritation of this or any other deposit. Hence, antiphlogistics should be used with caution, and never with a view of uprooting the entire evil, in these cases.

Wounds in all their varieties are treated in Chapter IX; their dressing and mode of repair is also described. In Chapter X, on the effects of injuries upon the nervous system, shock and traumatic delirium are discussed; the latter affection is, we think, not sufficiently distinguished from delirium tremens.

Syphilis forms the subject of Chapter XI, and is very fully and clearly discussed; gonorrhœa is treated of elsewhere, in connection with diseases of special organs.

Chapter XII, on general diagnosis, occupies thirty pages. It embraces the general examination of the patient and of the several organs; mensuration; the attitude of the patient; external characters of the part affected; instrumental explo-

rations; examination of the discharges; and microscopica examinations. Each of these topics receives due attention, but diffuseness is avoided.

Minor surgery is the subject of Chapter XIII.

The remainder of this portion of Dr. Gross' work is occupied in the general discussion of operative surgery in its various forms; and seems to us to be fully up to the times. The last chapter is devoted to the subject of anæsthetics. Dr. G. earnestly advocates the employment of chloroform, which certainly possesses some decided advantages over all other agents of this kind, although perhaps a majority of the profession are deterred from its use by the recollection of its occasional bad effects. Amylene (incorrectly spelled *amylin*) is noticed, but very briefly; no one in this country, so far as we can at present recollect, has ever recorded an instance in which it has been tried. Local anæsthesia is not regarded by our author with much favor; we have however seen it used to great advantage in operations involving only superficial parts, such as the removal of a toe-nail.

Part second embraces special surgery, or the diseases and injuries of particular organs, textures and regions. It contains twenty-eight chapters, eight of which are placed in Vol. I, while the remaining twenty make up Vol. II. The subjects of these chapters are as follows: diseases and injuries of the skin and cellulo-adipose tissue; of the muscles, tendons, bursas and aponeuroses; of the lymphatic vessels and ganglions; of the nerves; of the arteries; of the veins; affections of the capillaries; diseases and injuries of the joints. In Vol. II, diseases and injuries of the bones and their appendages; of the head; of the spinal cord and column; of the eye; diseases of the ear; injuries and diseases of the nose and its cavities; of the air-passages; of the neck; of the chest; of the jaws, teeth and gums; of the mouth and throat; hernia; diseases, injuries and malformations of the anus and rectum; wounds of the abdominal organs; diseases and injuries of the urinary organs; of the male genital organs; of the female genital organs; special excisions of the

bones and joints; special amputations; affections of the extremities.

From the above formidable list of the surgical affections and operations to which poor human nature is liable, we shall select a few of the more prominent, as samples of the mass of material presented in the work before us.

The first chapter of Part II contains a very full article upon erysipelas. We cannot altogether agree with Dr. Gross in some of his views regarding the treatment of this disease Thus we cannot bring to mind a single case that has ever come under our notice, in which we should have felt justified in performing venesection *for erysipelas;* nor would we willingly give either purgatives or mercurials to check it. Nothing has ever proved so beneficial in our hands as the tincture of the sesquichloride of iron, in conjunction with the sulphate of quinine; we are disposed also to attach great importance to frequent and careful washing of the affected part with very hot water and some un-irritating soap, protecting it from the air, in the interim between the washing by a mucilage of slippery elm or of sassafras-pith.

We agree heartily with our author in condemning that common but miserable dressing for burns and scalds, the carron oil; we have never tried white-paint, as recommended by him, but have always been fully satisfied with the effects of a thick layer of fresh lard, smeared upon soft linen or muslin.

It would be interesting to analyze many of the ensuing chapters, but time and space are wanting. Chapter V., embracing the important topics of hæmorrhage, aneurism, and the ligation of arteries, appears to us to present an excellent survey of all that is known in regard to them.

Chapter VIII., the last in Vol. I., is devoted to diseases and injuries of the joints. In this chapter, at page 1,012, occurs the following singular statement: "Suppuration, however, does not always take place in this stage of the complaint, (hip-joint disease,) or, if it does, the matter is either very small in quantity, or it is so soon absorbed as not to

produce its characteristic symptoms." We were already aware, from a paragraph on page 160, that Dr. Gross believed in the possibility of this being removed by absorption; but how he finds out that it is present, when it causes no peculiar symptoms, is to us a mystery.

The hot iron is mentioned as an advantageous means of establishing an issue in this affection, which is very properly called "tuberculosis of the hip-joint," but we are surprised to find that the use of linear cauterization in chronic diseases of other joints is not alluded to; we have seen it attended with marked benefit, and believe that its value has not as yet been appreciated.

Dislocations, their mode of occurrence and their treatment, are discussed at length. (Some years ago a very peculiar form of this injury, affecting the carpal bones, came under our notice; and as nothing similar to it is recorded, it may be well to mention it here. A young man had his wrist drawn in between two rollers, in a cotton-bleaching machine; the space was very narrow, and the carpal bones were transferred *en masse* to the palm of the hand. The circumstances of the case were unfortunately such, that we were unable to trace it to its final issue.)

Dr. Gross of course alludes to the mode of reducing luxations by manipulation, which has of late years become so constant a resource. His remarks on this subject strike us as extremely appropriate; but we regret that the *generalization*, so to speak, of the operation—the linking together of the words *luxation* and *manipulation*, is not more strongly insisted upon. For we think that the scope of this mode of procedure will be greatly extended upon further experience, so as perhaps to almost entirely supersede the main force which still has to be often employed.

The initial chapter of our author's second volume is devoted to the diseases and injuries of the bones and their appendages; these appendages being the endosteum and periosteum. This very important chapter occupies 235 pages; it is divided into sixteen sections, the first fourteen of which are taken up with inflammation, caries, necrosis,

tumors, etc., the fifteenth with fractures in general, and the sixteenth with fractures of particular bones.

Dr. Gross divides all fractures into simple and complicated; he mentions the other distinctive terms so long employed, but proposes that they should be laid aside. We do not think that this change would be an improvement; the use of those terms has become so universal, and affords so great a degree of precision, and they are really so few, that we think they will generally be retained.

Much difference of opinion has prevailed as to the propriety of applying bandages next the skin in cases of fracture. Dr. Gross advocates this practice, which we think is useful only in exceptional instances, or during the latter period of the treatment. He says also: "If the material is new, it should be washed before it is used, to rid it of its starch." Now the fact is, the best material for this purpose is new, unbleached, and unwashed muslin, which is softer and more elastic, and therefore much more suitable for making equable pressure than after it has been washed; we never have found "its starch" a source of inconvenience, for it contains none.

In fractures of the extremities, Dr. Gross seems partial to the employment of the tin case, which is more apt to be badly made than any other form of splint. Many a surgeon can carve a splint out of wood, or mould one of pasteboard or gutta-percha; very few can do any thing with a piece of tin, and the ideas of most tinsmiths in regard to anatomy are very vague.

Fractures of the femur, and their treatment, are quite fully described. Our author speaks disparagingly of the apparatus of Desault, as modified by Physick, but he afterwards says, that "in the New York Hospital, where factures of the shaft of the thigh-bone are very common, and where this accident is probably treated with as much adroitness and care as any where in the world, from a third to half an inch shortening is generally looked for, as a natural consequence, by the surgeons of the institution, and it is questionable whether the statistics of other similar establishments,

if they could be ascertained, would exhibit a more favorable result." Now the apparatus invariably used in the New York Hospital, as seen there by us, and as described by Dr. Shrady of that city,* is, in principle, simply that of Desault and Physick; it would, therefore, seem as if the latter gentlemen scarcely deserved to have it said: "The apparatus * * * * of Desault, as modified and improved by Physick, * * * is an awkward contrivance, constantly subject to derangement, and the results obtained by its aid are by no means so gratifying as they should be."

The chapter on injuries and diseases of the head is quite a full one, and seems to embrace the entire subject; but in this, as well as in that upon the diseases and injuries of the eye, the topics are so very various, that a selection can hardly be made among them for present notice.

We pass over the ensuing chapters, which have reference to the ear, nose, air-passages, teeth, etc.; and in Chapter XII. we find a discussion of hernia. This important subject is arranged very much in the usual manner. The forms of the affection treated of are reducible, the irreducible and the strangulated; a classification to which we must object, in spite of the sanction of many high authorities. For, strange as it may seem, many, even among the intelligent readers of surgical works, get the idea that a *strangulated* hernia is neither reducible nor irreducible; there is no statement in so many words that it may be either, and the arrangement alluded to implies the contrary. For the heading "Strangulated Hernia," we would, therefore, substitute "Strangulation in Hernia;" and we would have the very first sentence a statement that this condition may be present, whether the tumor is a reducible or an irreducible one. The anatomy of each form of hernia is concisely stated; so much so that perhaps a few additional cuts, illustrative of the text, would have been very valuable. But the fact is, Bourgery, Teale, and Maclise have been so lavish in this respect, that they

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* New York Journal of Medicine, March, 1859.

have spoilt our taste for less profusely illustrated writings on hernia.

As regards the treatment of strangulated hernia, we think that our author has given hardly sufficient importance to the use of anæsthetics as adjuvants to the taxis. It is, in our opinion, extremely bad surgery to abandon this resource without having first placed the patient *thoroughly* under the influence of ether or chloroform; not discarding other relaxing measures, but trusting to this as the test. If this does not enable us to accomplish the taxis, the knife should be at once employed.

And although here, as in other surgical operations, it is not so much the instrument as the hand and the brain which direct it, there is certainly a great advantage in having a proper knife with which to divide the stricture. Sir Ashley Cooper's knife answers very well, but would perhaps be safer and not less efficient if made somewhat straighter, with a rather shorter cutting edge.

Dr. Gross directs, with great propriety, the administration of a full opiate immediately after the operation, and the continuance of this locking up of the bowels for several days. We are convinced that this measure forms an important element in the success of some operators, and that life has often been lost from its neglect.

A few remarks on internal strangulation of the bowels, and a section on artificial anus, concludes this chapter.

Important as are the subjects treated of in the two succeeding chapters, they must not detain us at present. We would simply remark, that among the operations for the relief of piles, no notice is taken of the wire ligature and double canula; a means of inducing strangulation of the tumor, which we use in preference to any other, and one which ought at least to have been alluded to.

Diseases and injuries of the urinary organs are taken up in Chapter XV. As the subject has been already made the basis of a monograph by Dr. Gross, and his experience in this branch of surgery is known to be very extensive, this chapter could not fail to be a very valuable one. Having

examined it with some care, we have only to allude to it as a very masterly summary of the knowledge at present available upon the topics included in it.

Chapter XVI., on the diseases and injuries of the urinogenital organs, includes some subjects of great importance. We are glad to find that in orchitis Dr. Gross approves of compression by means of adhesive plaster, this mode of treatment having been undeservedly disparaged by some recent writers. For the radical cure of hydrocele, his favorite operation is the introduction of a seton, which is readily done by merely re-inserting the trocar after emptying the sac in the ordinary way, carrying the canula through from within outwards, removing the trocar, passing the seton, and finally removing the canula; the seton being allowed to "remain from twenty-four to forty-eight hours, or until the scrotum is quite hard, and at least one-fourth as large as before the operation." This mode of procedure might certainly be adopted in many cases where the ordinary plans would be objectionable, and presents a great advantage in the action set up being so much under control.

Gonorrhœa is very properly placed under the heading of this chapter, as are also urethritis and spermatorrhœa. The former affection is regarded by Dr. Gross as exclusively local in its character, and we think the reasons he gives justify this view; his remarks upon its treatment are in exact accordance with our own ideas, although many will doubtless be inclined to pursue a more vigorous course. We have for some years been convinced that, if we may be allowed the expression, gonorrhœa cannot be bullied; that it requires a gentle, judicious, and persevering course on the part of the surgeon.

Our author thinks that there is such a thing as non-specific urethritis; the affection is certainly extremely rare.

The discussion of diseases of the female genitals, in Chapter XVII., is somewhat cursory, if we except the sections on ovarian tumors and vesico-vaginal fistula. Ovariotomy is thought by Dr. Gross to be "under certain circumstances, not only justifiable, but absolutely necessary;" these circum-

stances are stated in general terms, but we think that very great care should be exercised in judging of their existence in individual cases, and that in this lies the great difficulty which the practitioner has to encounter.

Chapters XVIII. and XIX. contain descriptions of special excisions and amputations, and are eminently practical in their character.

In the concluding chapter, Dr. Gross has placed the various deformities of the extremities, and some minor affections, such as whitlow, inverted toe-nails, etc. The entire omission of exact references to the authors whose opinions are quoted, seems to us a serious defect in these volumes; it does indeed make them more readable, but this does not atone for the want of a bibliography, which might very easily have been placed at the end of each chapter, or of the whole book.

Dr. Gross' publishers have never issued a book in better style. The illustrations are many of them entirely new, and some of them are admirable; but in Fig. 118 we recognize an old and worthless acquaintance, representing a left-handed man putting a bandage on an arm which is neither right nor left. Fig. 286, intended to illustrate bronchotomy, and all the cuts of uterine diseases, are poor. These, however, are exceptional, and we are perhaps the more struck with them from the contrast they offer to such exquisite cuts as Figs. 20 and 21, 233 and 275, in Vol. I., or Figs. 25, 293, 399, and 457 in Vol. II.

We are aware that the foregoing survey of Dr. Gross' work is quite inadequate to do it justice; he has evidently bestowed great care upon it, and has written with a due sense of his responsibility to the profession. The very arduous task involved in the conscientious authorship of a system of surgery has not been lightly assumed by him, or hastily performed; and we think that few of his readers will be disposed to cavil at the result of his labors. It is so much easier to criticise what any man has done than to do it better, that the former office is often very readily filled by those who would cover themselves with ridicule if they should

change places with the author whom they abuse. Our own object, in the comments we have ventured to make upon the volumes before us, has been simply to offer the impressions made upon us by a very discursive turning over of their pages. Points of difference are readily noted, and soon stated; but the mass of sound and solid doctrine must be left in the main untouched. Nor would we have it thought that the dissent we have expressed from some of our author's views is other than respectful. Theory is to be judged by its influence upon practice, and practice by its results; the man of experience may speak authoritatively upon both, especially when, like Dr. Gross, he speaks modestly.

We recognize, therefore, in this "System of Surgery," a highly important and useful addition to professional literature; and hope that its author may see it accorded the place to which every such effort is entitled.

ART. V.—*A Treatise on Gonorrhœa and Syphilis.* By SILAS DURKEE, M.D., Fellow of the Massachusetts Med. Society, Member of the Boston Society for Medical Improvement, and of the Boston Society of Natural History, &c., &c. With eight colored plates. 8vo. pp. 442. Boston: John P. Jewett & Co. Cleveland, Ohio: Henry P. B. Jewett. 1859.

Venereal diseases have in the last few years assumed a new importance, and excited much discussion; to be accounted for, perhaps, by the more general diffusion of their distressing consequences through the different ranks of society. If late reports on the subject are to be trusted, their prevalence has increased to an alarming extent, and calls for the most urgent consideration. We welcome this volume therefore, as being the first comprehensive treatise, as far as we remember, on gonorrhœa and syphilis, which has issued from the American press.

The work, though not conspicuous for originality, nor presenting new pathological theories, abounds in sound doctrines and in satisfactory and rational treatment. "The paramount design" of the author has been to furnish a book that shall be practically useful, and we feel sure, that its objects in this respect will be fulfilled, and that from its pages much valuable and serviceable knowledge may be acquired.

The author has very properly devoted the first half of his book to the consideration of gonorrhœa—its complications and consequences, for neither the etiology, pathology, or sequelæ of this disease are so well understood, nor its treatment so satisfactory, as to deprive a treatise on the subject of interest or instruction. Indeed, we conceive that there is no one among the long list of human maladies which remains so conspicuously an object of empiricism and quackery as this ordinary consequence of immorality; a disease only less disgusting than its near relative, syphilis.

Dr. Durkee has divided blennorrhagia into simple, non-specific, non-contagious, for which a variety of causes are given, supported by numerous authorities to be capable of producing a gonorrhœal discharge from the urethra, and into true gonorrhœa or contagious blennorrhagia, a specific disease. Dr. D. says "gonorrhœa cannot give rise to secondary syphilitic accidents, and in all cases where such phenomena have followed a gonorrhœa of the urethra, it is certain that true chancres, such as are termed by the French writers *chancres larves*, must have co-existed." M. Baumés of Lyons is here cited as holding the old opinion which has been lately revived by M. Vidal, as regards the constitutional and secondary effects of gonorrhœa. But there are so few of the profession at the present day who entertain the views of this disease held by these Frenchmen, than it can no longer be considered questionable whether or no gonorrhœa ever produces constitutional accidents. As the author remarks, "at the present day, the more enlightened members of the profession agree in the opinion, that gonorrhœa is not a venereal disease," and, one of the best arguments that can be offered against the identity of the syphilitic virus and the virus

of gonorrhœa, is, that the constant tendency of the first is to produce secondary symptoms, which we denominate secondary syphilis, while it is equally true that gonorrhœa, however severe, or however protracted in duration, is never followed by such symptoms.

"Certain consequences and sequelæ follow blennorrhagia but they are not venereal. Gonorrhœal ophthalmia and gonorrhœal rheumatism are constantly met with; so also swelled testicle, sometimes called blennorrhagic epididimytis, occurs somewhat frequently as an accompaniment or consequence of gonorrhœal discharge. The inguinal glands now and then become engorged, and even suppurate while a man has gonorrhœa upon him. This condition of the gland is to be regarded in the light of a sympathetic affection, as a bubo, an adenitis, which is no more to be considered a venereal symptom than an engorgement of the axillary ganglia, which may take place while a felon is forming on the finger, or a vaccine pustule on the arm."

Copaiba as an Injection.—The repeated experiments of Ricord, Egan, and others, tend to show that the balsam of copaiba, when employed as an injection, has no power to avert gonorrhœal secretion, nor to allay the irritable and inflamed condition of the urethra. The theory is, that the remedy requires to undergo a certain modification, which can only be effected by its transit through the kidneys, before it can exert any salutary effect on the urethral disease. Very recently statements have been made by M. Dallas of Odessa, and by Dr. Dick of London, to the effect that they have employed the balsam as an injection in gonorrhœa with complete success. The oil of copaiba also answers well according to Dr. Dick. Here is the formula:

℞ Olei Copaibæ.....ʒj
Pulveris Acaciæ.....ʒij
Aquæ Fontanæ.....ʒvi M

In subacute gonorrhœa and gleet, this injection may be used twice a day for a few days, afterwards more frequently.

The balsam may also be used as an enema for patients who cannot tolerate it on the stomach. Dr. Durkee has

never made trial of it in this manner, but quotes Velpeau as his authority. The following formula is given for an enema, which may be used two or three times in the twenty-four hours:

℞ Copaibæ .....ʒij
Mucilaginis Acaciæ.....℥ijs
Tincturæ Opii.....ʒss

Various formulæ are also given for the internal use of both copiaba and cubebs, which still remain the main pillars of the treatment of gonorrhœa.

We believe the advice on the use of injections to be excellent. Although the idea of curing blenorrhagia, in its initial stage, in conformity with the old adage, *cito tute, et jucunde*, by strong injections of nitrate of silver, is scarcely correct, so far as relates to private practice, yet in many cases, injections of some sort may be advantageously resorted to, and without risk to the patient. As adjuncts to other agents in the latter stages of the disease, when the active inflammation has materially diminished, and the individual no longer complains of urethral pains, nor of ardor urinæ, then injections of a mild, astringent, or sedative character may be employed. They are, generally speaking, most opportune when anti-blennorrhagic remedies have had a fair trial, and have failed to arrest the discharge. Ice cold water may be safely thrown into the urethra several times a day during any stage of gonorrhœa. This can do no harm; on the contrary it often exerts a very favorable influence. In nearly all cases we advise the use of it, many times in the twenty-four hours, provided the patient is so situated that he can employ the syringe in a proper manner.

During the last few years the use of caustic injections has not enjoyed very great favor. The employment of mild astringents seems to have taken their place with the majority of practitioners, and the abortive treatment is not altogether free from disagreeabilities: not unfrequently the pain and inflammation appear to be much increased, and should swelling of the testicle come on subsequently, the patient is often

inclined to attribute this painful consequence of his disease to the previous caustic injection.

We have frequently found an injection very similar to the one here recommended, answer every purpose in the first stages of urethral discharge:

℞ Zinci Sulphatis,
Plumbi Acetatis a a,.....grs. xv
Tinct Catechu,
" Opii a a,.....ʒj
Aquæ Rosarum,.....ʒvi M

We agree, however, with Dr. Durkee that it is often advisable, and even necessary, to vary the character of the injections in different or the same cases.

The third chapter is occupied with a consideration of that troublesome chronic condition of the urethral canal, which not unfrequently baffles the most experienced practioners. In the treatment of gleet, Dr. Durkee recommends very highly the local use of blisters, applied along the whole length of the penis, except two or three lines towards the preputial orifice. The cantharidal collodion is used in preference to the blistering cerate, as being better adapted to the uneven surface.

The very great number and variety of injections which have been recommended and used in gleet, as well as in acute gonorrhœa, expose conspicuously the inefficacy and uncertainty of any particular treatment in a very great number of cases. Almost every practioner has his favorite, but should any one wish to add to his list, we doubt not he may be aided by a reference to the pages of the work.

M. Gaby has lately recommended the employment of oxide of bismuth as an injection in this complaint, which he thinks constitutes the best treatment of *gleety discharges.* Thirty parts are suspended in two hundred of rose water, and so injected as to leave as large a deposit of the salt as possible in the urethral canal.

In chapter fifth orchitis is treated of. The practice of puncturing the tunica vaginalis, *aud even the testicle itself*, first proposed by M. Petit, and since adopted by several French

surgeons in severe cases, is here advised, although the author does not appear to have himself made use of this truly heroic and formidable mode of treatment. We have enjoyed the opportunity of seeing a number of cases subjected to this procedure in several of the French hospitals, with the most speedily happy results. We were told of one case only, out of a large number, where the testicle was lost.

In the sixth, seventh, and ninth chapters, herpes preputialis, eczema preputialis, and excoriations are respectively considered. These conditions of the prepuce we regard as of importance, in as much as an imperfect knowledge of their appearance sometimes leads to a false diagnosis by confounding them with syphilitic affections, and consequently induces the most injudicious and even injurious treatment.

We pass on now to the seventeenth chapter, in which commences the consideration of the more important syphilitic affections.

"The researches," says our author, "prosecuted by those who have enjoyed most ample opportunities for studying the syphilitic disease in all its phases and consequences, as displayed in the human organization, point almost without exception, to the fact that its primal type and characteristic qualities are one and the same." "The virus has its best expression, its most distinct embodiment, as well as its earliest incarnation in the chancre; and we must regard this lesion, *with here and there an exception*, as the starting point or focus, from whence result all the malign and diversified influences and developments known to the medical man as syphilitic. We may consider the fact as established beyond reasonable doubt, that in all the varieties of the disease there is but *one true poison* of syphilis, which poison produces different effects according to the nature of the tissue and the peculiar idiosyncrasy of the constitution in which the disease is manifested; all these different phenomena depending on the same morbific cause."

Dr. Durkee, therefore, believes in the unity of the syphilitic virus, and that the character of the primary expression, viz., the chancre, is owing to the "nature of the tissue and the

peculiar idiosyncrasy of the constitution." He seems to regard this as the generally received opinion. He says further on: "At the present day the general opinion of the profession is, that primary sores, not endowed with the characteristic sign of hardness, have within them the contagious element and the power also to generate secondary accidents."

It is some time since M. Ricord has entertained the doctrine of two distinct and characteristic forms of primary chancre, the soft or non-infecting, and indurated or infecting chancres. We think, notwithstanding the severe criticism of a late number of the *North American Medico-Chir. Review*, that the opinions of this distinguished syphilographer must carry with them great weight; and that so far from doing "an immense amount of harm as a writer on syphilis," his works have had a good effect in provoking a more serious study and consideration of one of the most universal and malign diseases. Moreover, if M. Ricord's doctrines concerning the plurality of chancres be true, their diagnosis must influence materially not only the treatment, but the mental condition of the patient. M. Ricord was led to make this distinction from observing that of the great number of primary cases which came under his eye, very few were followed by secondary symptoms.

Dr. Durkee, however, coincides in the distinction of soft and indurated chancre; believing that the first class of sores is much more common than the latter, and that "there is no variety of chancre so uniformly followed by constitutional symptoms" as the indurated. He differs, at the same time, materially from M. Ricord, in the impression that the soft chancre possesses the power to generate secondary accidents; the great French syphilographer asserting most positively that simple, soft chancre, with a non-indurated base, is a purely local affection and totally incapable of producing constitutional syphilis. Our author admits, however, that the syphilitic "poison may be, and *generally* is, destroyed by the sloughing or mortification of the part in which it is located." This he thinks may account for the fact, and help

us to understand why some primary sores are followed by secondary affections and others are not.

In the treatment of chancres, Dr. Durkee advocates the trial of cauterization up to the third or fourth, and even the eighth day after the lesion has been first noticed. The opinions regarding the local applications of caustics to the primary sores are various and conflicting, both as to whether the caustic can ever be beneficial, and as to the limit of time in which such application may be judiciously made. The potassa fusa, the acid nitrate of mercury, and the concentrated nitric acid are the remedies, used to neutralize the active syphilitic virus, with which Dr. Durkee is most familiar. He agrees with Wilson, that the nitrate of silver does not extend deep enough in its destruction, it only acts superficially. We think further that it forms a protective coat to the virulent matters below. Ricord speaks highly of a caustic made by mixing together powdered charcoal and sulphuric acid in such proportions as to form a thick paste; this is spread completely over the surface of the chancre, and rapidly destroys the poisonous matters.

The constitutional treatment of chancre is made the subject of the eighteenth chapter, in which is included a few remarks condemnatory of artificial syphilization.

M. Grassi's recent analysis of the blood of syphilitic patients is here given. The results, if correct, are valuable and interesting. There is said to be no material variation from the normal condition of the blood in simple chancre, whereas in indurated chancre, and secondary syphilis, there is more or less diminution in the corpuscles of this fluid, an increase in the albumen, with no perceptible variation in the quantity of fibrine.

Iodide of potassium seems to be the remedy most appropriate for this condition, as it increases rapidly the proportion of the blood corpuscles. "In indurated chancre, the proportion of corpuscles in one instance, was reduced to ninety-four parts in the thousand. This was increased by the iodide of potassium, which was taken for one month. The corpuscles at the expiration of this period, were increased

to one hundred and twenty-eight parts to the thousand. In one case of indurated chancre with syphilitic spots, the proportion of corpuscles was reduced to forty-eight parts in the thousand."

The testimony concerning the use of mercury as a remedial agent in the treatment of chancre is of the most conflicting character, and is calculated to fill the mind of the young practitioner with doubt and embarrassment. The employment of the mercurials or their omission in the treatment of the primary sore will depend, we apprehend, upon which of the theories concerning the syphilitic poison, is accepted by the practitioner. The faith in the power of mercury to eliminate the poison of syphilis from the system has not, as a general rule, we believe, lost any of its strength in these latter years; and the physician who sincerely believes in the power of every variety of chancre to generate secondary affections, would be inconsistent and neglectful of his duty, whether for good or evil, in withholding this article; but we are persuaded that the distinction of chancres into simple, non-infectious, non-indurated, that constituting a trite local affection; and into the true constitutional indurated chancre, is daily enlarging the circle of its advocates, and thus the consumption of mercury in this affection is absolutely and relatively diminished. It will be seen by a reference to the very interesting statistics here collected by Dr. Durkee that each separate method of treatment has its enthusiastic defenders, each claiming triumphant success.

Two thousand cases occurring in two years in the British army, under the supervision of the late Sir James McGrigor, were represented as cases of primary syphilitic ulcers, and reported as cured without mercury. The average length of time required to complete the cure, where no buboes existed, was said to be twenty-one days; for those with buboes, forty-five.

"In 1828 experiments instituted by Dr. Fricke in the Hamburg General Hospital, were first made public. In four years, out of 1649 patients of both sexes, 582 were treated with a mild course of mercury, and 1067 without mercury.

The mean duration of the latter method was forty-one days; and that with mercury eighty-five days. Dr. Fricke states that relapses were more frequent, and secondary accidents more severe, when mercury was given. He observes, also, that he has treated more than five thousand patients without it, and has still to seek cases in which that remedy may be advantageously employed."

Prof. Bennett, of Edinburgh, from the evidence of over eighty thousand cases, believes that primary syphilis is cured by what is termed the simple treatment more readily than by any other, and with fewer secondary consequences.

Mr. Bacot thinks that secondary symptoms occur in one case in ten, where no mercury has been used against the primary sore; whilst the proportion is only one in seventy-five where that remedy is employed. Erichsen is of opinion that the simple treatment is more frequently followed by secondary accidents, than the mercurial, where that agent has been judiciously used. Now, in the face of such conflicting testimony, what are we to do? We ourselves are inclined to admit the distinction, incalculably important if true, between the two varieties of chancre, the indurated and non-indurated; and although Dr. Durkee says the difference in the character of the chancre is to be looked for, rather "in the difference in the natural structure of the parts in which the chancre is situated," he admits the importance of this distinction when he says, in reference to the treatment for the cure of indurated chancre, "they (the mercurials) are almost indispensable," and again: "One of the earliest and most frequent forms of syphilis, in which it (mercury) is mischievous, is the non-indurated chancre or ulcer, the cure of which it tends to prevent, by altering the system, in such a manner, as to favor the progress of ulceration rather than arrest it."

The twentieth chapter treats of masked chancre; its definition; usual seat; chancre and gonorrhœa from the same impure connection; *chancre in the vagina and upon the os uteri.* In speaking of the treatment of phagedœnic chancre, the potassio-tartrate of iron is very highly recommended. We

are glad to be able to add our testimony in favor of this salt as an application to this variety of syphilitic sore, as well as an internal remedy in debilitated and atonic conditions of the system.

In cases of sloughing phagedœna, between which and simple phagedœna our author draws a distinction, he gives the preference to concentrated nitric acid as a local remedy.

The carbonate of ammonia is also recommended as capable of fulfilling an important indication in the treatment of phagedœnic chancre, given internally, in those cases where the constitution has been injured by the habitual use of intoxicating liquors, or by other modes of dissipation and debauchery. It is said to be an excellent substitute for alcoholic stimulants.

From the twenty-fourth to thirty-sixth chapter the secondary manifestations are fully treated of. There is no disease whose period of latency is so indefinite, or whose access is so mysterious and treacherous as that of the one under consideration, hence the difficulty and danger of venturing a prognosis from the phenomena offered by the primary sore. The state of incubation of the syphilitic virus is so indefinite that the consequences of its secret action have presented themselves, on the one hand, at the end of a few weeks, and on the other have remained latent for a period of many years. suddenly to burst forth with apparently accumulated energy.

Dr. Durkee lays stress upon the agency of exciting causes, which seems to be required to bring forward the constitutional development in a tangible appreciable form. "The most frequent accidental cause," he says, "is exposure to an inclement atmosphere." Some cases are here cited as showing the great interval which sometimes exists between the primary disease and the secondary affections; one of twenty years occuring in the author's practice.

It is such cases as these which have given rise to the opinion, that the poison of syphilis is never wholly eradicated from the system; the poison being, according to Wilson, slow in accumulating, its excitation of febrile symptoms

seems rather a matter of accident than the consequence of an irresistible law; the patient enjoys no immunity from a recurrence of the morbid action, and the poison is only partially removed by the febrile effect. The poison of syphilis having once entered the system, the blood and tissues appear to become accustomed to its presence; it remains for years, or for life, and gives notice of its existence from time to time, by a variety of symptoms.

Dr. Durkee relates in connection with secondary syphilis, some cases, as occurring without primary accident. We are rather inclined, notwithstanding these, the exact truth of the circumstances attending which it is always difficult to attain, to trust in the doctrine of Ricord, that the only origin of constitutional syphilis is the chancre, unless in hereditary transmission. M. Fournier in his notes on Ricord's lectures has made an interesting analysis of a large number of cases of constitutional syphilis, in confirmation of the doctrine, that secondary accidents are only acquired from chancre or hereditary transmission.

In the course of the year 1856 there were admited into the hospital 826 cases differently affected with constitutional disease—these were divided into two groups, viz.:

Patients with secondary symptoms,	759
" tertiary,	67
Total,	826
Symptoms traced to primary chancre,	815
Thus there remained eleven cases,	11

in which the disease could not be traced to chancres; but on careful examination some of these were found to be examples of hereditary transmission, and in the others characteristic cicatrices or inguinal enlargements proved the previous existence of the primary affection. Such cases as these related by Dr. Durkee are interesting as bearing upon the question, which is still actively discussed, of the contagiousness of secondary accidents. Velpeau has lately stated his belief in the contagiousness of most of the manifestations of second-

ary syphilis. *Dr. Henry K. Oliver, of Boston, has seen true chancre procured by inoculation from a *plaque muqueuse.* Several other authorities are quoted as advocates for the doctrine of the communicability of secondary symptoms.

We must refer our readers to the pages of this volume itself for further information on this all important question, from which he may glean many valuable hints. This portion of the volume alone would establish its claim to a careful perusal. We cannot do more than direct special attention to the concluding pages, which treat very fully and ably of all those affections which are incident to the latter stages of this deplorable malady; including the various exanthematous affection, peculiar to the secondary and tertiary periods, and which are the manifestations of the syphilitic poison by the skin, and the apparent attempt of the organism to accomplish its elimination from, or prevent its accumulation in the blood. The chapters on syphilitic skin diseases are made more interesting and instructive by several well executed plates, and *cases* illustrative of the subject are appropriately introduced, enhancing largely the value of the book.

ART. VI.—*Normal Presence of Alcohol in the Blood.* By WM. HUTSON FORD, M.D. Read before the Elliott Society of Natural History of Charleston, South Carolina, May 18th, 1858. Walker & Evans, Charleston, 1859. Pamphlet, 2to. pp. 57.

Previously to the year 1858, it was generally held by physiologists, that while sugar existed in the blood of animals which had introduced this substance, or those capable of conversion into it, into the digestive organs, it was not produced

* Quoted by Dr. Durkee from Boston Medical and Surgical Journal. Aug. 1857.

in the animal economy, and disappeared entirely if saccharine or amylaceous food was not ingested. Bernard, however, at this date, published a series of experiments in proof of the fact, which he considered thenceforth established, that the liver possessed the power of creating a species of sugar nearly allied to glucose, no matter what the character of the aliment, and even after prolonged fasting. This glycogenic function was said to be most strongly exhibited by the livers of warm blooded animals, but belonged to most reptiles and many fish, and, furthermore, sugar was found in the livers of young calves, in increasing quantities from and after the fourth month of intra-uterine existence.

So remarkable a discovery naturally attracted much attention, a marked impulse was given to experiment in this direction, many of the ablest physiologists have since devoted a large share of time and talents to its investigation, and if no satisfactory conclusion has been so far definitely attained, most important results—both direct and incidental—have followed its agitation and discussion.

Bernard traced this sugar-forming substance—designated by himself glycogenic matter, by Nasse amylum, and by Pavy hepatine—no further than the right ventricle of the heart, but, soon after, Magendie asserted that when large quantities of starch were eaten, traces of sugar could be found in the systemic veins. Moreover, as a solution of cane sugar, or the sugar of milk injected into the veins were excreted through the kidneys, and entirely reappeared in the urine, while grape sugar thus introduced was not found in that fluid, he supposed that the latter was decomposed in the lungs into carbonic acid and water, and was perhaps the great source of animal heat.

It is not surprising that this hypothesis should have been favorably received. Lavoisier's theory of respiration, though unsupported, nay, contradicted by many known facts, was too brilliant to be lightly discarded. Liebig, the renowned authority, had reduced the entire subject of nutrition and calorification under the simplest of formulas; nitrogenous elements being purely tissue-making, and hydrocarbons heat

producers and little else. Most opportunely now a new substance, belonging to the latter class, was discovered; a body rich in oxidizable elements, having its origin within the economy, independent of external supply, and formed at the nearest possible point to its furnace the lungs. What more reasonable than to conjecture that this was the long looked for fuel of the economy, elaborated for the sole purpose of maintaining the animal temperature, decomposed with the greatest facility, and disappearing by conversion into the two chief products of expiration as soon as it was brought into contact with the oxygen of the atmospheric air.

Bernard adopted these views so far as the transformation of the glycogenic matter in the lungs was concerned, but supposed this to be effected by the pneumic acid which Verdeil reported he had found in the pulmonary tissue. The hypothesis was as crude as it was untenable; no such property was proved to belong to the newly discovered acid, and Cloëtta has since proved this pneumic acid to be identical with taurin, itself a product of decomposition. Bernard saw fit afterwards to abandon this position, and asserted that the hepatic sugar was not destroyed after the manner or for the purpose assumed, but converted in the lungs into lactic acid, which is destined for other uses in the system than mere combustion. The sugar he thought performs its chief rolé at the moment of its origin, when certain animal germs, whose offices are developmental and formative are simultaneously, perhaps complementally produced.

It would be tedious and out of place to enter into a detailed account of the discoveries of the effect produced upon the glycogenic function of the liver by puncture of certain parts of the cerebrum, section of the pneumogastric nerves and the spinal cord, irritation of the respiratory organs, and the introduction of stimulants, directly, into the portal circulation. They form an interesting body of unexplained and somewhat doubtful facts, connected more immediately with the pathology of glucosuria, but do not clear up the obscurity which still involves the origin of glycogene in the animal economy, and have no important bearing on the discussion in hand.

It is necessary, however, to glance at some of the more recent opinions promulgated by a few distinguished laborers in this field of physiological research, and to take cognizance of the grounds upon which their views concerning the formation and destruction of glycogene are based.

In 1856, M. Chauveau presented a memoir to the French Academy, detailing the results of a large number of experiments made upon the arterial and venous blood of dogs, horses, etc., from which, if reliable, he justly concludes that sugar is not destroyed in its passage through the lungs, but slowly disappears in the systemic capillaries. The following year Dr. George Harley, in an essay contributed to the British and Foreign Review, re-asserted the truth of the data from which these conclusions were deduced, and confirmed the results of Chauveau's investigations by similar experiments. Blood was drawn from the right side of the heart, after the method of Bernard, by means of a flexible catheter passed into the auricle through the external jugular vein, and from the left ventricle, directly, by puncturing the carotid artery. The blood was carefully introduced into boiling water, its albumen coagulated with previously added acetic acid, and afterwards filtered. The amount of sugar in the clear liquid was then estimated by Fehling's standard solution of sulphate of copper. The blood from the right side of the heart contained 0.100 per cent of saccharine matter, while that from the left side contained 0.085 per cent. In another experiment performed on a cat, in a similar manner, the blood drawn from the two sides of the heart, respectively, contained precisely equal amounts of sugar. By an analysis, moreover, of an ounce of blood drawn from the femoral artery and corresponding vein of a dog, four hours after he had taken animal food, the arterial blood was found to contain 0.24 per cent, and the venous blood only 0.16 per cent, (one third less,) of saccharine material. From these facts Dr. Harley argues that sugar plays an important part in the nutritive processes; that it is concerned in the development and repair of the tissues, and assists in those chemical

changes, constantly going on in the blood, by which nutritive materials are prepared for assimilation.

From all the above evidence it must be admitted as fairly and indubitably proven that sugar is found in the liver and blood of animals *after death*. This is by no means equivalent, however, to a demonstration of its presence in the same situations during life, and to determine this point, Dr. Pavy has performed a large number of elaborate, ingenious and careful experiments, the results of which are stated in a contribution to Guy's Hospital Reports, Vol. IV, 1858. Without examining these circumstantially--and they are probably familiar to most of our readers—we may say, in a word, that they have led him to the apparently unavoidable conclusion, that, during life, the liver does not contain sugar, but a substance nearly related to it, (hepatine) and convertible into it with great facility by a process allied to fermentation, though it resists this transformation as long as it remains in contact with the living tissues of the healthy organ. An interval of only *two* minutes, between the time of pithing an animal and that of making an incision into the right side of the heart is found to be sufficient for the completion of this fermentative change in the blood flowing from the wound. In performing these experiments, no source of error seems to have been overlooked or unprovided for, and every positive method of examination instituted. The blood was drawn from the right side of the heart of living animals in sixty cases; animals were pithed, and a ligature immediately passed around the base of the heart; the *post mortem* transformation of the sugar-forming substance prevented by the injection of a strong solution of potash into the liver; portions of the same liver thus injected and uninjected were compared; pieces of liver plunged into a freezing mixture immediately after death were examined; animals were killed after exposure to very low and very high temperatures, and after reduction of their normal temperature by section of the cord, and oiling their integuments. All of the investigations were conducted with the utmost nicety, and directed by the highest skill which a

long experience could command, and their author has been irresistibly forced to the conclusion, that though the liver, and the blood of the right ventricle are strongly saccharine very soon after death, no sugar, as such, can be detected in either, or really exists during life.

Whatever opinion some may entertain as to the conclusiveness of these experiments, their results are positively affirmed on no mean authority, and the onus of proving these to be partial or false lies with those who question their validity. Dr. Pavy has been the very first—so far as we are aware—who has accepted the hard conditions of exhaustive experimental research in this matter, and who has eliminated the important source of error arising from an examination of the tissues and blood some time after they have been withdrawn from vital influences, by taking the precaution to check all fermentative or decompository changes simultaneously with their removal. He has denied, and we think, absolutely disproved the normal existence of liver sugar within the economy; he has demonstrated the extreme instability of hepatine outside of the body; his experiments and his interpretation of them are not invalidated by any information of which we are possessed at this date, and therefore, even where assent is withheld, it is not permissible to ignore or reject the results of his labors, until actual proof of their valuelessness is thoroughly established.

In accepting Dr. Ford's paper, therefore, as matter for argument, we waive the fatal objection that might be urged against its physiological worth, viz: that the very existence of the substance from which he maintains alcohol to be formed is not proven, and are willing to discuss it on its own merits; merely premising that the great element of error above indicated underlies and vitiates every process in this otherwise careful investigation, upon which he has bestowed so much time, labor and skill.

The author prefaces his experimental examination of the true destination of hepatic sugar, by a consideration of the probabilities in favor of its change, in the lungs, into *lactic acid*, and disposes well and easily of the very singular asser-

tion of Bernard, that the yeast plant is indispensable to the alcoholic fermentation of sugar, by offering numerous illustrations to show that the use of the *torula* as a ferment is artificial and exceptional, and that decomposing albumen, any nitrogenous animal substance, the gluten of seeds, &c., are endowed with the same power, and take the place of yeast in all the operations of nature. While Bernard, however, rejects the theory of the destruction of sugar by fermentation, and denies that it can be converted into alcohol because yeast is not present in the blood, he observes that when the sugar arrives at the lung, it may be changed by reason of the extreme division of the blood into lactic acid by a simple molecular modification, in which oxygen plays a secondary part. Dr. Ford very reasonably objects, that this is really a lactic acid *fermentation*, and that Bernard is logically bound to point out its proper ferment, which he has failed to do. The only substances which possess the power of inducing this fermentation in sugar, are, putrefying *diastase* pytalin, decomposing animal membranes and fibrous tissues, and casein, none of which can be present in the lungs or blood, or be consumed as a ferment; lactic acid, therefore, cannot be formed.

While we are willing to admit the fact that lactic acid is not formed in the lungs, we cannot subscribe to the conclusiveness of the argument by which the author has assured himself of its truth. In dealing with *casein* for example—he is not satisfied to assert that it only holds the relation of a ferment to sugar of milk, and that lactin does not exist in the blood, but goes on to say, that it is only present in the blood of females during the pregnant or lactiferous state, and is then obviously intended for the use of the fœtus, and not the parent; "for should it act as a ferment for sugar, it must be destroyed within the parental body; all ferments appearing to be in process of destruction in the very fermentations which they induce." Now the author's entire theory is based upon experiments which take for granted the identity of behaviour of all fermentescible bodies and their ferments within and outside of the body. No predominating

or controlling influence is granted to the ever-present vital forces, as numerous expressions throughout the essay indicate. Here, however, we have an elective power ascribed to a ferment, in constant contact with oxygen, prone to decomposition, and yet preserving its equilibrium from the necessities of its final destination, despite the disturbing influences to which it is subjected. We shall see, a little farther on, that this perversion of the great argument of final causes—a similar maladroit use of which by some physiologists, gave rise to the memorable controversy between Cuvier and Geoffrey St. Hilaire—is in more than one instance made use of to support his views, in defiance of all sound logic and analogy.

What lactic acid exists in the muscles, Dr. Ford regards as a product of inosite; itself probably formed from the excess of hepatic sugar, and holding an intermediate position between this body and lactic acid. Inosite, therefore, is a transition substance, allied in its properties to the sugar of milk, rather than hepatic glucose, and converted into lactic acid by the incipient putrefaction of the fibrous tissue with which it is in contact. "If it was possible for the sugar of the liver to reach the muscles, it would appear probable that the muscular lactic acid was formed at the expense of the hepatic glucose." No such opportunity occurs, it is affirmed, in the *fasting state, because* then the liver sugar is entirely consumed in the lungs; a very remarkable example of the petitio principii, wherein the labors of Chauveau and Harley, and their demonstration of the existence of sugar in equal quantities in both sides of the heart are quietly ignored, without a word of evidence to the contrary. During *digestion*, it is admitted, the temporary surplus of sugar passes into the muscular system unchanged, and is indirectly the source of their lactic acid. But if hepatic sugar becomes inosite so readily, and if this is transformed into lactic acid in the presence of the ferment already named, how is it possible, that the liver sugar can reach the lungs without undergoing this conversion, in part at least, brought into intimate connection as it is with decomposing nitrogenous substances,

which Dr. Ford regards as in a state of "incipient putrefaction," at every step.

If the hepatic sugar then is *not* converted into lactic acid, (which our author has by no means proved,) what becomes of it? We know that the albuminous principle present in the fluids of fruits, seeds, etc., readily effects a fermentation of their saccharine matter into alcohol and carbonic acid. "May not these processes be simulated in the animal organism?" "When glucose, the true fermenting sugar, is largely and constantly present in an albuminous fluid, filled with oxygen, at a high temperature, perpetually undergoing varied and destructive changes, analagous to those of putrefaction, exposed to the completest aeration, and spread out upon a vast surface, can it in reality escape the alcoholic fermentation?" Analogy, Dr. Ford thinks, inevitably leads us to this conclusion. The processes of vegetable and animal life are in many respects similar, as regards both their methods and agents. In the one, starch universally precedes glucose, and is transformed into grape sugar by the diastase of the grain when germination commences; in the other, ingested amylaceous matters are converted into grape sugar by the intestinal juices, and transported through the portal vein to the liver for further elaboration. Now the germinating grain absorbs oxygen, and expires carbonic acid, its azote remaining intact, according to Boussingault, while its hydro-carbon is diminished. During this process heat is evolved, and an *indirect* combustion whose product is *not* lactic acid takes place at the expense of the sugar.

The glucose of the grape, saps, etc., changes we have seen, in the presence of a ferment, into alcohol and carbonic acid. Shall we reject, therefore, the author asks, the possibility of such a transformation for the animal body? But "if alcohol is a physiological substance, why, it may be asked, do we not meet with it in fresh saps, living plants or seeds? It is, we answer, because plants, fruits, and seeds are *the food of animals*, and because animals cannot with impunity introduce large quantities of alcohol into their systems in so short a space of time, as, for other reasons, periods of repast must

be." Hepatic sugar, however, is more easily transformed into alcohol and carbonic acid than all the rest of its class, and resists the lactic acid fermentation, and this ready transformability authorizes us to assume this to be its end, and "to seek the mode of its destruction, which is obviously its office, in this property."

It is almost superfluous to point out the sophism of the foregoing argument, in which the author reasons from the *non conversion* of glucose into alcohol in the living plant to its transformation into this substance in the living animal. The final cause of those germinative processes in the seed, during which sugar disappears and heat is evolved, is the primary growth of the plant itself, not the secondary support of animal life. Were the requisite conditions present, alcohol would be formed in obdience to universal and necessary laws, without any regard for the subserviency of vegetables to a higher purpose than their own development. The argument in short assumes that the physiological purpose of glucose is conversion into alcohol, whose ready oxidation may liberate a certain quantity of caloric, but that this purpose which is fulfilled in the animal, is interrupted in the vegetable *because* of the requirements of another order of existences, obeying different laws from its own. It would be better physiology, we think, and certainly as good logic, to assert that in both instances the sugar is destined to play a part in the nutritive processes, and is not converted into alcohol because of the unfitness of this substance to aid in the development or repair of vegetable or animal tissues.

Such is the process of reasoning by which Dr. Ford convinced himself of the probability of the metamorphosis of liver sugar into alcohol and carbonic acid, which he has undertaken to demonstrate by direct experiment; and we proceed now very briefly to indicate the method and results of the experiments themselves. The process adopted for separating the alcohol from its associated fluids and solids, was that of distillation, in which large copper balloons, holding from two to sixteen quarts, were employed; where corrosive re-agents had to be used, glass ones were substituted.

Charcoal furnaces and the ordinary spirit lamp supplied the source of heat; baths of sand and of common salt were sometimes employed. All necessary precautions were of course adopted, but we have not space to enumerate these in detail, and for minute description and directions must refer the reader to the essay itself. Fifteen or more successive distillations were required for the concentration and purification of the ultimate distillate. The quantity of material varied from one to twenty pounds, and each experiment occupied some days. The balance used was an exceedingly delicate one of German manufacture, and showed easily the ten thousandth part of a gramme when lightly loaded. The specific gravities were determined by filling a thermometer tube, broken across at the 120° F., with the ultimate distillate at 60°, and then weighing it; this, with the weight of the same tube when empty and when filled with distilled water at 60°, furnished all the necessary data. The table of Drink water was adopted, which gives the amount by weight of alcohol that exists in any aqueous fluid, corresponding to the specific gravity up to ten per cent.

We must omit all reference here to the details of the successive steps by which the organic fluids or solids were made to yield a tolerably pure mixture of water and alcohol. The processes were all similar by which adventitious substances were removed from the distillands without injury to the alcohol. Hydrochloric acid, acetate of lead, and nitrate of silver, were the principal re-agents by which the separation of ammonia free and carbonated, sulphuretted hydrogen, empyreumatic substance, oily matter, etc., was effected. The principle tests relied upon for the qualitative determination of alcohol were: 1*st. Chromic acid dissolved in sulphuric acid*, which throws down an emerald-green precipitate of sesquioxide of chromium, when an equal bulk of this liquid is added to any fluid containing the feeblest traces of alcohol. 2*nd. The optical appearance within the glass tubes when the distilland just begins to boil.* 3*rd. Inflammation of the vapor of alcohol in a test tube up[illegible] of the ultimate distillate.* The last, i[illegible] admitted [illegible] conclusion requires

great delicacy of manipulation, and is only applicable where not less than two per cent, by bulk of alcohol, exists in an aqueous fluid. Its presence was also verified by its sensible properties of expansibility, specific gravity, and boiling point.

It had already been ascertained by Bernard that, in blood left to itself, sugar invariably disappears. How has it been destroyed? Into what substances has it passed? If it has been fermented into alcohol and carbonic acid, cannot the first of these be recovered by distillation? These are the questions which Dr. Ford proposes to answer in the following experiments, the first series of which are directed with a view to obtaining alcohol from portions of blood, set aside at a mean temperature of 60°, in which, after a certain lapse of time, sugar had disappeared. The experiments are five in number; sugar was traceable in each specimen of blood at the time of distillation. The shortest period for which the blood was set aside after being drawn, was forty hours, the longest ninety hours. The largest quantity of blood distilled was 1120 grammes; the largest quantity of alcohol obtained was $0^{gr.}$ .1552. Lactic acid was not tested for. We therefore assert, that so far as these experiments go, it has only been *proved* that somewhere between 80° and 120° F., the sugar remaining unchanged in the blood at the time of distillation, was transformed into alcohol. The author can make no objection on the score of time, since he regards the instant occupied in the circulation of the blood between the hepatic and pulmonary veins as amply sufficient for the same metamorphosis.

Experiments six and seven show that when fresh ox liver is comminuted and set aside at mean temperatures of 60° and 75° F. from 90 to 120 hours, putrefaction becomes well established, sugar entirely disappears, (the test used is not mentioned unfortunately,) and the ultimate distillate burns on ebulition at the mouth of the test tube, and also yields the emerald green precipitate with the chronic acid test.

Experiments 8, 9, 10 and 11, were instituted to determine whether, contrary to Bernard's assertion, healthy lung-tissue

contains sugar. The largest quantity of superficial lung-tissue used was 5160 grammes. The lung-tissue was set aside in these experiments, in the order above mentioned, 72, 96, 17 and 65 hours. The ultimate distillate burned on ebullition in all, struck the green tint with the chromic acid solution in one instance. In the experiment of least duration, the quantity of alcohol was too small to be measured; in that where the largest quantity of tissue was used, the alcohol weighed $0^{gr.}.1640$. No test for sugar previous to distillation was employed.

To ascertain whether fresh lung-tissue contained alcohol, experiments 12, 13 and 14 were conducted. In the one of *least* duration, 870 grammes of comminuted ox lung-tissue, full of blood, was raised over the bath of common salt to 212° F. in *fifty-five* minutes after the animal was struck down. The ultimate distillate burned in the test tube upon ebullition. The quantity of alcohol was $0^{gr.}.0168$.

Experiments 15 to 24 were performed on mixed thoracico-pulmonary blood, collected from the incised throat and lacerated lungs, and received whilst warm in covered vessels of tinned iron, previously washed, wiped, and dried over the furnace. In six cases, hydro-sulphuric acid was slowly added to the blood during its collection. In the experiment of *least* duration, the temperature of the blood when set upon the furnace was 96° F., and it was raised to 212° F. in *forty-five* minutes. The largest quantity of blood distilled was 36,300 grammes. The greatest weight of alcohol obtained was $0^{gr.}.5652$. Alcohol was obtained in every instance; the mean quantity for 10,000 parts of blood without *hydro-sulphuric* acid, was 0.0570, and with this substance was 0.0874. Dr. Ford by a slight mistake in the calculation has stated it to be 0.0862. "It is therefore quite probal" says Dr. Ford, "that the action of the hydro-sulphu is such as has been supposed, and that the great y of alcohol obtained when it was employed was ity, to an abolition or impairment of post-mortem

If the hypothesis, contin ceding experimentation

sugar and alcohol co-exist in the blood. The blood of every organ is in a state of metamorphic change sufficient to cause the conversion of glucose into alcohol and carbonic acid. The flowing blood wherever derived, acts as a ferment upon sugar; but sugar is created in the cells of the liver, and passed into the circulation through the hepatic veins; it should, therefore, be decomposed in the parenchyma of the organ, and alcohol must be obtained from the fresh liver.

In experiment No. 25, 2985 grammes ox liver tissue was raised to 212° F. *forty minutes* after the animal was killed. The quantity of alcohol obtained was $0^{gr.}.0064$. It did not burn in the test-tube, but the ultimate distillate was colored green by the chromic acid test.

No. 26. Dog liver, weighing 650 grammes, was raised to 212° F. *seven minutes* after the animal was pithed. Quantity of alcohol, if present, $0^{gr.}.0024$. The ultimate distillate did not burn, but exhibited reaction with chromic acid solution.

No. 27. 1800 grammes fresh ox liver, distilled in three portions, at mean time of forty minutes after death of animal. Quantity of alcohol obtained was $0^{gr.}.0324$. The vapor of alcohol burned brightly in test-tube, and answered chromic acid test.

If the author's methods and tests are reliable, he hence concludes that alcohol exists in the hepatic tissue of the ox and dog within the period of molecular life.

With the above data before us, we now approach the great physiological question which it is the purpose of these experiments to decide.

"Alcohol being present in the liver, the pulmonary blood and the lungs, within forty-five minutes after death, with what authority are we thence entitled, to argue that it is likewise present during life in those organs and their fluids?" Dr. Ford's argument may be presented in a condensed form as follows:

It has been shown that incipient putrefaction, which is *identical* with the metamorphic changes of the animal body during life, induces the formation of alcohol from hepatic

sugar. What we call putrefaction is the *terminal reactions* of a series of changes whose first steps are in continual progress within the living body. While therefore the alcoholic fermentation of sugar within the organism is due to the fermentative or catalytic influences of organic changes which are prone to pass into putrefaction, the latter being *final*, cannot be the cause of changes which occur during the initial grades of organic decay. The alcoholic fermentation of sugar cannot be simply a post-mortem phenomenon. It is not possible that simple extravasation can entirely subvert the chemistry of the blood and of respiration. The chief difference between the blood within the lungs, and newly drawn blood consists in the impaired oxygenation of the latter. The difference in aeration, however, is immaterial since the fermentation of sugar into alcohol only requires the presence of a little oxygen at the commencement. The proposition upon which hæmatologists have based their reasonings, viz: that there is little difference between blood within the body and that newly drawn, is axiomatic for researches of great value, though not *absolutely true*. It cannot be otherwise than true for the fermentations of sugar, it may be false for the coagulation of fibrine. The natural proclivity of fibrine, however, seems to be towards coagulation, and the change which occurs so easily out of the body, seems to be always imminent within the body. "Blood, therefore, while newly coagulated is plainly in the discharge of its natural functions, and alcohol obtained by distillation from such blood must be the result of healthy and natural processes." It can only be admitted then that the alcohol found in fresh blood and other organic matters, in the foregoing experiments, was formed during life, or after death by processes identical with those of life itself, or at both periods; the main conclusion being, *that if alcohol is produced by fermentation, from hepatic glucose, immediately after death, this substance must be normally present in the liver, lungs and blood during animal life.*

To undertake a minute examination of the above argument, would be to enter upon a discussi[illegible] endless almost as the entire field of [illegible]

stated involves corrollaries which, if accepted, must completely divert the current of modern physiological opinion from the channel in which it seems to be now steadily flowing; and if the fundamental idea be admitted, we will be logically forced to concede that Biology has no other than a derivative existence, though a science which imposes all its conditions of research, formularizes every phenomenon in its own language, assigns the limits of its progress, and determines the very laws of its being.

Before we can surrender so much, we must require that Dr. Ford shall prove the identity of metamorphic change in the living body and putrefaction, by showing that the same products arise from the decomposition of the same substances within and outside of the body; that he shall satisfactorily establish the fact that the blood—that most subtle and composite of all fluids—can be for an instant withdrawn from the vital influences of the vessels and tissues, without undergoing a radical change; that he shall explain the paradox how blood newly coagulated is in the discharge of a natural function, when this phenomenon never occurs so long as its healthy composition and the normal conditions of its circulation are undisturbed; and, finally, that he shall prove the correctness of that logic by which a product of fermentation, occurring after death in an animal fluid, in obedience to chemical laws, can be asserted to be formed by processes identical with those of life itself, and that the distillate of any animal organ necessarily represents one or more of the normal constituents of the same tissue, when it formed a component part of a vital organism. Until he shall have satisfactorily answered these objections, we will still adhere to the belief, that the chemistry of the laboratory and the chemistry of life are neither identical nor convertible terms; that the definite combinations which determine the quantitative laws of the one, are widely different from those indefinite compositions which necessitate the qualitative laws of the other. Doubtless physiology has received and will continue to receive much valuable aid from both physics and chemistry, and a knowledge of these sciences is indispensible to

the solution of many questions connected with organic action, but the laws of life are peculiar and specific, and can only be deduced from a careful and profound study of physiological phenomena. The chemical affinities which govern the behavior of substances outside of the body, are not adequate to explain how the fitness of so composite a fluid as the blood to effect the repair of the tissues can be constantly maintained, through all the variations which the accidents of age, sex, food, temperature, health, etc., are continually impressing upon even the same individual from year to year and from hour to hour. In our search after hidden wisdom, and our earnest toil for truth and knowledge, let us not forget the valuable caution: "*Never to attempt to solve the problems of one science by the order of conceptions peculiar to another.*"

The remainder of the pamphlet before us, discusses the final cause of the alcohol, whose production between the hepatic cells and pulmonary veins its preceding experiments and arguments are held to have established. It should not appear strange, the author thinks, that alcohol is a natural component of the body, "a reliable analogy may be drawn from universal inclination and habit, which point in most cases to the internal chemistry of life." As sugar, syrups, sweet fruits and liquids are abundantly consumed by almost all nations and all classes of animals, so an overpowering desire for alcohol is widely diffused among men, and points to its employment in the organism for the support of an essential function. It is scarcely necessary to say that the same process of reasoning requires us to assert a similar economic purpose for tea, coffee, tobacco, spices, opium, etc., all of which are as imperiously craved and almost as widely used by men as alcohol itself. The true explanation of the large consumption of these articles is, as Drs. Julius Lehmann, Böcker, and Chambers have shown, that they stimulate the nervous system, invigorate the circulation, protract metamorphic decomposition, diminishing thereby the demand for animal food, and produce a general feeling of comfort, elasticity, and cheerfulness.

Dr. Ford is further of opinion that this steady but minute stream of alcohol, flowing into the lungs, is intended for a hæmal combustion, whose ultimate products are carbonic acid and water, and furnishes the prime source of animal heat. The red corpuscles of the blood are the agents of this hæmal oxidation by virtue of their ozonification, and properties analogous to those possessed by platinum black, of condensing large quantities of oxygen, and of parting with it again with great facility in the presence of oxidizable substances. As this change of alcohol by oxidation into carbonic acid and water occurs in the pulmonary organs, much heat is evolved and diffused in the blood while still within the lung substance. *Sent to the tissues without sensible elevation of temperature* (which would perhaps be improper) (*sic.*) by reason of the enhanced *capacity for caloric due to arterialization*, as the blood becomes venous in the systemic capillaries, its caloric is *discharged*, becomes *sensible*, and *the body is warmed.* As albuminous bodies in a state of incipient decay, are assumed to be the proper ferment of sugar, the venous blood laden with this effete material should be more active in this respect than the arterial blood. When, therefore, sugar is distributed through the systemic capillaries, as during digestion, alcohol should be continously formed from it there until the sugar is exhausted, and two experiments, conducted after the method of those already described, resulted in the discovery of traces of alcohol, during digestion, in the pancreatic tissue of the ox, and the blood from the cephalic extremity of the jugular vein of the dog.

The paper closes with an estimation of the ratio which the products of hæmal alcoholic combustion bear to the sugar destroyed, and to the oxygen directly concerned in its ultimate oxidation. We pass the calculation over with simple mention, because, in common with all that relates to the final disappearance of alcohol in the economy, its entire interest depends upon the degree of credit which we attach to the proofs which have been adduced in favor of the formation of the latter substance within the living body.

We have thus passed in review very briefly, and stated,

we believe fairly, the facts and deductions which Dr. Ford thinks establish the conversion of hepatic sugar into alcohol for the purpose of maintaining the requisite temperature of the body. To attempt an estimate of the physiological value of this essay in detail, would, as we have already hinted, imply an argument of entirely too great a magnitude to find admission in these pages. The acceptance of its conclusions, however, involves the surrender, in our opinion, of all the most important advances which our science has recently made, and subjects us again to the thraldom of a chemical tyranny which has been the apparent vassal, but the real despot of biological research for the last quarter of a century. While we cannot, therefore, at present pursue the discussion of this subject further, we submit the following as a summation of the more prominent objections to the correctness of his premises, the conclusiveness of his experiments, and the legitimacy of his inferences and conclusions.

Imprimis, the normal existence of saccharine matter in the liver, during life, is extremely questionable if not absolutely disproved, and requires positive demonstration, at the hands of any one who argues from its presence the necessary formation of secondary products.

Dr. Ford's experiments to be at all conclusive, should have been conducted with liver and lung tissue, and hepatico-pulmonary blood so treated instantly after death, as effectually to prevent any fermentative or decompository action.

Sugar of milk, grape sugar, and what is called "hepatic sugar" are either isomeric or differ at best by one or two atoms of water; the two last indeed, according to the formulas of Bernard and Pelouse, which Dr. Ford accepts, are identical in composition. Now, as sugar of milk is converted into lactic acid at temperatures between 50° and 78°, but forms alcohol and carbonic acid if the temperature is above 90°, while the sugars of fruits—as grape sugar—will yield alcohol and carbonic acid so long as the temperature ranges about 75°, and manna, gum, or lactic acid, if the temperature be raised to 100° or 120°, it is in the highest degree important that Dr. Ford should have shown how "hepatic sugar" behaves

in the presence of a ferment at different ranges of temperature; more especially as glucose, with which it appears to be isomeric, is *not* converted into alcohol at the temperature of the blood.

In all of the experiments, save two wherein the liver tissue distilled had undergone *putrefaction*, sugar was present in the distillands when first subjected to the heat of the furnace, and the *traces* of alcohol found in the ultimate distillates, may without any conceivable difficulty have been formed during the first steps of the distillatory process.

While assuming throughout the chemical reactions of the same substances to be identical within and without the living body, Dr. Ford in endeavoring to prove by *post mortem* changes the transformation of "hepatic sugar" into alcohol in the animal economy, has failed even approximatively to imitate the conditions of its existence within the organism.

It is granted that *inosite*, a possible product of the "temporary surplus" of "hepatic sugar," is changed by fermentation into lactic acid, in the *muscles*, and yet no sufficient reason offered why a different series of transformations, occurring between the liver cells and the lungs, should convert the sugar there, successively, into alcohol aldehyde, acetic acid, formic acid, oxalic acid, and finally, carbonic acid and water.

In arguing the necessity of a special oxydizable substance to supply the caloric of the body, Dr. Ford has completely ignored the heat producing power of plants, which absorb carbonic acid and exhale oxygen under the solar influence, and all those mechanical and electrical agencies and the immense variety of compositions and decompositions constantly taking place in the interior of the organism, which many of the ablest physiologists consider amply sufficient to maintain the normal temperature of the body, by the *secondary* production of animal heat. Moreover, as he is aware of the well tested fact, that the lungs exhibit a lower temperature than any other organ, and as the liberation in them of all the caloric which is to supply the entire system would speedily produce a local combustion, he has committed him-

self to the maintainance of the doctrine of latent heat, which is itself inexplicable, and not universally accepted.

In conclusion, it is our duty and pleasure to add, that while we cannot subscribe to the correctness of the physiological opinions expressed in the pamphlet before us, or grant the conclusiveness of its experimental results, we regard the paper as a most valuable one to the author personally, and welcome it cordially as the earnest of future labor in a field of research where he will doubtless gather a rich harvest in time to come. The essay is eminently thoughtful, original, suggestive; it is a monument of laborious, patient investigation; the result of long and severe study, whose conclusions have been tested by direct experiments, involving necessarily considerable sacrifices, and exhibiting an unusual capacity on the part of their author for delicate and intricate organic analyses. However much we may disagree with the opinions he entertains, or disapprove of his method of investigation, we feel assured that nothing but an earnest love of truth, and a lofty enthusiasm, could have induced him to undertake, or enabled him to carry out, so arduous a task. We shall look forward to his next essay in this department with curiosity and confidence, and shall expect from the same thoughtful and facile pen, a contribution to physiology of abiding interest and value.

BIBLIOGRAPHICAL NOTICES.

ART. VII.—*Contributions to Midwifery and Diseases of Women and Children, with a Report on the Progress of Obstetrics and Uterine, and Infantile Pathology in* 1858. By E. NOEGGERATH, M.D., and A. JACOBI, M.D. New-York: Bailliere Brothers. 1859. 8vo. pp. 466.

Drs. Noeggerath and Jacobi, have undertaken a laborious, and performed a most valuable work in the preparation of the volume before us, and in their promise to present us with a similar review of the progress of obstetrics from year to year. Impressed, as they might well be, with the frightful state of ignorance in which many contributors to our journals are in regard to every thing that has been done before on the subject which they undertake to expound, and with the absence of any full source of reference to the bibliography of midwifery, they conceived the idea, which they intend hereafter to carry out, of making up annually a report of all the manuals, memoirs, essays, cases, &c., &c., published in this country and abroad relating to obstetrics and uterine, and infantile pathology. Wherever the work or paper is of any value or interest, they have given a sketch, very brief of course, of its contents, and indicated the name and authorship, at least of every article which has been brought to their notice. If the volume before us may be taken as a fair specimen of its successors, we can safely promise to all who are interested, a compendious, reliable, and most useful abstract. The references to and translations from the foreign journals are specially valuable to all who have not access to a large exchange, and whom taste or the desire of authorship

or the necessities of the lecture room, urge to a knowledge of so much of the literature of the subject as illustrates the particular object of their study or prelections.

The fact is that the progress of medicine in all of its departments, is chiefly advanced and illustrated by periodical literature. Time is too valuable, and the world too eager and active to wait on the comparatively slow process of book making; print first and publish afterwards, seems to be the universal, and we think very useful maxim. If worth preserving in a more permanent form, the after collection of periodical contributions is not difficult, and the author enjoys the great advantage of having sufficiently circulated his opinions to receive in return a popular verdict as to their worth, which should, but unhappily does not always, guide him in his future disposal of his waifs. In the mean time, the general reader has enjoyed the opportunity of becoming acquainted with the progress of opinion and discovery coincidently with its march, and each new fact or theory finds in consequence, immediately upon its publication, a hundred hands eager to examine and test their validity and worth. Now it is impossible for any one to subscribe to or read the numberless medical periodicals of the day, nor would such labor, if practicable, compensate for the time consumed in the work; consequently many interesting, and some very important papers necessarily escape notice by the individual. Just here, we conceive, lies the value of such a work as the one before us, where in a narrow space may be found such references as the reader can easily consult at his leisure, and with no considerable loss of time. It is as an index and not a guide that it was chiefly prepared, and will be most generally employed, and as such we most heartily recommend the work to the attention of all our readers, with the sincere wish that the patronage may be such as to induce and enable its authors to continue the yearly publications they propose.

In this first volume we have, in addition to the matter above indicated, seven original articles from the editors, four of which are reprinted from our valuable and esteemed cotemporary, the New-York Journal of Medicine. The first

paper, from the pen of Dr. Noeggerath, is on the *Induction of Premature Labor by Cohen's Method*, which the author hopes will supplant the douche, ergot, rupture of membranes, and the other modes commonly resorted to in this country, and on the continent. Three cases are reported, where Dr. Noeggerath used it successfully on account of contracted pelvis and morbus brightii. The method which is an extremely simple one and which may be performed by instruments always accessible, consists in the injection of a few ounces of blood-warm water into the uterus through a flexible catheter previously introduced between the walls of the organ and the membranes of the fœtus. When, from the condition or situation of the vaginal portion of the womb, the catheter cannot be introduced, the douche is of course to be resorted to, and when induction of labor is required in cases of uterine hæmorrhage, Braun's colpeurynter, introduced empty and then filled with ice cold water, is an instrument of incalculable value, both for arresting the bleeding and inducing labor pains, and one which is to be preferred above all others. We have not space to introduce the author's remarks upon the other modes of treatment pursued; but may add that they are uniformly judicious and appreciative. We subjoin the table of his cases, with an additional one performed in New-York by Dr. Blatchford of that State.

AUTHORS.	NUMBER OF INJECTIONS.	TIME OF LABOR AFTER FIRST INJECTION.	FATE OF CHILDREN.	FATE OF MOTHERS.	REASON FOR OPERATION.	REMARKS.
Blatchford.	1	113 hours.	Alive.	Recovery.	Contracted pelvis.	Cranial presentation.
Noeggerath.	2	23 hours.	Alive.	Recovery.	Contracted pelvis.	Cranial presentation.
Noeggerath.	1	19 hours.	Dead.	Recovery.	Contracted pelvis.	Cross presentation. Turning.
Noeggerath.	1	16 hours.	Died soon after birth.	Recovery.	Morbus brightii.	Cross presentation. Turning.

The next article from the same author, describes *four cases of Injection of a caustic solution into the cavity of the Womb*, in one of which there was no sensible reaction; two exhibited very alarming symptoms, and one resulted in death. Dr. Noeggerath concludes from his own experience that the dan-

gers connected with intra uterine injections do not proceed from the passage of the fluid into the abdominal cavity, but from the inflammation set up in the mucous coat of the uterus (endo-metritis) proceeding to the deeper layers, the muscular tissue, and finally to the peritoneum itself, and terminating in the most fatal form of metro-peritonitis.

"From this it would appear, that we ought to abstain entirely from the use of caustic injections into the cavity of the womb. For if it is true that they are at times followed by dangerous and even fatal consequences, they must be considered as means inadequate to the evils which they are intended to relieve. I mean to say that a complaint which is not endangering in a direct way the sufferer's life, ought not to be attacked with a remedy that might possibly remove the disease and the patient at once. To this class of morbid alterations belong hypertrophy, ulceration, abnormal secretion, and fungoid excrescences of the uterine mucous membrane, conditions which have been often treated with solutions. From this consideration, the treatment of violent hæmorrhages is naturally excluded; with regard to them, we must act after the principle; aux grands maux les grands remedes.

"In coming to this conclusion I am far from advising against the use of caustics in general. All I want to impress upon my readers is the necessity of being cautious in their application, more cautious I mean than some of our obstetric specialists. There seems to exist a certain climax in the different remedies themselves, some of them, although very effectual, are comparatively innocuous, while others are almost always followed by violent reaction. Among the former we count the tincture of iodine, and some of the organic acids, such as tannin and benzoe, among the latter, the solutions of silver and mercury, as well as the stronger mineral acids. The remedy which most happily combines a high degree of innocuity and of efficiency is the tincture of iodine. I have had frequent occasions to inject it into the cavity of the womb, and as yet I have never remarked the least untoward symptom from its application."

Article third, from the same source, offers some *Remarks on the employment of Pessaries, with the Description of a New Instrument.* The description of the pessary specially recommended, is prefaced by some very sensible remarks on prolapsus of the vagina and the womb, and the use of instruments in their treatment. Dr. Noeggerath condemns properly the indiscriminate use of pessaries, but when a physician is satisfied, after a careful examination of his patient, that such a mechanical support offers the best mode of relieving her painful condition, it is of the greatest importance to choose the right kind of instrument.

"The requisites of a good instrument are as follows: 1. It must retain the womb in or near its natural position. 2. It must neither irritate the womb nor the vagina. 3. It must not interfere with the patient's moving round, sitting, or excretion of urine and fæces. 4. It must be composed of a substance, which resists the corrosive influence of the secretions of the genitals. 5. It must be constructed so as to be easily introduced, removed and cleaned by the patient herself. 6. It must be as cheap as possible."

Prof. Kilian was the first who seems to have taken advantage of the indication which modern researches pointed out, viz.: that the vagina is the natural supporter of the uterus, and constructed a pessary in 1846, consisting of nothing more than a thin steel spring, four inches long, the points ending in wooden buttons, and the whole covered with india-rubber, which he called an elytromochlion, or vaginal supporter. The instrument has since been abandoned, but the theory of its construction was based upon sound principles, and it evidently gave the cue to Dr. Zwank of Hamburg, who in 1853 published the description of his new hysterophœ. It consists of two thin ovoid pieces of metal, covered with india-rubber or wood, and connected at one end by a joint, in the neighborhood of which, on the external surface of the wings, is a metallic pin on each side, two inches long, and which can be screwed together at the lower end. This instrument has been modified by Dr. Eulenburg of Coblentz, and a

similar one to Dr. Zwank's was almost simultaneously invented by Schilling of Munich.

Dr. Zwank's pessary has been highly recommended by the best obstetricians of Germany, and Dr. Noeggerath has altered Eulenburg's modification of it in the joint, so that it may be easily taken to pieces for the purpose of thorough cleansing. Tiemann & Co., of New York, will supply orders for it, and full directions for its use are given in this paper.

The fourth essay is *A contribution to the Pathogenesis of Uterine Polypi*, for which we are indebted to Dr. Noeggerath again. We can do little more than refer to it as an interesting, though brief, paper. Dr. Rupprecht delivered on the 12th of July, 1858, Mrs. Fischer, of New York, of a strong, healthy child. The placenta was entirely and easily removed. An hour after, the mother had a violent uterine hæmorrhage which ceased spontaneously. Nine days after it recurred in less quantity, and was repeated four days after this again. On examination a large tumor, filling the vagina, and attached to the anterior lip of the uterus was found. The polypus (?) was torn away accidentally, during an examination by Dr. Krackowizen, on the morning of the 28th. The entire mass was smooth and lined by a membrane. The upper aspect, however, presented a rugged patch about an inch in diameter. The tumor was solid, and as hard as the normal uterine tissue. When a small portion was examined under the microscope, "it became evident that the whole mass consisted chiefly of shriveled tufts belonging to the chorion. And consequently the polypus before us was nothing but part of the placenta."

Articles V. and VI. are both from the *New York Journal of Medicine*. The first is a case of *Invagination of the Colon Descendens in an Infant*, with repeated hemorrhages in the Colon Transversum. It is contributed, as well as the two remaining ones, by Dr. Jacobi, and is a most interesting and instructive case, to which we invite special attention.

The sixth article, on the *Oxgsulphuret of Antimony as an Expectorant in Inflammatory Diseases of the Infantile Respira-*

torg Organs, after giving an account of the preparation, composition, and use of the drug, details the author's experience with the article in the children's department of the "German Dispensary of the city of New York." The number of cases tabulated is thirty-four.

The author is satisfied that the want of knowledge as to its proper management, has alone impaired the success which will follow its use when administered rightly. He has seen no reason to dread either its nauseant or purgative effects, and urges the trial of it in large doses upon the profession, whose members, he is convinced, will surely adopt its use in general practice.

The last essay *On the Etiological and Prognostic Importance of the Premature Closure of the Fontanels and Sutures of the Infantile Cranium*, is in every respect a most philosophical as well as laborious paper, having reference to an interesting condition of the relative cerebral and cranial development in connection with some obscure forms of infantile disease. We regret our inability to do more at present than make mention of its value. The bibliographical references are copious, the physiology of the subject is very ably handled, and the reports of a fair number of cases elaborately and admirably presented. If the authors propose enriching the future volumes of this obstetrical retrospect by such original contributions as they have offered us in their initial number, we shall welcome its successors for additional and perhaps even higher reasons than those of mere convenience and extrinsic utility.

ART. VIII.—*A Practical Treatise on Enteric Fever; its Diagnosis and Treatment; being an analysis of one hundred and thirty cases, derived from Private Practice, and embracing a partial History of the disease in Virginia.* By JAMES E. REEVES, M.D. 12mo. pp. 199. Philadelphia: J. B. Lippincott & Co. 1859.

This volume is an enlarged account of cases of enteric fever reported in 1856 for the *Buffalo Medical Journal.* The author has been induced, he tells us, to publish these with additional remarks and statistics, partly by request, and principally because they represent the experience of a country practitioner, support the testimony of other more professional authors, and may serve to direct the attention of his rural co-laborers to the value of recording systematic notes of their important cases.

On the strength of the last reason we are willing to concede more utility to the work than we would otherwise be willing to grant; but, with rare exceptions, we are inclined to doubt the expediency or value of issuing in book-form matter already published in the pages of a respectable journal. Occasionally, when a long series of cases, investigations or speculative papers have been put forth periodically, possessed of great intrinsic value, or relating to a topic of unusual and permanent interest, it may be proper to collect and republish them in a more enduring shape, but in these days of universal book-making, the inducements should be imperative which provoke any one to add another drop to the already overflowing cup of the medical reader. If the call is urgent, and not to be resisted, the scribe may commit himself to print. His book, however, is not to be treated as a second edition, because its matter has already appeared, but *per contra* to be subjected to even a more careful scrutiny than common, both as to its original worth, and as rechallenging attention after a first hearing. The critic owes it to his public to set up this defence in their behalf.

A large portion of the volume before us is made up of

quotations and extracts from other writers, principally the works of Bartlett and Wood; from the "Practice" of the latter, indeed, the whole of Chapter IV, relating to the pathology of enteric fever, the most important in the book, is taken entire. The author has been careful, however—to do him justice—to acknowledge his indebtedness to these and other sources of information wherever he has made use of them.

The style is diffuse, and the construction not always correct, and occasionally we meet with a very faulty sentence, both as regards grammar and syntax, and, in consequence the meaning is sometimes extremely obscure. We may quote as an example, the following; the author is speaking of the malignant form of typhoid fever: "As already stated, it is not uncommon, after a seeming amelioration of all the symptoms, for sudden and alarming changes to occur, exposing the patient to the dangers of a still severer form, the most remarkable of which was the drying of the tongue after having become clean and moist."

In the first chapter, Dr. Reeves proposes a division of *enteric* fever, as he prefers to call it after Dr. Wood, into three distinct forms:

1st. The SIMPLE or MILD.

2nd. The INTERMEDIATE.

3rd. The MALIGNANT, including the FATAL.

Chapters two and three are entirely occupied with the symptomatology of these forms, and we can justly say of them that in respect to simplicity, clearness, accuracy and fulness, they leave little to be desired. The author has had a good field for observation, and has evidently made the best use of his opportunity and his senses. He is of opinion—so far as his own cases indicated, at least, that the rose-colored eruption, is diminished in proportion to the extent of intestinal alteration. It was always in the worst cases of the disease that he observed the smallest amount of this eruption.

Chapter five treats of the history and causes of enteric fever, and is made up of miscellaneous letters received from physicians practising in the different portions of Virginia.

Its construction is specially faulty. So loose, vague, unmeaning, and unsatisfactory a chapter could scarcely have formed a portion of a "Report on Epidemics" even for the American Medical Association. From a part of this correspondence the author extracts evidence in support of his unqualified belief in the contagiousness of the disease. His personal testimony is singularly inconclusive, and with the exception of a single instance, not one of the examples quoted by himself bear him out in his belief.

That portion of the volume which deals with the duration, complications, terminations, sequelæ, diagnosis, prognosis, and mortality of the disease may be passed over with this simple enumeration. It gives a concise and very reliable account of these accidents, but presents nothing novel or worthy of particular mention.

We cannot refrain, however, from quoting in full the concluding paragraph of chapter eight. As we have already mentioned, Dr. Reeves has accepted, from want of personal opportunity of examination, the description of the anatomical lesions in this disease given by Dr. Wood. It will be seen from the subjoined extract, which needs no comment, to what a unique physiology the author commits himself by his pathological speculations.

"*Nature.*—Concerning the real nature of enteric fever, much difference of opinion has existed; and even at the present day, notwithstanding the uniform teachings derived from a careful examination of the changes found on dissection, opinions still differ. The new inquirer, therefore, is left to select one of two courses; propose a new theory, or adhere, in part or in whole, to one or the other of those already promulgated. I choose to adopt that theory which supposes enteric fever to be the result of a specific poison, by some means introduced into the blood; that in the attempt to eliminate this poison from the blood-current, the glands of the bowels, whose office is assigned to be that of eliminating any putrescent accumulations from this fluid, become overburdened, and thenceforth result in more or less change of structure; that by such change the channels also, through

which nutriment reaches the blood, are more or less obstructed; that in consequence of this the blood becomes additionally depraved; and that these causes, primary and secondary, acting together, are capable of giving rise to the several conditions characteristic of enteric fever."

When we mention, that the concluding third of Dr. Reeves book, is wholly occupied with the subject of *treatment*, the reader must not infer that the author is in favor of much medication in this disease. On the contrary, his remarks on the injudiciousness of over zealous interference, are eminently sensible, and he is satisfied that our increased success in the management of enteric fever is largely attributable to the decreased quantity of drugs now used in its treatment.

"It should be recollected that enteric fever is a disease which cannot be suddenly broken up by any method of treatment, and to attempt to interrupt its progress by active measures is worse than folly: it would be risking the life of the patient. But while this is so, I think much may be done to moderate the violence of the disease, and, in many instances, a fatal termination averted. To be able to do the patient the greatest amount of good, requires close watching on the part of the physician. He should decide upon the 'tendency' to this or that mode of death, and work accordingly, remembering that 'violence in therapeutics is never justifiable when moderation is adequate to the same good end.'"

The therapeutic processes and agents, which have been recommended on good authority, as of use when timely employed are alluded to in more or less general terms, and one or two of the more reliable discussed at some length. The majority of these are of course too familiar to need enumeration; emetics, the favorites of Jackson, purgatives, diaphoretics, and the rest of the long catalogue are all dwelt upon seriatim, and with more or less contingent approval.

The author speaks highly of veratrum viride as a most valuable auxilliary in moderating the violence of the fever, and conducting it to a favorable termination. His method of employing it, is to begin with five or six drops of the

saturated tincture, and repeat the dose every three hours, (increasing the quantity at each repetition by two drops,) until the pulse is reduced to or below the normal standard. The effect is then kept up by the administration of from two to eight drops every four hours *pro renata.* If excessive nausea is produced, it may be relieved by a little sweetened brandy and morphia. In the author's experience, it had no effect on the bowels, and he has never seen any bad consequence follow its administration. Dr. Wood's oil of turpentine is also treated with much favor, he has given it a large trial, and been uniformly pleased with its effects. He is of opinion, however, that the free but discriminating exhibition of *opium* is equally beneficial in subduing all the symptoms for which turpentine is prescribed. His remarks upon the use of opium in this disease, the conditions in which it is useful, and the necessity of making very nice distinctions in this regard, are characterized by great good sense, and indicate a well educated judgment.

The author enumerates among other remedies the nitrate of silver so strongly recommended by the late Dr. J. K. Mitchell of Philadelphia. In many instances, he says, its action is prompt in correcting diarrhœa and tympanites, but when opium was admissible he has preferred to administer the nitrate in conjunction with it. He speaks of it as advised by Dr. Mitchell for the wasting diarrhœa of the *last* stages; but if we recollect aright the oral lectures of this distinguished and much lamented gentleman, he dwelt specialy upon the importance of administering it daily throughout the *entire course* of the desease, without reference to the other remedies employed, with a view to *prevent ulceration* principally.

The work closes with a review of the author's personal method of treating the three forms of enteric fever, and some remarks on the management of the convalescent. In the main these are quite judicious, but we must take exception to the permission he gives his patients to partake, early in convalescence, of some of the fruits—the orange, apple, strawberries, mulberries and grapes—when freed of the skin and seeds. Aside from the acescent character of these arti-

cles, the condition specified is one rarely if ever completely fulfilled, and we think it preferable to debar the patient this temporary gratification, than allow him to run the risk of losing his life by accidentally swallowing an orange or grape seed, when his intestines may still present an ulcerated condition. We are aware of one case in which such an accident proved promptly fatal by opening a large vein immediatley beneath an ulcer which was rapidly healing; the patient sank from hæmorrhage in a few hours.

ART. IX.—*Practical Remarks on Yellow Fever, having special reference to the Treatment.* By EDWARD JENNER COXE, M.D., Visiting Physician to the N. O. Charity Hospital, etc., etc. J. C. Morgan, New Orleans, 1859. Pamphlet. pp. 108.

The author of this brochure, tells us, in speaking of his qualifications as an instructor in regard to the treatment of yellow fever, that he had been forced by ill health to engage in the drug business, for sixteen years, and enjoyed his *first* opportunity of seeing the disease on a large scale, when he took charge of Dr. Fenner's wards during the absence of the latter in Norfolk in the summer of 1855. Since that period he has had two wards under his medical charge in the Charity Hospital, and from his observation and experience there confidently affirms that "the yellow fever ought to be more curable than is found to be the case."

The desirableness of this therapeutic advance no one, we suppose, will deny, but it is certainly questionable, on Dr. Coxe's own statement, whether he himself has done much to further it. In 1858, 198 cases of well marked yellow fever were brought into his wards, and of this number 117 were cured (?) and 75 died; a mortality by no means small, when compared with hospital statistics, any where, and terrible if

contrasted with the infinitely higher success met with in general practice.

The whole of the first part of this pamphlet is made up of copious extracts from Drs. Bennet Dowler, La Roche, and other writers, and the object of it to prove that blanketing and sweating patients, confining the air in their rooms, and neglecting their personal cleanliness—which, according to the author, seems to be a favorite mode of practice in New Orleans—are processes calculated to aggravate the severity of their attack, and increase its fatality. Facts which we suppose no sensible physician doubts, and which scarcely need demonstration at this date.

The *second part*, which details the author's method of treatment, is also enriched by large quotations from Dr. La Roche's book. The treatment consists in the use of purgatives, followed by acidulated and diaphoretic drinks in ordinary cases—the use of emetics when the stomach is primarily affected, and blood-letting with revulsives, if the head symptoms are prominent. The diet is restricted to a *very rich* (the italics are the author's) solution of gum-arabic, with or without flaxseed tea. A solution of quinine in a chlorine mixture was found of great benefit, when used as the basis of the more active preparations taken internally.

We have not space to speak of the adjoined cases, or remark at any length upon the looseness of method and logic, the bad style and general want of tact which characterize Dr. Coxe's paper throughout. The utmost value we can concede it is, that if widely read it may direct popular attention to the value of fresh air, personal ablutions, tranquility of mind and quiet of body, useful adjuvants in yellow fever, as well as in all other diseases.

ABSTRACTS AND TRANSLATIONS
FROM FOREIGN AND DOMESTIC JOURNALS.

Typhoid Fever in Children. From the Second Edition of the Traité des Maladies des Enfents. By MM. F. RILLIET and E. BARTHEZ. Translated for the Charleston Medical Journal and Review. By DAVID E. EWART, M.D., Newberry, S. C.

[Continued from Vol. XIV., No. 5, p. 653.]

ART. VI.—PROGNOSIS.

The distinctions which we have made between the different forms of typhoid fever will aid us in forming our prognosis. It is perhaps unnecessary to repeat that the first form terminates in a return to health in a very large majority of cases. Yet we think it incumbent upon us to verify this conclusion, since it differs from what is observed at a more advanced period of life. It is well known that typhoid fever in the adult, though apparently benign, sometimes suddenly terminates in death, this accident being caused by intestinal perforation. In the child, this complication, besides being rare, seldom ever takes places except in grave cases. Thus, in collating our cases in which there was perforation of the bowels, with those observed by M. Taupin, we find that out of five patients, it only once occurred in a light case; this case was in M. Barrier's practice. At the hospital, only one out of forty-seven patients attacked by this form of the fever died, and this was from a complication entirely independent of the primary disease; the patient was seized, during convalescence, with varicella complicated with thoracic symptoms. In our private practice, all our patients recovered.

The grave form, though more dangerous, usually terminates favorably, should the disease follow its ordinary course and no complication arise. Yet, as we have already seen, this form, by favoring the development of certain diseases, always renders the prognosis serious. In examining separately all the cases which come under this head, we find that

nearly all succumbed to complications; the youngest to severe enteritis, intestinal perforation or eclampsia; the oldest to pneumonia, pleurisy, pleuro-pneumonia, or eruptive fevers contracted during convalescence. It is beyond doubt that many of these diseases were favored in their development by the circumstances in which the patients happened to be placed, only in one case did the intensity of the intestinal lesion appear to have been the principal cause of the fatal termination. This occurred in a girl of seven years of age, who died on the eighteenth day of a typhoid fever, which was between a grave and a light form, and in which we discovered no other lesions than those belonging to typhoid fever, which were, in this exceptional case, very intense.

Very grave typhoid fever is always quite dangerous, and in this form also, death is often hastened by the development of a complication; though the disease itself, free from all intercurrent lesion, may terminate fatally. In the latter case, death is generally the result of a profound perturbation of the nervous system. Very grave typhoid fever is in fact nearly always ataxic or ataxo-adynamic, and most cases in which the ataxic character predominates, die. It is impossible in these cases, to attribute death to the extent of the intestinal lesions, as they have sometimes disappeared, although the worst ataxic symptoms may have been observed up to the day of death.

At the hospital we lost a fourth of our cases, but in private practice scarcely a tenth. Though if we leave out of our list of fatal cases, such as having been cured of typhoid fever, died from purely accidental complications occurring during convalescence, our sum of mortality in hospital practice will be much less.

We do not think it necessary to enter upon any lengthened account of the dangers of the different kinds of complications, for we have already mentioned them in a previous article. In order to point out with greater precision the rules which are to guide the practitioner in his prognosis, we will successively revert to the different symptoms in respect to their gravity.

The symptoms furnished by the digestive functions, will be of little service in forming our prognosis. Diarrhœa is as abundant in the cases that recover as in those that die. Though a long continued diarrhœa in children of two and three years of age, is of unfavorable augury; it is debilitating, and apt to bring on death by adynamia. Involuntary stools are met with less frequently in those children who recover, than in those who die; yet, as we have already

stated, their prognostic value is not so great in the child as in the adult. Great distention of the abdomen is a very serious symptom, but a permanent retraction, accompanied with a high grade of fever, presents still greater gravity; the rose-colored spots, sudamina, gurgling in the iliac region, and enlargement of the spleen, have never appeared to us to be of any service in forming a prognosis. Vomiting is more frequently seen in subjects who die than in those who recover. The condition of the tongue is very variable, whatever may be the termination of the disease; in bad cases it is generally encrusted, fuligenous and dry. If the tongue remains moist during the whole course of the disease, it is ordinarily a favorable prognostic symptom; though this is not always the case, for in some rare cases in which the disease ended fatally, the tongue continued moist throughout the disease.

The nervous symptoms are of great value in forming a prognosis. A persistent and intense delirium is a very alarming symptom; somnolence is less so, but the loss of sensibility and intelligence are very unfavorable. We have observed the latter on the evening preceding death in those cases which died on the seventh day. Carphologia is a very serious symptom; we have observed it in six children who died, and in one who recovered.

Rigidity of the body has only been mentioned in connection with cases followed by death. It is an unfavorable symptom for the head to be retracted, though it is not so much so as convulsions, which are nearly always fatal. Subsultus-tendinum is a less unfavorable prognostic symptom; we have seen it very bad in one case that recovered. The condition of the pupils cannot be of much service; their contraction or dilatation, though peculiar to grave cases, is met with just as frequently in those who recover as in those who die. A discharge from the ears is not an unfavorable symptom; we noticed it only once in a patient who died.

Sloughs of any size upon the sacrum seldom ever occur, except in fatal cases.

The intensity of the fever is a less serious symptom than certain particular conditions of the pulse, as its irregularity and smallness; a small, thready pulse is generally a bad symptom.

After having endeavored to determine the prognostic value of the different symptoms, we should now examine the influence which the seasons, different years, and the prevailing epidemic constitution may have upon the gravity of the disease. It would be impossible for us to answer these ques-

tions from the facts which we alone have collected; but by adding to our observations, those gathered from various medical journals, we are enabled to say that the seriousness of the disease varies in different years, and that, consequently, it is well, before forming a prognosis, to carefully examine what has been the effect of the epidemic constitution upon recently observed cases.

As complications are more readily developed in the youngest subjects, the disease should always be considered as very unfavorable in children under five years of age. Girls appear to be less apt to have the grave form than boys.

Art. VII.—Causes.

Nearly all the causes are predisposing. In the first place we will mention the influence of age.

Typhoid fever is especially frequent between nine and fourteen years of age, less so between four and eight, and is rare during the first years of life,* being proportionally more so as the period of birth is approached. The different cases published in various medical journals, have all, excepting one (a child of four years of age) been collected from subjects between eight and fourteen years of age. In our first edition, we regarded the proportion given by M. Taupin as an exception, when he says that in 121 patients, he has observed typhoid fever ten times in children of four years of age. Since then, however, we have seen the disease attack children of this age in a pretty large proportion, (about a sixth).

There is, no doubt, according to facts we have elsewhere published, but that dothinenteritis may occur at the age of two years, (see *archives* 1840). Are there to be found in the history of medical practice examples of typhoid fever occurring in children less than two years old? In our history of the disease, we will cite two observations of very young children, (one twenty-four days, the other thirteen months,) which Billard regarded as two examples of follicular enteritis (typhoid fever); in these cases, however, this physician unfortunately did not note the condition of the mesen-

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*Age.	Number.	Age.	Number.
2 years	3	9 years.	13
3 "	2	10 "	10
4 "	4	11 "	14
5 "	5	12 "	13
6 "	9	13 "	13
7 "	2	14 "	13
8 "	7	15 "	3

teric ganglions, and very imperfectly described the alteration of the plates.

To these two cases we might add others furnished by Dr. Abercrombie. These occurred in children of six or seven months old; the lesion of the plates and ganglions is already shown. "The intestines," says the author, "presented a healthy appearance on their exterior, excepting some superficial spots of discoloration; on the internal surface of the small intestines, there were in many points, circumscribed spots of discoloration, covered with small ulcerations. These spots were whitish or ash colored, and of a honey-combed appearance; they were slightly elevated above the surrounding parts, those of the interior of the intestine corresponding with those of the exterior; the mesenteric ganglions were tumefied, but the other viscera were healthy."

One of us, M. Rilliet, saw at Geneva, four children, two of seven months of age, one of ten months, and one of thirteen months, affected with well marked typhoid fever. In three of the cases, the disease was developed under the influence of contagion. Two of them recovered, the other two died, and upon examination, the lesions characteristic of dothinenteritis were discovered.

M. Charceley has published in the *Journal de Tours* some cases of typhoid fever observed by him in new-born infants.

Sex.—The influence of sex is quite evident; boys are much more subject to the disease than girls.*

Constitution.—We have just seen that though typhoid fever can occur in every stage of childhood, it principally attacks these who are in the vigor of age, (if we are allowed the expression); it also seems to select those of a good constitution and well developed subjects having firm flesh, brown skin, auburn hair, and dark eyes. Should it attack blonds, they do not generally possess the characteristics of the lymphatic temperament, such as soft cheeks, white transparent skin, and long eye-lashes, but on the contrary, they are very robust.

Anterior disease.—It is very seldom that we see typhoid

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* In our 111 hospital cases, we had 80 boys and 31 girls; a proportion almost exactly similar to that obtained by M. Taupin, who in 121 children, saw the disease 86 times in boys, and 35 times in girls. In our private practice, we have remarked the same thing; more than three-fourths of our cases occurred in boys. When typhoid fever rages epidemically, it may, according to the locality which it invades, exclusively attack either boys or girls. Thus, whilst our confrere, Dr. Dufrene observed, in 1845 typhoid fever attack two thirds of the children in a female boarding school, one of us, M. Rilliet, saw it in 1847 exclusively occur in boys at a country school.

fever occur in children debilitated by previous disease; for whilst in our hospital wards we are constantly seeing one cutaneous disease succeed another, and one phlegmasia another phlegmasia, we have never noticed typhoid fever developed under similar circumstances, except perhaps in the case cited at page 692. In our private practice also, the disease has always been primary.

Hygienic causes.—A considerable influence has been attributed to antihygienic causes in the production of typhoid fever. Thus, improper food, a filthy, badly ventilated, damp dwelling, insufficient clothing, and neglect of personal cleanliness, it is said give rise to it. We are not disposed to admit that these causes have so great an influence; for most of the patients brought to the hospital, belonging to the poorer classes of society, are nearly all placed in the same conditions; and besides, in private practice do we not see typhoid fever become developed in subjects placed in the most favorable circumstances? The only positive influence that antihygienic causes exercise, is that of aggravating the disease; indeed, hospital cases are always more unfavorable than those occurring in private practice, and these again are less so in proportion to their ease of circumstances.

Acclimation in the adult, plays an important part in the production of typhoid fever; is it not also the case in children? We will here make some statements which may throw some light upon this question. We have been able to note exactly the time of arrival at Paris in eighty-one of our patients at the child's hospital; twenty had been in the city less than six months; nineteen, six months or a year; nineteen, one or two years; nineteen, more than two years; and four were brought to the hospital from a commune in the neighborhood of Paris. In summing up this statement, we find that acclimation exercised some influence upon twenty out of eighty-one cases.

M. Taupin, with a somewhat different proportion, in his statements, has come to the same conclusion. He besides noticed that twenty children placed in the class of those recently arrived at Paris, had experienced, from their first arrival, a very notable influence from their change of circumstances, it displayed itself in twelve by transient but considerable increase of appetite, in eight by epistaxis, diarrhœa and emaciation.

Seasons—Epidemics.—Recent facts obtained by us accord with the conclusions to which Drs. Marc d'Espine and Lombard have arrived, viz., that autumn is of all seasons, the one which most predisposes to typhoid fever. The three epi-

demics which have specially attacked children in the canton of Geneva, have all occurred in autumn. Winter is the next season in which the disease most frequently occurs; and spring and summer those in which it is least frequently met with. In Paris, from the month of April to the month of September, in 1841, we saw a considerable number of cases of typhoid fever in the department for boys in the child's hospital. Since then, one of us, M. Barthez, has witnessed a certain number of epidemics of the disease both in autumn and spring. But those observed in the latter season, were less numerous and lighter than in the former.

Sometimes, in small localities, typhoid fever exclusively attacks children, whilst it almost entirely spares adults. One of us witnessed about the close of the year 1840, in a village situated two leagues from Geneva, an epidemic in which children alone were attacked; it lasted for three or four months. The disease was of a benign character, and all the cases recovered; whilst in an adjoining village, the adult population was decimated by an ataxo-adynamic dothinenteritis.

M. Taupin has made investigations to ascertain whether the depressing passions, nostalgia, dentition, intestinal worms, onanism, or laborious occupations, might not be regarded as predisposing causes of typhoid fever; but his conclusions in this respect are negative. Our facts, though less numerous and less detailed in this particular, exactly conform with those of M. Taupin.

The predisposing causes then may be reduced to: 1st, a very notable influence of age, sex, temperament, acclimation, and epidemic constitution; 2nd, a more doubtful influence of hygienic circumstances. Most of the other causes admitted by authors possess no influence.

Can typhoid fever be developed in children in consequence of an appreciable occasional cause? Though in some few cases we have had the parents of our young patients to attribute the disease to fright, or to a sudden cold change in the weather; though M. Taupin has seen the access of the fever coincide with the healing of a phlegmon, or the suspension of a purulent discharge from the glands of the neck, we cannot see any thing more in these facts than simple coincidences, and not the relation of cause and effect.

Contagion.—But there is another direct cause which demands our attention: Is typhoid fever contagious? The facts collected by us are not sufficient to enable us to give a clear and positive answer to this question. In four cases observed at the hospital, we noticed that the fever occurred

in children whose brothers or sisters were either at the time, or just previously, affected with the disease. We have seen this fever originate in the hospital only in four cases, but never in the wards in which patients attacked with it were lying under treatment. Let this result be compared with that to which we arrived in respect to eruptive fevers, and it will be seen what a great difference there is between dothinenteritis and these diseases as regards contagiousness. We are however aware that very plain cases of transmission of typhoid fever from one child to another, have been cited by some authors, and we ourselves have seen some facts of this kind which were apparently very convincing. So without denying the influence of this cause, we will merely affirm that it is less evident and less frequent than in many other diseases.

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Treatment of Nervous Headache by the Hydrochlorate of Ammonia. By Dr. A. BARRALIER.

The author recommends the hydrochlorate of ammonia as the best therapeutic agent in cases of nervous headache, and especially in idiopathic cephalalgia and migraine. For upwards of three years he has employed it with success 202 times out of 259. The salt is administered in the form of potion: distilled or mint water, 60 grammes; hydrochlorate of ammonia, 3 grammes; syrup of orange peel, 25 grammes; taken in three doses at half an hour's interval. These doses do not produce any evident physiological effects in the healthy condition; but, when administered during a paroxysm of nervous cephalalgia, their effect is manifested with great promptitude. Generally, after the first dose, the pain abates and the pulse rises, and a gentle perspiration relieves the dryness of the skin. The influence on the circulation is so great that the pulsations, which were under 50 during the paroxysm, rise above 70 after the first dose. The headache, which is calmed by the first dose, diminishes and entirely disappears during the second and third. An important circumstance is, that the sal-ammoniac does not develop its curative action except when the pain is at its height; at the commencement of an attack, the potion has only a slight effect, but when the sufferings of the patient are very intense, the medicine acts with wonderful promptitude. Besides the temporary relief, it was wonderful that, in cases of headache returning in periodical paroxysms several times a

month, the intervals gradually became longer, the attacks diminished in intensity, and ended by disappearing completely after having been several times arrested by the ammoniacal potion. To obtain success, however, it must be administered in certain cases, and according to precise indications. The results of the author's observations are that the potion of hydrochlorate of ammonia has almost constantly dispelled attacks of idiopathic hemicrania or migraine, and of migraine succeeding menstruation more abundant than usual. It has no effect in relieving attacks of hemicrania depending on irregular or suppressed menstruation; it has given pretty good results in cranial pains depending on functional disorder of the stomach, and in accidental nervous cephalalgia; and it has been successful in relieving headaches consequent on repeated attacks of intermittent fever, those occurring in the decline of low fevers, and in the period of irritation in typhus.—*Bulletin Gen. de Thérapeutique and Virginia Medical Journal.*

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Trousseau on the Diagnosis and Treatment of Hepatic Colic. Professor Trousseau lately directed the attention of his clinical class, at the Hotel Dieu, to the frequency with which cases of hepatic colic are mistaken for other affections. Although, in its severe form, hepatic colic is readily recognized, yet a slighter form, which is more common, especially in women, is very generally ignored. The reason is, that the pains caused by the small calculi are felt principally in the epigastrium, from which they radiate through the abdomen, the chest, the back, and sometimes even down the thighs. The practitioner, accordingly, is very apt to refer the pain to other organs than the liver, and, in particular, to ascribe it to a spasmodic affection of the stomach. This opinion appears, in many cases, to be further confirmed by the presence of vomiting.

M. Trousseau illustrated these observations by two cases, both women, of sedentary occupation, from 40 to 50 years of age. In the case of the first, pain in the epigastrium, with vomiting of matter not containing bile, occurred two days before her admission into the hospital. The severe pain was succeeded by a feeling of langour and fatigue. The fæcal matters passed by this woman were collected, washed, and carefully examined, when a calculus of the size of a pea, consisting of cholesterine, was found. The second woman had been subject to "cramps of the stomach" for several

years. These had been treated without success. When she came under M. Trousseau's care, she had been suffering for several days from attacks of severe pain starting from the epigastrium, and darting down through the belly into the right flank and into the back. These attacks came on two or three times a day, and lasted for from half an hour to two hours. On the 12th of March the pains continued for five hours, on the 13th for eleven hours; they were accompanied by non-bilious vomiting, and on the evening of the last mentioned day they ceased suddenly, and were succeeded by slight shiverings and well-marked jaundice. The bowels having been confined for some days, a purgative was administered; the fæcal matters were washed, and five calculi with polished facettes were discovered.

Before alluding to the treatment, M. Trousseau entered somewhat minutely into a consideration of the symptoms in these and similar cases. He laid it down as a general rule, that if, in addition to pains of the character described above, there be vomiting of matters not colored with bile, the symptoms depend upon the presence of a calculus in the common duct; and that, ninety-nine times out of a hundred, the presence of bile in the urine will next day confirm the diagnosis. It must, however, be borne in mind that, although the absence of bile in the matters vomited, and the subsequent occurrence of jaundice, entitles us to give a positive diagnosis, the opposite circumstances—namely, the presence of bile in the vomited matters, and the absence of jaundice—do not justify us in absolutely denying the existence of hepatic colic. In most cases the jaundice is so slight as to escape the attention of the patient; indeed, generally it is only by the condition of the urine that its existence is revealed. In addition to pain, vomiting, and the presence of bile in the urine, another diagnostic means is at our disposal. This consists in an examination of the fæcal matters; for this purpose, the evacuations are to be collected from the time of the cessation of the attack during the next three or four days, as a calculus may occupy this time in passing from the duodenum to the rectum. The matters so collected are to be washed in a hair sieve, until the solid matter is completely broken down. Under any other way of proceeding, the calculus might escape detection. It does not always happen, however, that the calculus, the cause of the suffering, is discharged. It not unfrequently happens that there are concretions in the gall-bladder which are too large to pass along the cystic duct. In such cases, the bodies get partially impacted in the duct, and give rise to much suffering; but on

their going back into the gall-bladder, the relief is as complete as when they drop into the duodenum. In these circumstances, we are glad to be guided in our diagnosis by the sudden appearance and disappearance of the symptoms, taken in connexion with the absence of symptoms pointing to disease of other organs.

With regard to treatment, nothing can be done to get rid of calculi already existing; onr attention must, therefore, be limited to the treatment of the attack of hepatic colic, and to preventing the formation of fresh calculi. The treatment of the attack is not very satisfactory, and often the remedies employed do more harm than good. Thus many practitioners prescribe opium; but as opium checks all the secretions, with the exception of the secretion of the skin, it is unfavorable to the accumulation of bile in the gall-bladder, which acts beneficially in helping the expulsion of the calculus. The pain, it is true, is deadened by opium, but the expulsion of the calculus is delayed. The relief of suffering is, according to M. Trousseau, more advantageously obtained by chloroform or sulphuric ether given internally, while frictions with an alcoholic extract of belladonna, reduced by water to the consistence of a syrup, are made over the seat of pain. The ether is generally given in the form of capsules, each of which contains eighteen drops; in this way a considerble anæsthetic action is produced, while the biliary secretion is not interfered with. Prolonged hot baths sometimes give relief, though in general they produce little effect. When the attack is past, M. Trousseau generally gives, with a view to improving the digestion, seven or eight pills daily, each containing three grains of extract of ox-gall. The real treatment of these cases, however, is prophylactic. As above stated, M. Trousseau thinks we cannot act upon calculi already formed, although M. Barth supposes that, by a vegetable diet, combined with the use of alkalies and turpentine, biliary concretions may be disintegrated. All we can hope for, therefore, is to prevent the enlargement of existing calculi and the formation of new ones; and this we must do by impeding the production of cholesterine. Now, as cholesterine is a fatty substance, we must, in order to diminish its quantity, diminish all the fatty matters of the economy. For this purpose we must look to alkaline substances, which saponify the fats and render them more soluble; to exercise, which promotes their combustion; and to an alimentation which shall contribute as little as possible to their development. Peyrilhe, the first professor of Therapeutics in the Faculty of Paris, noticed that, in large herbivorous animals

such as oxen and cows, which are very subject to hepatic gravel, the gall-bladder was filled with calculi from the month of April to the month of June, and that it ceased to contain any from the beginning of September until the end of December. He thought that the production of cholesterine was favored in these animals by a dry alimentation, consisting in part of oleaginous grains; while, on the contrary, the chlorophyl, or green coloring matter of plants, acted as a special solvent of this fatty matter. Peyrilhe was right; but he had not noticed another important element in their cases—the influence of prolonged repose. In man, a sedentery life acts in the same way as the stalling of oxen; and this is the reason why women are more subject to biliary calculi than men. Green vegetables, though acid, make the urine alkaline; they possess the property of saponifying fats; and you have thus, in exercise on the one hand, and in the use of green vegetables or alkalies on the other, the principal elements of the prophylactic treatment of biliary calculi. To patients suffering from this affection, we should, therefore, continues M. Trousseau, prescribe walking exercise, a regimen having for its basis green vegetables, to which may be added lean meat and fresh fruit thoroughly ripe. The patient should abstain from eating oleaginous matters, such as fat meat, butter, oil, milk; he should also partake sparingly of amylaceous or gelatinous matters, which, on account of the large proportion of carbon they contain, have the greatest analogy with fat. We may at the same time recommend the use of alkaline mineral waters, such as those of Vichy, the Mont Dore, etc. Not that these are to be given so as to saturate the system with the mineral principles of the waters, but only in such a way as to improve the general health. They should not be given continously for too long at a time, but their use should be interrupted and again resumed.—*Journal de Médecine et de Chirurgie Pratiques*, and *Edinburgh Medical Journal.*

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Do Bad Smells cause Disease?—The tendency of the human mind to rest satisfied with any belief that is authoritatively asserted, is too well known to require any comment. Philosophers of all kinds are no more exempt than other people from this easy style of dealing with difficult problems. Medicine is, we think, especially chargeable with cherishing pet answers to questions that force themselves unkindly on her; and we think that the way in which she has made up her

mind as to the causes of various kinds of fevers is an example of this style of cutting the Gordian knot.

Of late years, it must have struck all our readers that pig-styes, dirty pools of water, open privies, ash-heaps, etc., have been declared highly criminal, and on all occasions even adjudged guilty of producing any kind of fever or bowel complaint that may have broken out in their neighbourhood. If a child happen to suffer typhus in a farmhouse, it is the mixen at the end of the barton that caused it. If an epidemic of English cholera befall a village, it is traced to the duck-pond by the road-side. If in a wealthy household the inmates are stricken with diphtheria, some open sewer close at hand has, as a matter of course, been the cause. So accustomed are we to hear this sort of reasoning resorted to on all occasions, that one feels a little difficulty in expressing doubts as to the certainty with which the effect is thus unhesitatingly traced to its cause. Nevertheless, we think there is at least sufficient evidence to cause reflecting minds to pause ere they give in their adhesion to the general opinion, and thus shut their eyes to further research and inquiry. Dr. Watson has, we know, stated it as his distinct opinion "*that neither animal nor vegetable decomposition is sufficient to generate fever of any kind*"; and the researches of Dr. Guy and other observers have certainly gone some way to support that opinion.

Dr. Guy, in his very interesting contribution to the *Journal of the Statistical Society*, on the Health of Nightmen, Scavengers, and Dustmen, gives us a mass of statistical facts which, it must be confessed, run counter to the generally received opinion, that foul animal or vegetable emanations are the fruitful source of disease. This class of men whithout doubt spend their days in the very midst of filth of all kinds. He says:—

"In most of the laystalls or dustmen's yards, every species of refuse matter is collected and deposited—night-soil, the decomposing refuse of markets, the sweepings of narrow streets and courts, the sour smelling grains from breweries, the surface-soil of the leading thoroughfares, and the ashes from the houses."

This heterogeneous mass the scavengers or "hill" people have to sort or to pass through sieves, so that the emanations arising therefrom must be brought into intimate relation with their lungs and skin. If fever and diarrhœa are so clearly traceable to the vicinity of these so-called noxious materials, surely the scavengers ought to be a poor fever-stricken race. A medical examination, however, of this class of workmen as compared with brickmakers and bricklayers' labourers, proves

that the scavenger is comparatively exempt from disease. Thus, among a number of men examined in each of the three classes, it appeared that the numbers attacked by fever were, among the scavengers, 8 per cent.; among the bricklayers' labourers, 35·5 per cent,; and among brickmakers, 21·5 per cent.

This result seems extraordinary enough; but it may be argued that these men do not live in the laystalls or dust-yards, and therefore that their exemption from fever may be attributable to this; but what can be said, if the dustmen and their families, who live all their lives in the midst of these heaps of so-called fever-nests, are healthy? Dr. Guy says:—

"I do not think that, whether in town or country, such another body of men (as master dustmen) could be brought together exept by selection; and it is not going too far to assert of them that, if the comparison were limited to the inhabitants of London, or our large towns, no score of selected tradesmen could be found to match the same number of scavengers brought casually together."

Unless we suppose that the scavengers get used to this so-called miasmatic atmosphere, or that after a time it no longer affects them, we cannot see how the foul emanation theory can hold water. Nature cannot work in one place differently from another. Nightsoil must be just as deadly in an open yard in London as in the country. But here we have the experiment tried on a larger scale, of a whole class of men subjected to foul emanations, and yet they are far from being an unhealthy race, and are not nearly so prone to fever or bowel-disease as the brickmakers' labourers.

We are far from wishing it to be understood, however, that we do not consider foul emanations as dangerous or baneful under any circumstances. In our opinion, they become noxious when much concentrated. Our houses, for instance, are built on the principle of a bell-glass; and our drains and privies, and all other impurities, if allowed to give off a deleterious miasma, most certainly do become most virulent sources of disease. But, in the open air, we think it very doubtful whether these emanations are ever the cause of injury to man.

Let us watch with Dr. McWilliam a still more gigantic experiment on the health of the Thames waterside people, which has been going on for years, and is still proceeding. The whole sewage of two millions and a half of people has within the last ten years been turned into the metropolitan stream. Year by year its waters have become more conta-

minated, and its smell more disgusting. It should follow, that the health of the waterside community is proportionately decreasing; that febrile complaints, cholera and diarrhœa, are alarmingly on the advance. But what is the real state of the year 1858 on the health the Water Guard and Waterside Officers of Her Majesty's Customs, says:—

"As respects bowel affections, in which I include diarrhœa, choleraic diarrhœa, dysentery, etc., the types of those forms of disease, which in this country noxious exhalations are commonly supposed to originate, we find the additions during the four hot months of the past year from this class of complaints 26·3 below the average of the corresponding period of the three previous years, and 73 less than those of 1857."

The quantity of putrescent animal and vegetable matter in the Thames has been going on increasing; but the illness generally attributed to the emanations arising therefrom has been decreasing! We know that many will urge that all the combustibles (if we may use the term) being thus accumulated, it only requires the match to be applied, to find epidemics raging like wildfire. But the year before last, cholera did break out on the banks of the Lea, and there died out, apparently from want of sustenance. This year, according to the *Lancet*, cholera, veritable Asiatic cholera, has been on board the *Dreadnought*; yet it has not spread, and there seems no likelihood of its doing so for this season at least. As Dr. McWilliam truly says, "It is nowhere sustained by evidence that the stench from the river or docks, however noisome, was in any way productive of disease." It is true that one waterman, in June last, was said to have died of Asiatic cholera, and that his death was ascribed to river-poisoning; but, as the eminent observer whom we have just quoted correctly remarks; "it is opposed to all analogy, and to the usual order of nature, and therefore, entirely unphilosophical, to suppose that a cause so extensively diffused should have been so singularly limited in its effect."

Greatly doubting, as we do, the alleged ill effects of foul emanations in the open air upon human life, we nevertheless do not think that the crusade against filth should for one moment be relaxed. A bad smell may be no more unhealthy than a bad taste; but we should, if possible, avoid the one as much as the other. What we should above all things avoid, however, is the falling into the error of supposing that bad smells are the indubitable sources of many puzzling diseases, and of thus hardening our minds against investigations of the kind which were instituted a year or two ago by Dr. Barker, and which, when completely carried out, will enable us to

decide what the noxious principles are which make all the difference between an unpleasant and a malarious odour.—*Brit. Med. Journal.*

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Treatment of Tetanus by the Woorara Poison.—M. Vella of Turin, arguing from the fact shown by M. Bernard in 1850, that the woorara poison is a direct sedative of the motor nerves, undertook a series of experiments which clearly showed the antagonism between strychnine and woorara. Being appointed to the French Military Hospital of Turin during the late campaign, and seeing several cases of tetanus which had resisted opiates, ether, &c., M. Vella resolved to try woorara. The first trials were made upon two patients who had been suffering from tetanus for four and five days respectively, in consequence of gunshot wounds. They were both in a semi-asphyxiated and desperate state. The woorara produced a general relaxation of the muscular system, whereupon the patients felt much relief; but they both died. The same treatment was, however, employed upon a third patient, who recovered. He was a sergeant, 35 years old, tetanic from a gunshot wound of the foot. Two grains of woorara were dissolved in nine drachms of water, and compresses moistened with the solution were applied to the wound; the strength being gradually increased to fifteen grains in fourteen drachms of water. For the first four days the compresses were renewed every third hour; afterwards every fifth hour, up to the twelfth day, when the changes were reduced to three and two in the twenty-four hours. In twenty-dwo days the patient could leave his bed, and returned to France thirty-six days after the first application of the woorara.

SURGERY.

Necrosis of the Lower Jaw from the fumes of Phosphorus; complete removal of the Bone, followed by recovery. (Under the care of Mr. HOLMES COOTE, at St. Bartholomew's Hospital.

Fortunately for us, in this country we do not see so many of the terrible effects arising from the employment of phosphorus in lucifer-match making as are witnessed in France and Germany, where such manufactures are most extensively

carried on. Necrosis of the lower jaw is, perhaps, the only special result which comes under the notice of the English surgeon, and which does not appear until the teeth first become affected. Diseases of the lungs, in consequence of the manufacture, are with us almost unknown: whilst in Paris and elsewhere many of the workwomen have to leave their employment from what were looked upon as *neglected colds*, terminating in consumption, but which were, in reality, the result of the inhalation of an atmosphere, which Dr. Dupasquier found to consist principally of hypophosphoric acid, probably mixed with small quantities of phosphuretted hydrogen, and possibly the phophorus itself in the form of vapor. The breath of these people at night becomes luminous. The valuable Report of Dr. Waller Lewis, presented to both Houses of Parliament in 1855, "On the regulation of Noxious Trades and Occupations in France," very clearly shows that bronchitis, often in a very severe form, is a common affection amongst many of the workpeople, and, in proof of this, he furnishes abundant evidence from various sources, as well as from personal observation.

Now, although in this country we are free from the worst phases of the disease, it still behoves manufacturers to consider whether, in place of phosphorus, some other chemical substance could not be introduced which would produce the igniting effects desired, without the sickening and unpleasant alliceous odor which invariably arises from this element. Such matches are now being manufactured in Paris by M. Canouil. Amongst other varieties, they are made of chlorate of potass, powdered flint or glass, bichromate of potass, gum or dextrin and water, made into paste, A simple friction produces combustion; there is no unpleasant smell, because there is no phosphorus, and, what is of considerable importance, their manufacture is not injurious to the workpeople. Much misery and suffering are therefore obviated amongst those who are employed in the factories.

In a former "Mirror," when placing upon record an instance of necrosis of the lower jaw, arising from the fumes of phosphorus, in which Mr. Thomas Wakley removed the bone with success at the Royal Free Hospital, we devoted some attention to the consideration of this subject, and we urged the propriety of the periodical examination of the teeth of those employed in factories where it was used, so that the spread of its destructive influences might be arrested. We referred to several other cases which had appeared in our "Mirror;" but Mr. Wakley's was one of the most remarkable

ceased entirely. His health now began to improve, under the use of a liberal diet; his appetite increased, and his strength returned. The investing periosteum of the old bo now began to throw out fresh osseous material, and lower jaw was in the process of formation, as has noticed in other and similar instances. The pasty ap ance of the face, and the puffiness of the cheeks, howe remain and these would seem to be almost the perman consequences of the affection. He left the hospital a sh time back, completely restored in health, and able to spea and articulate with tolerable distinctness.

On examining the bone when cleaned and dried, we found it to be massive, and of nearly double the weight of the healthy bone. It was covered in some places with unhealthy lymph, undergoing osseous transformation.

Ten years ago, there was a patient in this hospital, under Mr. Stanley's care, who was a fellow-workman in the same factory, and whose lower jaw was affected with necrosis arising from the same cause.—*Lancet.*

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Extraction of a Thermometer-Tube from the Urinary Bladder.—An immense variety of foreign bodies have been found in the bladder, both of males and females. Hair. beans, fruit-stones, ears of corn, portions of bougie, nails, bullets, small bones, pins, needles, string, stalks of flowers, &c. &c., are amongst the substances enumerated by Mr. Coulson, in his work on the Bladder (chap. XIX.), as having been extracted from this viscus. If allowed to remain, they easily form the s of a stone. On the 22nd of June we witnessed the n of a thermometer-tube from the bladder, at St. Hospital. The patient was a young man, aged wo, admitted under Mr. Coulson's care on the 20th He had for many years been subject to nocturnal ons, and, after a long course of doctoring, he resorted e expedient of introducing a glass thermometer-tube the urethra and into the bladder, as a species of im- mptu bougie. He accomplished the introduction success- ly, although not without difficulty; but on attempting its al the glass tube broke and a piece remained in the presence did not cause any considerable inflam- fered little pain, and micturated without diffi- was no urgency to pass urine; but after the mptied he experienced pain in the peritoneum, acute character. There was a slight admixture

we had seen. The patient has since given up his old employment.

Besides necrosis of the jaw and diseases of the lungs, which arise from phosphoric vapors, it has been ascertained by M. Moignot that the women employed in the manufacture of lucifer-matches are very liable to miscarry, and so fully aware have the workmen become of this peculiarity that advantage has been taken of it to procure abortion.

In the case which we now record, the disease did not appear until the gums in contact with the diseased teeth became ulcerated, which produced pain and swelling of the jaw, with ultimate complete death of the entire bone. It is somewhat remarkable, that one condyle, together with the coronoid processes of the rami, should have been withdrawn entire, particularly when we consider the important muscles which are attached to the latter. The phosphoric infiltration remains in the soft structures of the cheeks, thus giving the patient an artificial appearance of health.

Charles T——, aged thirty-nine, a wax-taper dipper, was admitted into the above hospital on the 28th of December, 1858. About thirteen months ago, he complained of much pain and swelling about his lower jaw, commencing in one of the right incisor teeth. Abscesses formed and burst externally through the skin of the cheek. When admitted, his face generally was much swollen, exhibiting the peculiar pasty appearance witnessed in necrosis of the jaw arising from the fumes of phosphorus. Several fistulæ were noticed at the lower margins of the jaw, communicating with dead bone, and giving passage to matter. On opening the mouth, the lower jaw was seen exposed, denuded of periosteum, and quite black in color; it was also slightly movable. He had been working for seventeen years at his calling before the ulceration of his gums commenced.

On the 9th of April, chloroform was given by Dr. Martin, and, when anæsthesia was complete, Mr. Coote proceeded to saw through the symphisis of the lower jaw within the mouth; after which, by the aid of a pair of forceps, the left half of the lower jaw was drawn out entire, without its condyle, but with the ascending ramus. The same proceeding was adopted with the right half of the jaw, which came away with equal facility, but with the condyle, which appeared to be healthy. Some hæmorrhage necessarily ensued, but it was not great, and spontaneously ceased. For some three or four days after the operation, the patient complained of pain in the face, but it gradually diminished and soon

ceased entirely. His health now began to improve, under the use of a liberal diet; his appetite increased, and his strength returned. The investing periosteum of the old bone now began to throw out fresh osseous material, and a new lower jaw was in the process of formation, as has been noticed in other and similar instances. The pasty appearance of the face, and the puffiness of the cheeks, however, remain and these would seem to be almost the permanent consequences of the affection. He left the hospital a short time back, completely restored in health, and able to speak and articulate with tolerable distinctness.

On examining the bone when cleaned and dried, we found it to be massive, and of nearly double the weight of the healthy bone. It was covered in some places with unhealthy lymph, undergoing osseous transformation.

Ten years ago, there was a patient in this hospital, under Mr. Stanley's care, who was a fellow-workman in the same factory, and whose lower jaw was affected with necrosis arising from the same cause.—*Lancet.*

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Extraction of a Thermometer-Tube from the Urinary Bladder.—An immense variety of foreign bodies have been found in the bladder, both of males and females. Hair. beans, fruit-stones, ears of corn, portions of bougie, nails, bullets, small bones, pins, needles, string, stalks of flowers, &c. &c., are amongst the substances enumerated by Mr. Coulson, in his work on the Bladder (chap. XIX.), as having been extracted from this viscus. If allowed to remain, they easily form the nucleus of a stone. On the 22nd of June we witnessed the extraction of a thermometer-tube from the bladder, at St. Mary's Hospital. The patient was a young man, aged twenty-two, admitted under Mr. Coulson's care on the 20th June. He had for many years been subject to nocturnal emissions, and, after a long course of doctoring, he resorted to the expedient of introducing a glass thermometer-tube along the urethra and into the bladder, as a species of impromptu bougie. He accomplished the introduction successfully, although not without difficulty; but on attempting its withdrawal the glass tube broke and a piece remained in the bladder. Its presence did not cause any considerable inflammation; he suffered little pain, and micturated without difficulty. There was no urgency to pass urine; but after the bladder was emptied he experienced pain in the peritoneum, but not of an acute character. There was a slight admixture

of blood with the urine. He was a nervous and somewhat weakly subject, and under the influence of spermatophobia. Mr. Coulson operated as in lithotomy, and removed the piece of the tube entire, so as accurately to fit the other portion. The piece extracted measured between three and four inches in length. Some little difficulty was experienced in drawing it, owing to its being placed across the neck of the bladder. But little blood was lost. The patient has progressed uninterruptedly towards convalescence. We may observe that, in cases, besides the testimony of the patient and the general symptoms, the foreign body could be detected by introduction of a sound—*Lancet.*

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After-treatment of the Extraction of Cataract.—The old method of bandaging the eyes is never practised in the Central London Ophthalmic Hospital, and the modern one of plastering the eyelids is adopted in its stead. The object is to keep the eyelids motionless and closed, in order that the edges of the wound in the cornea may be kept in accurate adaptation by the gentle pressure of the eyelids. If a patient can keep his eyes shut, no appliance is needed; but few can do this. As the bandages are ordinarily applied, they are useless. To make them potent as bandages, would demand a degree of pressure that must be injurious. Very much annoyance is often produced from the cloth being stuck to the cheek by the aqueous humor when it trickles away; and the night's rest is broken in consequence. A couple of strips of court plaster, an eighth of an inch wide, accomplish all that is desired. Mr. Walton generally allows them to remain for a week; but if any unpleasantness is complained of, such as stiffness or hardness, they are removed after the third or fourth day. To this plan, which is wholly unobjectionable, Mr. Walton attaches much value; and he is convinced that the highest attainable results from the operation of extraction cannot be obtained without it. He always keeps the eyelids closed till the aqueous humor no longer escapes from the corneal wound; that is, till the cornea is quite healed. At the end of the week, when the patient has opened the eye, Mr. Walton does not raise the lid himself, but desires the patient to open the eye naturally, which he considers far preferable; he again closes it, if requisite, with court plaster for some days. There can be no doubt of the prudence of this. Many an instance of prolapse of the iris, with all the irritation and ulterior consequences, has been produced by

the use of the eye before there has been proper reunion. Young operators are often sadly puzzled to account for the secondary imperfection of prolapse of the iris, after they have had such promising results when the eye was looked at a few days after the operation. This is the solution; the eye is used far too soon; and by the action of its own muscles, the prolapse is occasioned.

The great proof of cicatrization of the cornea is the fulness of the anterior chamber. So long as the iris is pressed forwards, and there is not the natural accumulation of aqueous fluid, the eye should be kept closed. A very little attention will enable the practitioner to decide this point.

When the iris is wounded in the operation of extraction, prolapse is very apt to occur under any condition; hence, Mr. Walton lays it down as a rule, that much pains should be taken to prevent the accident. He considers it far better on every occasion to finish the operation with the secondary knife, rather than to complete it with the cataract knife, when by so doing the iris is at all injured. The late Mr. Alexander, than whom there never was a better operator, always made the section of the cornea with two knives. As soon as he had effected the counter-puncture with Wenzel's knife, he withdrew it, and completed the section with the secondary knife.—*British Medical Journal.*

Fissure of the Anus.—One of the most painful affections situated in the neighborhood of the anus, is the fissure alongside of the sphincter. When examined, scarcely any lesion is to be detected; but on rendering the structures tense, a very small slit with reddened margins may be observed, and from which there may be a little secretion. This apparently trifling malady occasionally causes the most intense agony. Latterly, several examples have come under Mr. Hancock's care at the Charing-cross Hospital, which have been effectually cured by the division of a few of the muscular fibres of the sphincter at the situation of the fissure. It is unnecessary to divide the entire sphincter in the treatment of this affection, and it is now seldom resorted to. On the 2nd inst. this operation was repeated by Mr. Hancock, on a woman twenty-seven years of age, whose sufferings have been very great for nearly twelve months, from the presence of a fissure of the kind mentioned. On passing her motions, the sensation was compared to that of a knife running through her. When we saw her on the 7th, five days after the operation,

she expressed herself as completely relieved; all pain had gone, her health had generally improved, and she was beginning to assume a cheerful aspect. Patients with a fissure of the anus have a careworn and anxious expression of countenance, more so than is observed in fistula of the bowel; but it quickly disappears when surgical relief has been obtained. —*Lancet.*

PHYSIOLOGY.

The Glycogenic Function in Fœtal Life. A Communication by M. CLAUDE BERNARD to the Academy of Sciences, Paris, at its Sitting of January 10th, 1859. (Translated for the Nashville Monthly Record, by DANIEL F. WRIGHT, M.D.)

On a New Function of the Placenta, by M. Claude Bernard. The object of my commnnication is to establish anatomically and physiologically that the placenta, with other uses which are undoubtedly varied and numerous, is destined during the earlier stages of fœtal development to accomplish the glycogenic function of the liver, before the latter organ has acquired that development of structure which subsequently enables it to perform that function.

I have been for a long time led aside from the result towards which my researches aimed, through making my experiments upon the placentæ of the ruminant animals, which are easily procurable at the shambles of Paris. During several years I fruitlessly made repeated observations upon calves and lambs taken at every stage of intra-uterine life, and was still unable to find any part of the placenta of these animals which contained the glycogenic matter. Spite of these early failures, I afterwards had recourse to the placentæ of rabbits, guinea-pigs, etc.

I found that there is in the placenta of these animals a white substance formed by epithelial cells or agglomorate glandules. I moreover determined that these cells, like those of the liver in the adult animal, were filled with glycogenic substance. This mass of glycogenic cells appeared to me to be chiefly situated between the maternal and the fœtal portion of the placenta, and subsequently to its development seemed to be atrophied in proportion as the fœtus approached the period of its birth. I have since ascertained that the

placenta of rabbits and guinea-pigs is formed of two portions having distinct functions: the one, vascular and persistent till birth; the other, glandular, preparing the glycogenic matter, and having a more limited duration.

Meanwhile the negative observations made upon so large a number of the ruminants arrested my attention: negative experiments which were to me just as indubitable as those from which I obtained the above positive results.

In repeating my researches, I succeeded in establishing a remarkable arrangement which no one before had distinctly pointed out: that while the vascular portion of the placenta represented by its various cotyledons accompanied the allantois and was distributed on its external surface, the glandular portion was distinct from it and was developed on the internal surface of the amnion. Whence it results, that if in the rodentia, and the other animals with a simple placenta, we find the vascular and glandular portions of the placenta mixed together, in the ruminantia, on the other hand, we find the vascular and glandular portions of this organ developed separately on distinct membranes, and capable, in consequence, of being examined separately in their respective evolution. Thanks to this anatomical arrangement, we are able to prove clearly that the vascular portion of the placenta persists and grows until birth, while we see that the glycogenic portion attached to the amnion increases during the earlier periods of gestation, and attains its greatest development towards the third or fourth* month of intra-uterine life, then disappears by degrees, passing through the various forms of atrophy and degeneration; so that at the birth of the mammifera there no longer exist any traces of that temporary hepatic portion of the placenta. But we ought moreover to add, to arrive at the true character of these organs, that during the whole period while the hepatic placenta of the amnion is growing and performing its function, we observe that the fœtal liver does not yet possess its appropriate structure or functions, and that it is precisely at the moment when the liver is developed, and its cells, having acquired their characteristic form, commence secreting glycogenic matter, that the hepatic organization of the amnios has a tendency to disappear.

The hepatic patches of the amnios appear in ruminants

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* I am only able to give here these limits approximately, by reason of the impossibility of ascertaining the age of those calves which are procured at the shambles.

from the earliest periods of embryonic life. They are developed by degrees on the internal face of the amnios, covering over the umbilical cord to a point where a well-defined line separates the fœtal integuments from the amnion. Afterwards these patches, which, particularly along that portion of the membrane which invests the cord, assume the appearance of villosities, extend themselves along the other portions of the amnios, in proportion as the blood-vessels which accompany them are developed. They gradually increase in volume: formed at first of a transparent matter, they become at a later period opaque, especially towards their edges, which are elevated a little, and sometimes cause them to resemble in their appearance patches of lichen. At other times they have quite varied appearances, flattened or filiform, and are sometimes blended one with another so as to become confluent. At their complete development, the patches attain a thickness which sometimes amounts to three or four millimetres; those which are filiform often present a considerable length, and are sometimes enlarged at their extremity in the form of a club. At a later period these hepatic patches of the amnion cease to be developed; at certain points they become yellowish, of a fatty appearance; in other places they fall off and float in the amniotic fluid, and leave on the membrane a kind of cicatrices which afterwards completely disappear.

We can determine with great ease the presence of glycogenic matter in the hepatic patches of the amnion at all periods of their development. From the time when they first appear, it is easy to recognize this matter under the microscope with the aid of iodine. Until the patches are completely developed we can obtain from them their glycogenic matter in large quantities and study its characters. To obtain it easily, the process will consist of digesting the amniotic membrane in boiling water, which will enable the patches to be easily detached for the purpose of rubbing them in a mortar, and extracting the matter from them by boiling, exactly as we proceed for the extraction of glycogenic matter from the liver.

As regards its characters, we may say that the glycogenic matter of the amniotic patches exhibits the most perfect identity with the glycogenic matter of the liver. It is dissolved in the water, giving it a milky appearance; it may be precipitated by alcohol and crystalized by acetic acid. Iodine communicates to it an intense vinous red color, which disappears when heated and reappears on cooling. This coloring of the glycogenic matter of the amniotic patches by iodine

takes place not only when the matter has been extracted from the cells by boiling, but it may be observed also in the cells of the organ themselves, as we shall see presently. Like the glycogenic matter of the liver, that of the amniotic patches also changes into dextrine or fermentible sugar (*glucose*) with great ease under the influence of diastatic ferments, both animal and vegetable, and by the action of boiling with strong acids.

While we study the structure and the histological development of the hepatic patches of the fœtus, we can follow out very distinctly the formation of the glycogenic cellules, as well as the development of their contents.

The amniotic membrane in the calf seems to be at first destitute of any well-marked epithelium, and we find its tissue chiefly constituted of fibres of elastic tissue with nuclei contained in a network of cells of a fusiform aspect. At the very moment of the appearance of the patches we discern under the microscope on the internal face of the amnios, and continuously along that part of the membrane which invests the umbilical cord, a kind of spots formed of epithelial cells; then in the center of each spot we observe groups of glandular cells in very small number at first, and soon we are able to observe the patch at its very origin and composed of only one or two glandular cells. We very easily distinguish the glandular or glycogenic from the epithelial cells which accompany them, both by their appearance and by their reaction with iodine. In fact, when we add to an amniotic papilla or patch on the stage of the microscope a little tincture of iodine acidulated with acetic acid, we soon see the glycogenic cells assume a vinous red tint, while the epithelial cells continue colorless or become faintly yellow. By degrees during development the groups of glycogenic cells increase and assume the form of papillæ, especially on that part of the membrane which invests the cord. Examined under the microscope, these papillæ are formed of glycogenic cells covered with an epithelium; as soon as we add the acidulated tincture of iodine, we see the glycogenic cells of the papillæ colored of a vinous red, especially at their base, which is very definitely separated from the surrounding tissue. The hepatic patches are composed of the same elements as the papillæ; it is always difficult to ascertain whether as regards their agglomeration they ought to be considered as consolidated papillæ, or as having another mode of growth. All we are able to say is, that we can see them spread in the direction of their circumference, which exhibits well-developed glyco-

genic cells, while in the centre these cells sometimes appear to be at a less advanced stage of development.

When we bruise the patches or the papillæ and mechanically separate their histological elements, we obtain isolated cells provided with a nucleus and sometimes a nucleolus, and containing a granular substance: the granular substance is colored wine-red by the acidulated tincture of iodine; the nucleus, the volume of which seems to me to vary under the influence of reagents, never assumes the same color by the action of iodine. The cells of the hepatic patches of the amnion offer a great resemblance in form and reaction to the cells of the liver in a state of function. In fact, we can isolate both the cells of the amniotic patches and those of the liver by allowing a small portion of the tissue of these organs to macerate for some time in a concentrated alchoholic solution of caustic potash; we then see that the contents of the two orders of cells continue insoluble in this reagent, and fall to the bottom of the liquor in the form of a white deposit which offers under the microscope both the original form of the preserved cells and an amorphous granular matter. When, again, under the microscope, we saturate the excess of potash with crystallizable acetic acid, and afterwards add tincture of iodine, we see the wine-red color appear, and that with greater intensity than when we operate on the fresh cells.

When the hepatic patches of the amnios begin to grow yellow, to fall off, to be absorbed, or to degenerate into fatty matter, we perceive changes in their microscopic structure; the glandular cells in general lose their nucleus at the same time with their glycogenic matter, so that upon treating a fragment of these altered patches with the acidulated tincture of iodine, we see a mixture of cells, some of which assume the wine-red color, while others remain uncolored. It is evident, moreover, that the cells which remain uncolored are destitute of nuclei and of granular contents. We sometimes perceive a transition between these two extremes, that is to say, we see cells in which the nucleus and the granular matter have nearly disappeared, and in which the wine-red color is scarcely perceptible.

A little later, when the patches of the amnios form mere cicatrices, we find only the flat cells, destitute of nuclei, and in which it is impossible to detect the slightest trace of glycogenic matter. These cells at a later period finish by disappearing themselves. When the patches, instead of falling off and disappearing, degenerate into fatty matter, we observe

under the microscope the presence of a fatty matter at the same time that we see mixed with it very fine octahedral crystals, which exhibit all the characteristics of oxalate of lime, so far as to be insoluble in water or in acetic acid. It is needless to add that there is, besides, a complete absence of glycogenic matter in these degenerated hepatic patches.

If now we institute an examination of the structural development of the liver, parallel with that which has just been made of the evolution of the glycogenic patches of the amnios, we shall be struck with the constant inverse relation which we observe between the development of the cells of the liver and of those in the amniotic patches.

At the early periods of embryonic life,* when the amniotic patches are well filled with glycogenic matter, it appears that the liver of the fœtus, still very soft, is made up of rudimentary cells rounded or fusiform, soluble in the alcoholic solution of potash, deriving no color from iodine, and showing none of the characteristics of glycogenic cells. At this period the tissue of the liver does not give the least trace of glycogenic matter.

At the end of their period of growth, when the glycogenic cells of the amniotic patches begin to disappear or degenerate, we find in the liver of the fœtus cells which have acquired their definitive forms as liver-cells including one or more nuclei with granular contents, not dissolving in an alcoholic solution of potash and deriving the wine-red color from the contact of iodine, after we have saturated the alkali with acetic acid. It is at this period that we begin to be able to separate from the liver of the fœtus, which has become more firm, glycogenic matter exactly similar to that which the adult liver produces.

Later still, when the patches have entirely disappeared or degenerated into fatty matter, and the fœtus is near the period of its birth, we find that the tissue of the liver, now grown as solid as in the adult animal, is now made up of elements which have assumed their definite form: all the cells of the liver are then filled with glycogenic matter, and at that period we can separate from the liver of the fœtus glycogenic matter, as abundantly as in the best nourished adult animal.

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* At the commencement of embryonic life in the embryo calf while from two to three centimetres long, I have not as yet perceived the amniotic patches. Perhaps hereafter glycogenic cells will be discovered in the umbilical vesicle.

To resume: From all the facts which are contained in this paper, I think we can make the following inferences:

1. There exists in the placenta of the *mammifera** a function which till now had remained unknown, and which appears to take the place of the glycogenic function of the liver during the earlier periods of embryonic life. This function is located in a glandular or epithelial structure of the placenta, which in certain animals is mixed with the vascular portion of that organ, but which in ruminants appears separate. so as to form on the amnion patches of an epithelial appearance, which every one has doubtless seen, but the significance of which has been hitherto unknown.

2. This temporary hepatic organization of the placenta, by permitting us to study directly in an insulated anatomical element the production of glycogenic matter, confirms and completes by a new example that which I have long ago maintained, that the formation of the glycogenic amylaceous matter is a common faculty of the animal and vegetable kingdom. The observations contained in this paper furnish us with further novel analogies, as we see glycogenic amylaceous matter form around the embryo animal, just as in plants it accumulates in their seeds around the embryo vegetable.

3. The glycogenic function in animals commences, then, from the origin of fœtal life, and before the organ in which that function is located in adult life is developed. But then it is located in a temporary organ belonging to the fœtal appendages.

4. All which has been said in this paper relates exclusively to the *glycogenic* function of the liver; but it will be really a question, to examine whether the *biliary* function which the liver possesses in the adult is equally accomplished by the placentary hepatic organ which we have described. The question should be put in these terms, viz.: Whether the same glandular cells are charged with two functions which thence must be consolidated and united; or whether, on the contrary, the liver ought not to be considered rather as a complex organ in which are found mixtures of anatomical elements distinct from one another, and destined, the one for the formation of the amylaceous matter, the other for the

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* In birds, (the chicken,) I have ascertained that before the development of the glycogenic cells of the liver there exist glycogenic cells which are developed in the walls of the vitelline sac; but not having as yet completely followed out their evolutions, I will treat this subject in another communication, confining myself at present to speaking of the *mammifera*.

biliary secretion? This question, which as yet has not been solved, in spite of the numerous histological labors of which the liver has been the object, appears to me capable of being illustrated and even decided by physiological researches, made, on the one hand, on the embryonic developments of the function, and, on the other, upon the inferior animals. I have entered upon some researches on this subject, of which I will give an account to the Academy as soon as they shall be terminated.

EDITORIAL AND MISCELLANEOUS.

THE CASE OF DR. SMETHURST.

This important and interesting trial has for some time past agitated the mind of the British public, and excited the attention of medical men wherever its history has been reported. The ignorance of some, and the conflicting testimony of all the medical witnesses, the obstinacy, inaccuracy and criminal negligence of the great English toxicologist, Dr. Taylor, the utter and evident incapacity of the jury to sift or even understand the evidence, and the partial and almost savage charge of the presiding judge, constitute the most signal features of a case, whose whole course and result may in the end, we trust, prove valuable, by directing popular attention to the necessity of changing the entire mode of trial when such delicate, obscure and grave subjects are to be examined and decided upon.

For the details of this matter we must refer our readers to the English journals; it is only in our power to give an outline of it. Dr. Smethurst, it seems, had for some months been cohabiting (with the knowledge and consent of his wife) with Isabella Bankes, who in course of time became pregnant, apparently without the knowledge of either party. About this date, or soon after, she was attacked with diarrhœa, which did not yield to treatment, but was succeeded by persistent vomiting, and violent dysenteric discharges. On the 3rd of April, Dr. Smethurst called in Dr. Julius, of Richmond, the partner of the latter, Dr. Bird, attended in consultation from the 18th, and on the 28th of the same month. Dr. Todd saw her and prescribed for her condition. None of her

attendants took the trouble to ascertain whether or not she was pregnant, although her liability to become so was fully understood by all of them.

Dr. Julius, it seems, first suspected that some irritant poison was being administered to her secretly, and mentioned his suspicions to his partner, Dr. Bird. On the 30th of April, they obtained from Dr. Smethurst himself, a portion of an evacuation of the patient, and soon after another. These were both properly secured, labelled, and sent away for analysis. On the 2nd of May, Dr. Smethurst was taken into custody, and the following day, Isabella Bankes died. The sufficient motive for the murder was found in the fact of a will having been made by deceased in favor of the prisoner. It came out afterwards, however, that Dr. Smethurst had not only enjoyed the entire and unreserved use of her own property previously, but also of an annuity in which she had a life interest, and of which considerable sum he was, of course, deprived by her death.

The following resume of the material evidence given during the trial, we extract from the Dublin Medical Press:

The most eminent physicians, the most renowned medical jurists, and most able practical chemists, were ranged upon opposite sides, and seem to have left behind them nothing but uncertainty, traces of ignorance, incompetency and contradictions. Drs. Todd, Julius, Bird, Barnwell, Wilks, Babington, Copland, a formidable array, all believed the symptoms under which deceased had labored were to be attributed to the action of irritant poison. Dr. Alfred Swaine Taylor, the well-known medical jurist, after seventy-six analyses, Dr. Odling, also of Guy's Hospital, Professor Brande, all of whom we had supposed able to give a positive opinion as to the presence of arsenic, were examined for the prosecution, and agreed in attributing the death to that substance. Dr. B. W. Richardson, J. E. D. Rodgers, John Louis W. Thudichum, F. C. Webb, G. F. Girdwood, James Edmunds, W. Tyler Smith, some of them physicians, some professors of chemistry, some medical jurists, some midwives, all concurred in referring the decease to natural causes, and in believing that the symptoms of dysentery and diarrhœa under which she labored presented nothing abnormal. Dr. Bird was ignorant of the woman's pregnancy, and said a knowledge of such a

fact would not have caused any change in his opinion. Dr. Julius swore he had no means of ascertaining the existence of pregnancy until the post-mortem examination, but, like his partner, such knowledge would have made no difference in his treatment, neither had he ever heard of vomiting and diarrhœa in a pregnant woman which would not give way to ordinary remedies. Dr. Todd also entirely overlooked the possibility of pregnancy, and though agreeing in opinion with the other two lamps of science, that Miss Bankes was suffering from poison, he also agreed with them in their practice of ordering no antidote, except sulphate of copper can be looked upon in that light. Dr. Metcalfe Babington had delivered more than two thousand women, but had never know a case in which, in the early stages of pregnancy, vomiting was so urgent as to require abortion for the preservation of life. Dr. Webb expressed an opinion that the pregnancy of deceased was a material ingredient in her case, that the sulphate of copper administered to her by some of the medical attendants was highly improper, and that she died of dysentery aggravated by pregnancy. Dr. Babington never saw violent diarrhœa in the commencement of pregnancy, whilst Dr. Edmunds deposed that in its early stages there is frequent severe vomiting and diarrhœa, so as to produce very great exhaustion, and Dr. Girdwood, having very great experience in cases of midwifery, has seen a good many in which pregnancy was combined with dysentery, and in one case the dysentery was very severe. He considered it highly improper to administer mineral medicines under such circumstances. Dr. Bird informed the jury that towards the final termination of the case, vomiting and diarrhœa had ceased, which was attributed to the fact of the prisoner being in custody, and consequently not having access to the patient. Dr. Tyler Smith, who gave the evidence of a well informed and experienced physician, accounted for the above fact by saying that in some cases in his practice, where death had occurred from vomiting in pregnancy, the stomach had retained food for some days before death. Dr. Thudichum had analysed grey powder and bismuth, and found them to contain arsenic, but did not agree with Dr. Rodgers that the quantity found in those substances did not render it improper as a medicine. Dr. Rodgers from his own knowledge stated positively that nearly all the bismuth sold contained arsenic, and all the grey powder antimony. Dr. Julius administered chalk mixture catechu and compound tincture of camphor, also grey powder, and compound ipecacuanha powder, quinine, dilute sulphuric acid, prussic acid, chloric ether,

infusion of gentian; he never prescribed bismuth, nitrate of silver, or acetate of lead, whilst his partner (Dr. Bird) took a turn at all the latter medicines, eschewing the former. Dr. Todd ordered sulphate of copper and opium, whilst Mr. Caudle, dispensing-assistant to the firm of Julius & Bird, at the suggestion of Dr. Taylor, transmitted some hydrate of magnesia as an antidote to the suspected arsenic. Dr. Todd alluded to a very peculiar expression of countenance which he could only describe as an "expression of terror," quite different from what medical men call "anxiety," and which he had never seen in any one suffering from any natural disease; Dr. Tyler Smith, on the other hand, stated that Dr. Paul Dubois, who had seen twenty fatal cases of vomiting in pregnancy in thirteen years, had expressly noticed a morbid expression of the features, as one of the symptoms of danger calling for the induction of abortion. Dr. A. S. Taylor swore to the presence of arsenic in some fœcal matter passed by Miss Bankes, having, at the preliminary examination before the magistrate three months previously, also most positively sworn to the presence of the same substance in a bottle numbered 21. His evidence on that occasion was so remarkable, that we will give his own words: "The bottle No. 21 contains seven grains of chlorate of potash, and one grain of arsenic in each ounce. I believe the effect of giving small doses of this mixture from time to time would be to produce nausea, vomiting, pain in the bowels with purging, and if the doses were continued, it would produce chronic inflammation and ulceration of the stomach and bowels, and the discharges would contain blood and mucous. If these doses were continued, the effect would be to slowly lead to death by exhaustion. That in the bottle No. 21 is what we call white arsenic." On cross examination at the trial, the following admission, in respect to this very analysis, was forced by the counsel for Smethurst from the author of "Taylor on Poisons:" "I discovered (referring to the stools) the presence of arsenic by using copper gauze of the same description that I used afterwards to the contents of the bottle No. 21; in the experiment made with this bottle the arsenic was deposited by myself! Dr. Odling and I believed that the arsenic we found was contained in the liquid!! I likewise told the magistrates that I had tested all the materials used in my tests before I commenced making my analysis!!!" In plain language, the "white arsenic," for the professor was particular, he had discovered, was not contained, even a trace of it, in the bottle No. 21, but in the copper gauze, which he swore before the magistrate to have

been perfectly pure, which he told the jury he had been using for a number of years, and would certainly still continue to use, and in regard to which Professor Herapath of Bristol wrote in the Times: "If the same impure copper has been used for twenty years, and evidence given upon it, what shall be said of the justice of the convictions and executions which have taken place during those years upon Dr. Taylor's evidence."

Although Dr. Smethurst was condemned, the great mass of the profession in England was satisfied that the evidence did not justify the verdict which had been rendered. Indeed, the conviction of his legal innocence was so strong, that a powerful appeal was made in his behalf, through the *Times*, by Tyler Smith, and a letter addressed to the Home Secretary, Sir G. C. Lewis, by Drs. Thudicum, Richardson and Webb. The arguments advanced by these distinguished gentlemen, to prove that her death was incompatible with the hypothesis of poisoning by arsenic or antimony, but compatible with natural disease as the producing cause, were so convincing, that a reprieve was soon procured, and this was speedily followed by a full pardon.

No one can read the full account of this unfortunate trial, without a shudder at the thought of so narrow an escape from a judicial murder, and a feeling of painful uncertainty, whether in the majority of criminal cases, as now conducted, medicine may not be and is not made an instrument of the direst injustice. It is terrible to think of the slender thread upon which the life of a human being may hang, when we see how nearly culpable ignorance and petty, selfish rivalries have terminated in the undeserved death of a legally innocent man.

It is humiliating in the last degree to witness the majesty of true science degraded and dethroned by the imbecility or recklessness of her own followers. We heartily endorse the following sentiment of the *British Medical Journal*, which, in common with nearly all of its associates, has given utterance to wise and bold expressions of approval or condemnation without fear or favor of those concerned, and with as much point as truth.

"If every fresh suspected criminal is to drag medicine into the pillory, there to exhibit itself as it has done of late, we need not be surprised to find public faith in us rapidly departing. . . . . What security is there in the secret proceedings of any one man in investigations of this nature (analysis in poison cases)? A chemist receives a jar—it may have been upset on its transit, as in Palmer's case—and he forthwith sets to work in the seclusion of his laboratory to eliminate from it infinitesimal portions of poison; his instincts as a hunting animal are, perhaps, called forth. What guarantee have we that some fatal error has not been made in the course of his long distillations? that his tests are pure? The matter subjected to analysis having been destroyed, the power of veryfying his results no longer exists, and his bare word is often enough to send the accused to his long account. It is impossible that life can much longer be at the mercy of such hap-hazards as these. If test-tubes and wire-gauze are to be the fatal instruments of justice, the public should at least be convinced that no error has arisen in their application."

HYSTERICAL RELIGION.

From time to time brief and unsatisfactory notices of the melancholy occurances below alluded to, have reached us, but we have seen no authentic account of this hysterical epidemic, and meagre as the quotations from Archdeacon Stopford's pamphlet are, they furnish us the first authentic report of the great Celtic *revival.* For "true religion and undefiled" we entertain the profoundest respect. God forbid! that any word of our's should be construed into a jeer or jest at the expense of Christianity, in any of those phases which, however peculiar, are held sacred by earnest and thoughtful men. But in this civilized day, when the distinct voice of science may claim a hearing without charge of irreverance, and when man is slowly learning that God's written word and materialized thought can never really be at variance, we may surely direct attention to the purely physical character of some of those phenomena which have in all ages been regarded by the masses as the result of a peculiar spiritual influence, but which are really only manifestations of a physical disorder referable to mental and bodily laws.

Many persons who would sneer at the mad antics of the ancient priests, of which we have such frequent mention in classical and sacred writings, and who would read Hecker as a curious book merely, have been half persuaded to believe in table tippings, and are zealous converts to the spiritual origin of the "Kentucky Jerks," and those similar though less strongly marked convulsionary seizures which are of frequent occurence at many modern religious meetings. Were we to indicate the lesser degrees of this sympathetic influence by name, the want of that scientific knowledge and liberal opinion of which we speak, would at once condemn us and our article for gross impiety. He would be undertaking a valuable but bold task who should inform the public in undisguised language of the nature and extent of those sympathetic laws which originate and direct nearly all the movements of great masses, but we fear the day is still far distant when the people will be prepared to receive these truths, however fairly and certainly they may be established.

We make no apology for extracting the subjoined remarks, from the *British Medical Journal* of September 17th, ult.

"To the psychologist and physician, these pseudo-religious epidemics offer a study full of interest. As they are now presented to our view, it is impossible not to see in them, in an intense degree, the workings of disease both of mind and of body. Without attempting to deny that a community of people may from time to time, and with benefit, experience an unaccustomed development of religious feeling, we must express our conviction—and it is one which must be agreed in by every sound-minded physician, clergyman, and layman —that the outrageous scenes which are related as sometimes occurring in places of public worship, of members of the congregation exercising their voices and limbs in the most unearthly manner, until, perhaps, bodily prostration incapacitates them from the performance of the duties of their daily life, are utterly inconsistent with the very principles of that religion which is professed to be taught to the people, and under the influence of which they are imagined to act.

In the narratives of these so-called conversions, certain facts now and then appear which render the cases at once clear to the physician, and point out the individuals as sub-

jects for pity rather than for congratulation. The attentive readers of Hecker's account of the Dancing Mania can have arrived at no other conclusion, than that the phenomena described by him were various modifications of the diseased condition which, comprehensively, we call hysteria: and, though it does not appear always on the face of his narrative, there can be little doubt that in a number of instances a state of the body existed, which rendered it the too willing organ of an unenlightened mind. Speaking of a convulsive epidemic, which at one time broke out in a chapel at Redruth in Cornwall, he significantly observes: "It seized none but persons of the lowest education;" and we know, that the general hygiene of persons of this class is generally not of the best. In a recently published pamphlet* on the "religious revival" which is described as taking place in the north of Ireland, the author, who had visited the district with the laudable object of obtaining reliable information on a subject so intimately connected with his calling, says, in speaking of his visit to the meeting-house "famous for cases":—

"The preacher, before giving out his text, requested that if any cases occurred the congregation would be quiet, and leave it to the office-bearers of the church, who had made full preparation for their reception. While the preacher was urging, with the peculiar pointing of the hand before described, "Your case is as bad as hell can make it," a poor girl cried and fell, In reproving the excitement which followed, the preacher said "God is doing His work in that individual." When the sermon closed, I obtained admission to the room to which the girl had been caried, pursuant to the arrangements announced by the preacher. The room was small, and very narrow and stifling, no air, no water was there. A more pitiable sight I never saw. This girl was about 15 years of age, or perhaps a year or two older. Her frame was weak and thin; her small hands stained and ground with hard work; her skin delicate and transparent; her hair and eyelashes long and dark; her neck marked with scrofula; with a highly intellectual face, seldom seen in her class of life except in weakly girls, and now made painfully interesting by the unearthly expression of cataleptic hysteria. Every movement of the head and hands, every expression of the countenance, every moan, was markedly hysterical. She

* The Work and the Counterwork, or, the Religious Revival in Belfast. With an Explanation of the Physical Phenomena. By Edward A. Stopford' Archdeacon of Meath. Pp. 92. Dublin: Hodges, Smith, and Co. 1859.

had previosly been struggling and screaming. She was now quiet; her lips sometimes moving, but inaudibly. She had spoken of the devil catching souls to throw them into hell, crying. "Away! you shan't have mine:" just the last impression made upon her failing mind. I learned that this was the third attack that this poor girl had had in a short time; each being more severe than the former, so readily does the habit grow. I could have wept to see this sad disease superadded, in the name of religion and the Holy Ghost, to a poor weak frame, a scrofulous habit, and a life of toil."

The case here related by Archdeacon Stopford may be used as the key to a number of others which are supposed to arise from a development of religious influence. In another part of his pamphlet, he speaks positively of the hygienic condition of the subjects of these cases.

"The diet of the mill-girls often consists wholly of bread and tea....At work for thirteen hours, they are then kept, often till long past midnight, in crowded and ill-ventilated assemblies, subjected to the greatest exitement. I have heard them singing after one o'clock in the morning."

But the reference to a work on the subject by a clergyman—a man, too, who writes evidently with highly enlightened views—leads us to consider the part taken by the ministers of religion themselves, in reference to the manifestations to which we have alluded.

In the first place, it is too obvious that the hysterical phenomena are often encouraged, under the idea that they are the outpourings of a mind filled with religious fervour. In the language—profane, we must call it—of the preacher alluded to by Archdeacon Stopford. "God is doing his work in them." Hecker, in recording from an English author the history of the religious epidemic in Cornwall, to which we have already referred, says :—

"The clergy, when, in the course of their sermons they perceived that persons were thus seized, earnestly exhorted them to confess their sins, and zealously endeavoured to convince them that they were by nature enemies to Christ; that the anger of God had therefore fallen upon them; and that if death should surprise them in the midst of their sins, the eternal torments of hell should be their portion. The over excited congregation upon this repeated their words, which naturally must have increased the fury of their convulsive attacks. When the discourse had produced its full effect, the preacher changed his subject; reminded those who were suffering of the power of the Saviour, as well as of the grace of God, and

represented to them in glowing colours the joys of heaven. Upon this a remarkable reaction sooner or later took place. Those who were in convulsions felt themselves raised from the lowest depths of misery and despair to the most exalted bliss, and triumphantly shouted out that their bonds were loosed, their sins were forgiven, and that they were translated to the wonderful freedom of the children of God. In the mean time their convulsions continued, and they remained during this condition so abstracted from every earthly thought, that they stayed two, and sometimes three, days and nights together in the chapels, agitateted all the time by spasmodic movements, and taking neither repose nor nourishment."

Archdeacon Stopford gives even a more revolting picture than this of the proceedings at Belfast. He says that "hysteria, in connection with revivalism, is now intentionally produced and propagated by man;" that "hundreds of mill girls in Belfast have prayed, and are praying, to be "struck';" that "there can be no doubt that in places of worship which have become famous through their hysterical cases, or in which hysteria is prayed for, the production of it is an object desired and aimed at in the preaching."

Is it not monstrous, that such a state of profound ignorance (if not of something worse) should prevail as to lead to the deliberate propagation of a diseased condition? and still more so, that the manifestation of this condition should be palmed off on the people as the direct and beneficent act of the Deity?

Turning to another part of Archdeacon Stopford's pamphlet, we find him expressing a most judicious view as to the course which ought to be taken. When a hysterical cry, he says, breaks out in a place of worship, the clergyman may at once stop it by an exersise of calm and quiet authority.

"If such a cry arise, let the clergyman stop his discourse, and say instantly, in the calm voice of one who knows that he must be obeyed, "I will not let you cry." The words must be thus absolute; anything of the nature of an appeal or an entreaty will probably give increased vigour to the cry."

The phenomenon is purely emotional, and is to be restained in its commencement by the will of the preacher. Possibly, too, a kind of feeling of awe may operate beneficially; as it apparently did in the case of the Shetland minister mentioned by Hecker, who put an effectual stop to the interruptions frequently produced in his sermons by cases of catalep-

tiform hysteria, by threatening to have the patients immersed in a fresh-water lake adjoining his kirk.

Archdeacon Stopford fully recognises the important aid which the medical profession might afford in the treatment of this epidemic hysteria, but laments that there is a tendency to exclude this assistance rather than to employ it.

"I fear there will be few medical reports upon a subject so interesting to the medical and clerical professions. I heartily wish also that the medical instruction necessary for the clergy on this subject should be afforded to them by members of the medical profession, who alone are truly competent to give it. But I believe it to be necessary that the clergy should first be awakened, to a sense of the value of such instruction, and the necessity of seeking for it. When so sought for I believe, from my own experience, that it will be cordially afforded. My task is only to lead to the desire for it; and, in the meantime, to give to my brethren that which they have especially asked me for, some light, dim as it may be, on the state of things which they have to deal with at the present moment.

We have brought forward this subject, as one of interest to the medical as well as to the clerical profession. The amount of mischief is great that is done where weakly, excitable frames, worn out with bodily toil, and sparely fed, are exposed to the mental influence of designing or injudicious men. That the clergyman to whose work we have referred in this article, has taken a correct view of the duty of his profession in the case which has been described, is manifest; and it is to be trusted that his example will lead to a strict and judicious co-operation between the clergy and the members of the medical profession, in all instances where such phenomena as those which have been described as occurring in Belfast may break out.

NECROLOGICAL NOTICES.

Since our last issue, we have received the painful intelligence of the death of two widely known and highly esteemed physicians, one a native, and both residents of our State.

Dr. J. P. Barrett terminated his career in Abbeville, his native district we believe, with whose interests and prosperity he was closely identified. He had long been suf-

fering from a cancer of the stomach, and although his removal will be a severe loss to the large circle of his professional clients, by whom he was sincerely beloved, we can scarcely mourn the decree which has at last relieved him of the agony of a terrible and lingering disease.

He was an ex-President of our State Medical Association, a valued contributor to our periodical literature, and to this journal particularly, in past years, and a gentleman of irreproachable moral character, urbane and elegant manners, and considerable attainment and original talents.

Dr. E. H. Barton was a native of Virginia. At the age of manhood he removed from that State to West Feliciana, and afterwards to Cuba, in both of which places he practiced successfully for many years. From the last named island he emigrated to New Orleans, where he spent sixteen years of his life. During this period he was specially engaged in the collection of mortuary, meteorological and other statistics. His reports upon yellow fever, more particularly, are noted for their laborious and accurate fullness, and his name is honorably connected with the literature of this subject, to whose study he devoted the greater part of his time and talents.

His sanitary reports upon the city of New Orleans, form an interesting portion of the bibliography of that pernicious and illy understood disease, the yellow fever, and will secure for him distinguished mention by all future historians of this epidemic pestilence upon our continent.

Three years ago he moved to Columbia, S. C., in consequence of his conviction of the insalubrity and bad hygiene of New Orleans, and in this city, where he had made many warm friends, and was rapidly attaining a high professional eminence, he closed the record of his useful and laborious life, while in the very meridian of his powers and influence.

THE MEDICAL COLLEGE OF THE STATE OF SOUTH CAROLINA.

This old and honored institution, will commence its annual course of lectures on the SECOND MONDAY (the 14th inst.) of this month.

Since its origin, the College has never been in a more flourishing condition than at present. Each year has continued to improve its capacities for supplying every form of information that the student needs or could desire. A large and well assorted museum, to which the liberality of the faculty has lately made important additions, furnishes all the material for pathological, physiological, and anatomical instruction, in which last department, a large and well supplied dissecting room, presided over by the most competent demonstrators, offers attractions unsurpassed by any Medical College in the whole country.

The clinical courses delivered at the Roper and Marine Hospitals, insure the attentive student a field for the study of the diseases peculiar to our latitude, as well as those more generally diffused; and surgical cases are operated upon before the class at both of these institutions, and at the excellent weekly clinic of the Surgical professor.

The professorial corps of the College has lately undergone some changes, which the experience of last winter authorizes us to affirm, has not been for the worse. The chair of Prof. Dickson will hereafter be occupied by Prof. Geddings, whose reputation as a pathologist needs no mention at this date; Dr. Chisolm, as Professor of Surgery, has already exhibited his capacity to perform its duties to the highest satisfaction of his classes, and Dr. Miles, will, as adjunct Professor of Anatomy, continue his admirable prelections upon this branch.

We trust sincerely that, in common with all our Southern Institutions, this College will receive its full share of patronage from pupils at home, and we may refer preceptors,

students, and others interested, to the advertisement of the Faculty published in this journal.

PROFESSORS HUGHES BENNETT, AND ROKITANSKI ON THE WHITNEY CASE.

Most of our readers will remember the violent discussion which agitated the New York Academy of Medicine some months ago, in regard to the charge which had been brought against Dr. Green of that city of having caused the death of S. S. Whitney by pushing a probang through the trachea into the apex of his lung. Every one was satisfied upon the printed record of the case, that Dr. Green had been unjustly accused, and that all the weight of pathology, anatomy, and common sense was against the absurd assertions of his distinguished opponents. Copies of the *necroscopy* (certified) and a report of the proceedings of the Academy were sent to Prof. J. Hughes Bennett, of Edinburg, and Carl Rokitanski of Vienna, and letters from both these gentlemen have been received in return. We have only space to publish a portion of the remarks of the first, Prof. Rokitanski agrees with him in almost every particular.

As to whether the treatment caused the disease.—Dr. Beales informs us that Dr. Mott told the family, after the post-mortem examination, that they had not seen any disease that might not have been produced within a week. According to this idea, it could not have been the injection into the trachea which caused the pulmonary disease, as that operation was performed on the 6th of December, without causing any irritation, and he died on the 21st. Besides, the pulmonary lesion existed on the 25th October, as proved by physical signs. The injection, therefore, could not have caused that lesion. Is it not more probable that, a chronic exudation having existed at the apex, and a cavity formed, this latter perforated the pleura, causing the pleural exudation, followed by the emphysema of the cellular tissue. At what moment did this perforation take place? We are told that Mr. Whitney visited Dr. Green on the 14th of December, after breakfast, (the time is not stated;) the doctor passed an instrument

into his throat, and finding some obstruction, he pushed the instrument with some force; he (Mr. W.) felt something give way, immediately experienced *severe* pain about the top of the windpipe, and told the doctor he had "hurt him;" he returned home, informed the family of what had occurred, and was seen by Dr. Beales at 1 P. M. Dr. Green's account of what happened on the 14th is different. According to him, when the sponge reached the glottic opening, the patient partially closed the throat, so that the instrument did not enter the windpipe at all. It was at once removed, no more force having been used than that which is constantly employed every day in operations on the air-passages. The patient, after talking a while with Dr. Foy and myself, and remarking that "the operation hurt him more," or that "he felt it more than usual," left, with the arrangement that he should return the next day and have the tube employed. The patient's account to Dr. Beales is, that at the moment the probang was arrested at the glottis, he felt something give way, and experienced *severe* pain about the top of the windpipe. But is this consistent with the fact that he talked for a while with Drs. Foy and Green, and went home intending to come next day, &c.? Again, was the pain, if felt *at the top* of the windpipe, symptomatic of perforation of the pleura in the chest? It appears to me difficult to answer these questions, more especially when we read the account of the alarming condition of Mr. W. at 1 o'clock, when first seen by Dr. Beales. I am, therefore, inclined to ask, What occurred in the interval between the patient's leaving Dr. Green and his seeing Dr. Beales? This may have been an interval of three or four hours, and of this most important period I can find no account whatever. It is true, Mr. Whitney seems to have had the impression that his severe symptoms commenced when the sponge was arrested, but this is negatived by the evidence of Drs. Green and Foy, who saw no evidence of severe pain even in the throat. When, then, did the pulmonary and pleural perforation take place, which induced the emphysema and subsequent symptoms? With the facts at present before me, I cannot answer this question, but it is clear to my mind that it may have occurred during the hours not accounted for; that it might have been altogether spontaneous, connected only with the progress of the chronic pulmonary lesion; and that it had no relation whatever to the operation performed by Dr. Green.

METEOROLOGICAL ABSTRACT FOR AUGUST, 1859.

BAROMETER.

Maximum, 30.324, - - Minimum, 29.918, - - - Range, 00.406

THERMOMETER ATTACHED.

Mean.

7 A. M., 76.93, - - - 2 P. M., 80.32, - - - 9 P. M., 78.87

THERMOMETER DETACHED.

Mean.

7 A. M., 74.58 - - - 2 P. M., 81.80, - - - 9 P. M., 77.48

Highest Degree.

7 A. M., 80, - - - 2 P. M., 91. - - - - 9 P. M., 82

Lowest Degree.

7 A. M., 69, - - - 2 P. M., 73, - - - - 9 P. M., 72

REGISTER THERMOMETER.

Mean.

Night, 72.87, - - - - - - - - - - - Day, 84.70

DEW POINT.

Mean, - - - - 72.22

WINDS.

Sun Rise—N. 1 days; NE 10; SE 13; SW 6; W 1. Prevailing SE.
4 *P. M.*—NE 6 days; NW 1; S 1; SE 12; SW 9; E 2. Prevailing SE.

WEATHER.

Fair, 11 days—Cloudy, 4 days—Rain, 16 days

QUANTITY OF RAIN.

Inches, - - - 15.21.

JOHN L. DAWSON, M.D.

METEOROLOGICAL ABSTRACT FOR SEPTEMBER, 1859.

BAROMETER.

Maximum, 30.438, - Minimum, 29,934, - - Range, 0.504

THERMOMETER ATTACHED.

Mean.

7 A. M., 75.40, - - 2 P. M., 79.66, - - - 9 P. M., 77.83

THERMOMETER DETACHED.

Mean.

7 A. M., 72.10, - - 2 P. M., 80.66, - - - 9 P. M., 75.86

Highest Degree.

7 A. M., 79, - - - 2 P. M., 86, - - - 9 P. M., 81

Lowest Degree.

7 A. M., 66, - - - 2 P. M., 72, - - - 9 P. M., 70

REGISTER THERMOMETER.

Mean.

Night, 70.56, - - - - - - - - - - - Day, 82.70

DEW POINT.

Mean, - - - 69.33.

WINDS.

Sun Rise—N 1 days; NE 10; NW 4; S 1; SE 3; SW 8; E 2; W 1. Prevailing NE
4 *P. M.*—NE 6 days; NW 2; S 2; SE 8; SW 7; E 1; W. 4. Prevailing SE.

WEATHER.

Fair, 22 days—Cloudy, 1 days—Rain 7 days.

QUANTITY OF RAIN.

Inches, - - - - 1.59.

JOHN L. DAWSON, M.D., *City Registrar.*

RETURN OF DEATHS IN THE CITY OF CHARLESTON
FOR AUGUST, 1859.

WHITES.		BLACKS AND COLORED.	
Accident,	4	Abscess Abdominis,	1
Apoplexy,	2	Anasarca,	1
Brain, Congestion of	2	Apoplexy,	1
Cholera Infantum,	2	Birth, Premature,	1
Convulsions,	3	Bowels, Spasm of	1
Consumption,	5	Bronchitis,	1
Diarrhœa,	2	Cholera Infantum,	3
Dropsy,	1	Convulsions,	4
Drowned,	2	Consumption,	5
Dysentery,	1	Diarrhœa,	2
Erysipelas,	1	Dropsy,	2
Exhausted,	1	Drowned,	1
Fever Bilious,	1	Enteritis,	2
Fever, Continued,	1	Fever, Typhoid	1
Fever, Typhoid	2	Heart Disease,	1
Intemperance,	2	Hernia Strangulated,	1
Old Age,	4	Hip, Disease of	1
Spine, Disease of	1	Lungs, Congestion of	2
Tetanus,	1	Marasmus,	3
Trismus,	1	Metritis Puerperal,	1
Trismus Nascentium,	3	Old Age,	1
Want of Vitality,	1	Paralysis,	2
	—	Teething,	4
Males 30; Females 12. Total,	42	Trismus Nascentium,	8
	—	Whooping Cough,	1
		Worms,	1
		Males, 27; Females, 25. Total,	52

Ages.—Under 1 year,	8		
1 to 5,	3		
10 to 20,	2	Ages.—Under 1 year,	22
20 to 30,	2	1 to 5,	9
30 to 40,	8	5 to 10,	2
40 to 50,	6	10 to 20,	3
50 to 60,	5	20 to 30,	1
60 to 70,	2	30 to 40,	3
70 to 80,	3	40 to 50,	4
80 to 90,	1	50 to 60,	1
90 to 100,	2	60 to 70,	3
	—	70 to 80,	3
Total,	42	80 to 90,	1
	—	90 to100,	0
Natives of Charleston,	19	Total,	52
Natives of United States,	7		
Foreigners,	16	Natives of Charleston,	46
	—	Natives of United States,	6
Total,	42	Total,	52

J. L. DAWSON, M.D., *City Registrar.*

RETURN OF DEATHS IN THE CITY OF CHARLESTON
FOR SEPTEMBER, 1859.

WHITES.		BLACKS AND COLORED.	
Bowels, Ulceration, of	1	Apoplexy,	1
Brain, Congestion of	2	Anasarca,	2
Cholera Infantum,	1	Bowels, Congestion of	1
Consumption,	3	Brain, Congestion of	2
Dysentery,	1	Cancer	1
Enteritis,	1	Cholera Infantum,	2
Effusion of Chest,	1	Convulsions, Puerperal,	1
Fever, Typhoid,	2	Consumption,	6
Fever, Bilious	1	Diarrhœa,	1
Gastritis,	1	Dropsy,	2
Hepatitis,	1	Drowned,	1
Intemperance,	2	Enteritis,	1
Jaundice,	1	Fever, Typhus	2
Marasmus,	2	Fever, Typhoid,	3
Rheumatism,	1	Heart, Disease of	2
Scrofula,	1	Hernia Strangulated,	1
Trismus Nascentium,	1	Leg, Fracture of	1
Teething,	3	Marasmus,	3
Want of Vitality,	1	Old Age,	7
		Spasms,	1
Males, 17; Females, 10; Total,	27	Trismus Nascentium,	5
		Unknown,	1
		Want of Vitality,	1
		Wound of Thigh,	1
		Males, 26; Females, 23; Total,	49
		Ages.—Under 1 year,	8
		1 to 5,	9
		5 to 10,	2
Ages.—Under 1 year,	3	10 to 20,	3
1 to 5,	5	20 to 30,	2
5 to 10,	2	30 to 40,	4
10 to 20,	1	40 to 50,	5
20 to 30,	5	50 to 60,	6
30 to 40,	6	60 to 70,	2
40 to 50,	2	70 to 80,	4
50 to 60,	2	80 to 90,	2
60 to 70,	1	90 to 100,	1
70 to 80,	0	100 and over	1
Total,	27	Total,	49
Natives of Charleston,	15	Natives of Charleston,	43
Natives of United States,	5	Natives of United States,	4
Foreigners,	7	Foreigners,	2
Total,	27	Total,	49

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TO CORRESPONDENTS AND PUBLISHERS.

The following Books and Pamphlets have been received:

Pathological and Practical Observations on Diseases of the Alimentary Canal, Œsophagus, Stomach, Cœcum and Intestines. By S. O. Habershon, MD. London, 8vo. pp. 312, Philadelphia: Blanchard and Lea. 1859. [Through McCarter and Co.]

An Introduction to Practical Pharmacy: Designed as a Text-Book for the Student, and as a Guide for the Physician and Pharmaceutist, with many Formulas and Prescriptions. By Edward Parrish. 8vo. pp. 720. [Ibid.]

A Practical Treatise on the Diagnosis Pathology and Treatment of Diseases of the Heart. By Austin Flint, MD., 8vo. pp. 473. [Ibid]

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By James Paget, F. R. S. 8vo. pp. 700, Philadelphia: Lindsay and Blakiston, 1859. [Through Russell and Jones]

A System of Dental Surgery. By John Tomes, F. R. S. 8vo. pp. 686.

A Practical Treatise on Operative Dentistry. By J. Taft. 8vo. pp. 383.

Physicians Hand-Book of Practice for 1860. New York W. Townsend and Co.

Physicians Visiting List for 1860. Philadelphia, Lindsay and Blakiston.

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